SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

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NOTICE TO SUBSCRIBERS.

We consider it due to those subscribers who have favored us with their subscriptions, previous to the publication of our club rates, that they should have the privileges of the list. They can therefore send us subscriptions for any or all of the publications named at the reduced double rates, less $4, the subscription price of "SCIENCE."

THE Report of the United States Fish Commission* for the year 1878 constitutes a volume of nearly 1,000 pages of interesting matter, and, from the economic interests involved, should command more than a passing attention from those who are desirous of having the natural resources of this country fully developed.

A large portion of the Report, relating to purely scientific work, will be highly appreciated by every naturalist. For instance, the first division of the work including researches into the character of the fishes belonging to the North American fauna, was in charge of Mr. G. Brown Goode, assisted by Dr. T. H. Bean; while it is sufficient to say that the collection and investigation of marine invertebrates was conducted by Professor A. E. Verrill, assisted by Mr. Richard Rathbun, Mr. Sanderson Smith and Mr. Warren Upham, to show the value of the researches in this direction.

Few persons will peruse this Report without feeling an obligation to Professor Spencer F. Baird for the very thorough manner in which he is carrying out the objects of this Commission; for the ground he proposes to cover would appall one of less experience.

The amount of labor involved in carrying out the work of this Commission may be estimated by a brief reference to the programme which Professor Baird has sketched for future guidance:

1st. The preparation of a series of reports upon the various groups of aquatic animals and plants of North America, especially those having relation to the wants or luxuries of mankind, to be afterwards published as monographs, with suitable illustrations.

2d. The distribution of specimens of aquatic animals and plants, not required for the National Museum, to the numerous educational and scientific establishments in the United States.

3d. A complete account of the physical character and conditions of the waters of the United States, as to chemical composition, temperature, etc., with special reference to their availability in nurturing the proper species of food fishes.

4th. A history and description of the various methods, employed in the United States, in the pursuit, capture and utilization of fishes and other aquatic animals.

5th. Statistics of the various branches of the American fisheries from the earliest dates to the present time, so as to show the development of this important industry and its actual condition.

6th. The establishment by the General Government, or in connection with the States, of a thoroughly reliable and exhaustive system of recording fishery statistics for the future.

7th. The bringing together in the National Museum not only of a complete collection of the aquatic animals and plants referred to, but illustrations of all apparatus or devices, used at home or abroad, in the prosecution of the fisheries.

8th. An investigation of the movements and habits of various kinds of fish, to serve as a basis for legislation, either by the General Government or by the States.

9. The arrangement of a code of regulations in respect to close seasons, and other matters of detail respecting the capture of fish.

10th. The stocking of the various waters of the United States with the fish most suited to them, either by artificial propagation or transfer, and the best apparatus and methods for accomplishing this object.

Professor Baird intends to supplement this immense amount of work by collecting and compiling statistics for the proper treatment of international questions connected with the common use, by the United States and the British Provinces, of the waters of the North Atlantic.

The volume before us bears ample proof of the power of Professor Baird and his assistants, to carry out this programme to its fullest extent, and if the work progresses at the present rate, its accomplishment will not be so far in the future as many would suppose.

*United States Commission of Fish and Fisheries, Part VI. Report of the Commissioner for 1878.
A. Inquiry into the Decrease of Food-Fishes.
B. The Propagation of Food-Fishes in the Waters of the United States. Washington Government Printing Office, 1880.

We do not propose in this notice to epitomize the report; we prefer to do more justice to the subject by presenting from time to time brief abstracts of the paper, some of which are very elaborate, occupying 160 pages of closely printed matter, and 90 illustrations.

That part of the report describing the success of the commission in propagating salmon has been anticipated by the public press, but many of the details now given are new and of great interest. Many persons in the East will be astonished at the large scale of the salmon fishery in the Western rivers, where seven to nine thousand fish are sometimes taken in one day. From one station (the St. Cloud river), fourteen millions of eggs of salmon were secured and embryonized—sufficient to keep up the supply being returned to the river, the remainder were sent East; 7,250,000 arrived in Chicago between the 3rd and 7th of October. The report states that, after supplying the home demand, 500,000 were presented to Canada, 100,000 to England, 100,000 to France, 100,000 to Holland, 250,000 to Germany and 200,000 to New Zealand.

In regard to shipments to the last named country, it is satisfactory to be able to state, that they not only arrived in perfect condition, but that by the latest advices the young fish were seen in every direction, promising to be the ancestors of a numerous progeny.

Reference is made to Professor W. O. Atwater's investigations upon the food qualities of various species of fishes, the chief facts relating to which we were able to present in an abstracted form, to the readers of "SCIENCE," a few weeks since.

Various attempts have been made to introduce live specimens of the English Sole, one of the most delicious and prolific of British fishes. The last attempt by Mr. Fred. Mather, whose skill in fish culture is acknowledged in the report, was unfortunately like the rest—a failure. Mr. Mather gives a very reasonable explanation of his want of success, and it must be admitted that he was not supplied with the necessary conveniences. During 1880, Captain Mortimer was more successful, and succeeded in placing living specimens of the Sole (*Solea vulgaris*) in New York harbor. Captain Mortimer explained to us that his apparatus consisted of a tank having a fixed cover, to which were attached two globes, the constant rolling of the vessel causing the water of the tank to pass to the globes and return, thus keeping up a constant aeration for the fish, which naturally remained at the bottom.

We reluctantly close our notice of this most valuable and interesting Report feeling that our task has been but half fulfilled. We shall, however, again take up the subject in greater detail, and present our readers with many facts of much scientific interest.

THE AMERICAN CHEMICAL SOCIETY.*

The January meeting of the above Society was held in their rooms, Monday evening, January 3, 1881, Prof. C. F. Chandler in the President's chair. The nominations of Messrs. James F. Slade, Theodore M. Hopke, A. F. Hoppick as regular, and of Mr. E. K. Dunham as associate members were made. The resignations of Prof. Ira Remsen, Prof. S. P. Sadtler and Mr. L. W. Drew, read and accepted. A motion for the reduction of the annual dues from $10 to $5 was favorably considered, and the day of meeting was changed from Thursday to Monday, so that in the future, meetings will be held on the first and third Mondays of each month, instead of on the corresponding Thursdays. There being no papers before the society, the meeting was adjourned. We add herewith a list of the officers chosen at the December meeting for the present year: President, Prof. C. F. Chandler; Vice-Presidents, A. R. Leeds, G. A. Koenig, E. R. Squibb, Charles A. Goessmann, Henry Morten, Ira Remsen; Corresponding Secretary, P. Casamajor; Recording Secretary, Albert H. Gallatin; Treasurer, W. H. Nichols; Librarian, E. Waller; Curators, W. Rupp, A. J. Rossi, A. A. Fesquet.

ON A THERMO-MAGNETIC THERMOSCOPE.

By SIR WILLIAM THOMSON.

This thermoscope is founded on the change produced in the magnetic moment of a steel magnet by change of temperature. Several different forms suggest themselves. The one which seems best adapted to give good results is to be made as follows:

1. Prepare an approximately astatic system of two thin hardened steel wires, $r\ b, r^1\ b^1$, each one centimetre long, one of them, $r\ b$, hung by a single silk fibre, and the other hung bifilarly from it by fibres about three centimetres long, so attached that the projections of the two on a horizontal plane shall be inclined at an angle of about .01 of a radian (or .57°) to one another.

2. Hang a very small, light mirror, bifilarly from the lower of the two wires.

3. Magnetize the two wires to very exactly equal magnetic moments in the dissimilar directions. This is easily done by a few successive trials, to make them rest as nearly as possible perpendicular to the magnetic meridian.

4. Take two pieces of equal and similar straight steel wire, well hardened, each two centimetres long, and about .04 centimetres diameter. Magnetize them equally and similarly, and mount them on a suitable frame to fulfil conditions.

5 and 6. Call them R B and $R^1\ B^1$, B and B^1 denoting the ends containing true north polarity (ordinarily marked B), and R R^1 true south (ordinarily marked red). The small letters, r, b, r^1, b^1, mark on the same plan the polarites of $r\ b$ and $r^1\ b^1$.

3. The magnets R B, $R^1\ B^1$, are to be relatively fixed in line on their frame with similar poles next one another, at a distance of about two centimetres asunder, as thus, R B . . . $B^1\ R^1$, with B B^1 = two centimetres.

6. This frame is to be mounted on a geometrical slide upon the case, within which the astatic pair, $r\ b$, $r^1\ b^1$, is hung in such a manner that the line of R B, B R bisects $r\ b$, approximately at right angles, and that R B B R may be moved by a micrometer screw through about a millimetre on each side of its central position, the line of motion being the line of R B, $B^1\ R^1$, and the "central position" being that in which B and B^1 are equi-distant from the centre of $r\ b$.

7. A lamp and scale, with proper focussing lens if the mirror is not concave, are applied to show and measure small deflections as in my mirror galvanometres and electrometer.

* Communicated by M. Benjamin, Ph. B.

8. Place the instrument with the needles approximately perpendicular to the magnetic meridian, turning it so as to bring b and b^1 to the south of the vertical plane bisecting the small angle between the projections of $r\ b$, $r^1\ b^1$ and r, and r^1 to the north side of it.

9. By aid of the micrometer screw bring the luminous image to its middle position on the scale.

10. Cause R B, B¹ R¹ to have different temperatures. The luminous image is seen to move in such a direction as is due to r approaching the cooler, and receding from the warmer of the two deflectors B R, B¹ R¹.—*Proceedings Royal Society, Edinburgh.*

[*Continued from page* 270.]

THE UNITY OF NATURE.

BY THE DUKE OF ARGYLL.

IV.

ON THE LIMITS OF HUMAN KNOWLEDGE CONSIDERED WITH REFERENCE TO THE UNITY OF NATURE.

And yet, although it is to Nature in this highest and widest sense that we belong—although it is out of this fountain that we have come, and it is out of its fullness that we have received all that we have and are, men have doubted, and will doubt again, whether we can be sure of anything concerning it.

If this terrible misgiving had affected individual minds alone in moments of weariness and despair, there would have been little to say about it. Such moments may come to all of us, and the distrust which they leave behind them may be the sorest of human trials. It is no unusual result of abortive yet natural effort and of innate yet baffled curiosity. But this doubt, which is really nothing more than a morbid effect of weakness and fatigue, has been embraced as a doctrine and systematized into a philosophy. Nor can it be denied that there are some partial aspects of our knowledge in which its very elements seem to dissolve and disappear under the power of self-analysis, so that the sum of it is reduced to little more than a consciousness of ignorance. All that we know of Matter is so different from all that we are conscious of in Mind, that the relations between the two are really incomprehensible and inconceivable to us. Hence this relation constitutes a region of darkness in which it is easy to lose ourselves in an abyss of utter skepticism. What proof have we—it has been often asked—that the mental impressions we derive from objects are in any way like the truth? We know only the phenomena, not the reality of things. We are conversant with things as they appear, not with things as they are "in themselves." What proof have we that these phenomena give us any real knowledge of the truth? How, indeed, is it possible that knowledge so "relative" and so "conditioned"—relative to a mind so limited, and conditioned by senses which tell us of nothing but sensations—how can such knowledge be accepted as substantial? Is it not plain that our conceptions of Creation and of a Creator are all mere "anthropomorphism?" Is it not our own shadow that we are always chasing? Is it not a mere bigger image of ourselves to which we are always bowing down?

It is upon suggestions such as these that the Agnostic philosophy, or the philosophy of Nescience, is founded—the doctrine that, concerning all the highest problems which it both interests and concerns us most to know, we never can have any knowledge or any rational and assured belief.

It may be well to come to the consideration of this doctrine along those avenues of approach which start from the conception we have now gained of the unity of Nature.

Nothing, certainly, in the human mind is more wonderful than this—that it is conscious of its own limitations. Such consciousness would be impossible if these limitations were in their nature absolute. The bars which we feel so much, and against which we so often beat in vain, are bars which could not be felt at all unless there were something in us which seeks a wider scope. It is as if these bars were a limit of opportunity rather than a boundary of power. No absolute limitation of mental faculty ever is, or ever could be, felt by the creatures whom it affects. Of this we have abundant evidence in the lower animals, and in those lower faculties of our own nature which are of like kind to theirs. All their powers and many of our own are exerted without any sense of limitation, and this because of the very fact that the limitation of them is absolute and complete. In their own nature they admit of no larger use. The field of effort and of attainable enjoyment is, as regards them, co-extensive with the whole field in view. Nothing is seen or felt by them which may not be possessed. In such possession all exertion ends and all desire is satisfied. This is the law of every faculty subject to a limit which is absolute. In physics, the existence of any pressure is the index of a potential energy which, though it may be doing no work, is yet always capable of doing it. And so in the intellectual world, the sense of pressure and confinement is the index of powers which under other conditions are capable of doing what they cannot do at present. It is in these conditions that the barrier consists, and at least to a large extent they are external. What we feel, in short, is less an incapacity than a restraint.

So much undoubtedly is to be said as to the nature of those limitations on our mental powers of which we are conscious. And the considerations thus presented to us are of immense importance in qualifying the conclusions to be drawn from the facts of consciousness. They do not justify, although they may account for, any feeling of despair as to the ultimate accessibility of that knowledge which we so much desire. On the contrary, they suggest the idea that there is within us a Reserve of Power to some unknown and indefinite extent. It is as if we could understand indefinitely more than we can discover, if only some higher Intelligence would explain it to us.

But if it is of importance to take note of this Reserve of Power of which we are conscious in ourselves, it is at least of equal importance to estimate aright the conceptions to which we can and do attain without drawing upon this reserve at all. Not only are the bars confining us bars which we can conceive removed, but they are bars which in certain directions offer no impediment at all to a boundless range of vision. Perhaps there is no subject on which the fallacies of philosophic phraseology have led to greater errors. "That the Finite cannot comprehend the Infinite," is a proposition constantly propounded as an undoubted and all-comprehensive truth. Such truth as does belong to it seems to come from the domain of Physics, in which it represents the axiom that a part cannot be equal to the whole. From this, in the domain of Mind, it comes to represent the truth, equally undeniable, that we cannot know all that Infinity contains. But the meaning into which it is liable to pass when applied to Mind is that Man cannot conceive Infinity. And never was any proposition so commonly accepted which, in this sense, is so absolutely devoid of all foundation. Not only is Infinity conceivable by us, but it is inseparable from conceptions which are of all others the most familiar. Both the great conceptions of Space and Time are, in their very nature, infinite. We cannot conceive of either of these as subject to limitation. We cannot conceive of a moment after which there shall be no more Time, nor of a boundary beyond which there is no more Space. This means that we cannot but think of Space as infinite, and of Time as everlasting.

If these two conceptions stood alone they would be enough, for in regard to them the only incapacity under which we labor is the incapacity to conceive the Finite. For all the divisions of Space and Time with which we are so familar,—our days and months and years, and our various units of distance,—we can only think of as bits and fragments of a whole which is illimitable. But although these great conceptions of Space and Time are possibly the only conceptions to which the idea of infinity attaches as an absolute necessity of Thought, they are by no means the only conceptions to which the same idea can be attached, and probably ought to be so. The conception of Matter is one, and the conception of Force is another, to which we do not perhaps attach, as of necessity, the idea of indestructibility, or the idea of eternal existence and of infinite extension. But it is remarkable that in exact proportion as science advances, we are coming to understand that both of

these are conceptions to which the idea of infinity not only may be, but ought to be attached. That is to say, that the eternal existence of Matter and the eternal duration of Force are not only conceivable but true. Nay, it may be our ignorance alone, that makes us think we can conceive the contrary. It is possible to conceive of Space being utterly devoid of Matter, only perhaps because we are accustomed to see and to think of spaces which are indeed empty of visible substances. We can expel also the invisible substances or gases of the atmosphere, and we can speak and think of the result as a vacuum. But we know now that when air and all other terrestrial gases are gone the luminiferous medium remains; and so far as we have means of knowing, this medium is ubiquitous and omnipresent in the whole universe of Space. In like manner we are accustomed to see solid matter so dissipated as to be invisible, intangible, and wholly imperceptible; and therefore we think we can imagine matter to be really destructible. But the more we know of it the more certain we become that it cannot be destroyed, and can only be redistributed. In like manner, in regard to Force, we are accustomed to see Matter in what is called statical equilibrium—that is to say, at rest; and so perhaps, we think, we can conceive the cessation or extinction of Force. But here again the progress of research is tending more and more to attach irrevocably the idea of indestructibility—that is, of eternal existence—to that which we know as Force. The truth is, that this conception is really implicitly involved in the conception of the indestructibility of Matter. For all that we know of Matter is inseparably connected with the forces which it exerts, or which it is capable of exerting, or which are being exerted in it. The force of gravitation seems to be all-pervading, to be either an inherent power or property in every kind, or almost every kind of Matter, or else to be the result of some kind of energy which is universal and unquenchable. All bodies, however passive and inert they may seem to be under certain conditions, yet indicate by their very existence the power of those molecular forces to which the cohesion of their atoms is due. The fact is now familiar to us that the most perfect stillness and apparent rest in many forms of Matter is but the result of a balance or equilibrium maintained between forces of the most tremendous energy, which are ready to burst forth at a moment's notice, when the conditions are changed under which that balance is maintained. And this principle, which has become familiar in the case of what are called explosive substances, because of the ease and the certainty with which the balanced forces can be liberated, is a principal which really prevails in the composition of all material substances whatever; the only difference being that the energies by which their molecules are held together are so held under conditions which are more stable—conditions which it is much more more difficult to change—and conditions, therefore, which conceal from us the universal prevalence and power of Force in the constitution of the material universe. It is, therefore, distinctly the tendency of science more and more to impress us with the idea of the unlimited duration and indestructible nature both of Matter and of the energies which work in and upon it.

One of the scientific forms under which this idea is expressed is the Conservation of Energy. It affirms that though we often see moving bodies stopped in their course, and the energy with which they move apparently extinguished, no such extinction is really effected. It affirms that this energy is merely transformed into other kinds of motion, which may or may not be visible, but which, whether visible or not, do always really survive the motion which has been arrested. It affirms, in short, that Energy, like Matter, cannot be destroyed or lessened in quantity, but can only be redistributed.

As, however, the whole existing Order of Nature depends on very special distributions and concentrations of Force, this doctrine affords no ground for presuming on the permanence, or even on the prolonged continuance, of that order. Quite the contrary; for another general conception has been attained from science which at first sight appears to be a contradiction of the doctrine of "Conservation of Energy"—namely, the "Dissipation of Energy." This doctrine, however, does not affirm that Energy can be dissipated in the sense of being wholly lost or finally extinguished. It only affirms that all the existing concentrations of force are being gradually exhausted, and that the forces concerned in them are being diffused (generally in the form of Heat) more and more equally over the infinitudes of Matter and of Space.

Closely connected with, if indeed it be not a necessary part and consequence of, these conceptions of the infinity of Space and time, of Matter and of Force, is the more general concept of Causation.

It is impossibe to conceive of anything happening without a cause. Even if we could conceive the utter destruction or annihilation of any particular force or form of force, we cannot conceive of this very destruction happening except as the effect of some cause. All attempts to reduce this idea of causation to other and lower terms have been worse than futile. They have uniformly left out something which is of the very essence of the idea. The notion of "uniform antecedence" is not equivalent. "Necessary antecedence" is more near the mark. These words do indeed indicate the essential element in the idea with tolerable clearness. But like all other simple fundamental conceptions, the idea of Causation defies analysis. As, however, we cannot dissociate the idea of Causation from the idea of Force or energy, it may perhaps be said that the indestructibility or eternal duration of Force is a physical doctrine which gives strength and substance to the metaphysical concept of causation. Science may discover, and indeed has already discovered, that, as regards our application of the idea of cause, and of the correlative idea of effect, to particular cases of sequence, there is often some apparent confusion arising from the fact that the relative positions of cause and effect may be interchangeable, so that A, which at one moment appears as the cause of B, becomes at another moment the consequence of B, and not its cause. Thus Heat is very often the cause of visible motion, and visible motion is again the cause of Heat. And so of the whole cycle of physical forces, which Sir W. Grove and others have proved to be "correlated"—that is, to be so intimately related that each may in turn produce or pass into all the others. But this does not really obscure or cast any doubt upon the truth of our idea of causation. On the contrary, that idea is confirmed in receiving a new interpretation, and in the disclosure of physical facts involving the same conception. The necessity of the connection between an effect and its cause receives an unexpected confirmation when it comes to be regarded as simply the necessary passing of an energy which is universal and indestructible from one form of action into another. Heat becomes the cause of Light because it is the same energy working in a special medium. Conversely Light becomes the cause of Heat, because again the same energy passes into another medium and there produces a different effect. And so all the so-called "correlated forces" may be interchangeably the cause or the consequence of each other, according to the order of time in which the changes of form are seen. This, however, does not confound, but only illustrates the ineradicable conviction that for all such changes there must be a cause. It may be perfectly true that all these correlated forces can be ideally reduced to different "forms of motion;" but motion itself is inconceivable except as existing in Matter, and as the result of some moving force. Every difference of direction in motion or of form in Matter implies a change, and we can conceive no change without a cause—that is to say, apart from the operation of some condition without which that change would not have been.

The same ultimate conceptions, and no other, appear to constitute all the truth that is to be found in a favorite doctrine among the cultivators of physical science— the so-called "Law of Continuity." This phrase is indeed often used with such looseness of meaning that it is extremely difficult to understand the primary signification attached to it. One common definition, or rather one common illustration, of this law is said to be that Nature does nothing suddenly—nothing "per saltum." Of course this can only be accepted under some metaphorical or transcendental meaning. In Nature there is such a thing as a flash of lightning, and this is generally recognized as sufficiently sudden. A great many other exertions of electric force are of similar rapidity. The action of chemical affinity is always rapid. and very often even instantaneous. Yet these are among the most common and the most powerful factors in the me-

chanism of Nature. They have the most intimate connection with the phenomena of Life, and in these the profoundest changes are often determined in moments of time. For many purposes to which this so-called "Law of Continuity" is often applied in argument no idler dogma was ever invented in the schools. There is a common superstition that this so-called law negatives the possibility, for example, of the sudden appearance of new forms of Life. What it does negative, however, is not appearances which are sudden, but only appearances which have been unprepared. Innumerable things may come to be,—in a moment—in the twinkling of an eye. But nothing can come to be without a long, even if it be a secret, history. The "Law of Continuity" is, therefore, a phrase of ambiguous meaning; but at the bottom of it there lies the true and invincible conviction that for every change, however sudden—for every "leap," however wide—there has always been a long chain of predetermining causes, and that even the most tremendous bursts of energy and the most sudden exhibitions of force have all been slowly and silently prepared. In this sense the Law of Continuity is nothing but the idea of Causation. It is founded on the necesary duration which we cannot but attribute to the existence of Force, and this appears to be the only truth which the Law of Continuity represents.

When now we consider the place in the whole system of our knowledge which is occupied by these great fundamental conceptions of Time and Space, and of Matter and of Force, and when we consider that we cannot even think of any one of these realities as capable of coming to an end, we may well be assured that, whatever may be the limits of the human mind, they certainly do not prevent us from apprehending infinity. On the contrary, it would rather appear that this apprehension is the invariable and necessary result of every investigation of nature.

It is indeed of the highest importance to observe that some of these conceptions, especially the indestructibility of Matter and of Force, belong to the domain of science. That is to say, the systematic examination of natural phenomena has given them distinctness and a consistency which they never possessed before. As now accepted and defined, they are the result of direct experiment. And yet, strictly speaking, all that experiment can do is to prove that in all cases in which either Matter or Force seems to be destroyed, no such destruction has taken place. Here then we have a very limited and imperfect amount of "experience" giving rise to an infinite conception. But it is another of the suggestions of the Agnostic philosophy that this can never be a legitimate result. Nevertheless, as a matter of fact, these conceptions have been reached. They are now universally accepted and taught as truths lying at the foundation of every branch of natural science—at once the beginning and the end of every physical investigation. They are not what are ordinarily called "laws." They stand on much higher ground. They stand behind and before every law, whether that word be taken to mean simply an observed order of facts, or some particular force to which that order is due, or some combinations of force for the discharge of function, or some abstract definition of observed phenomena such as the "laws of motion." All these, though they may be "invariable" so far as we can see, carry with them no character of universal or necessary truth—no conviction that they are and must be true in all places and for all time. There is no existing order—no present combinations of Matter or of Force—which we cannot conceive coming to an end. But when that end is come we cannot conceive but that something must remain,—if it be nothing else than that by which the ending was brought about or, as it were, the raw materials of the creation which, has passed away. That this conception, when once suggested and clearly apprehended, cannot be eradicated, is one of the most indisputable facts of instructed consciousness. That no possible amount of mere external observation or experiment can cover the infinitude of the conclusion is also unquestionably true. But if "experience" is to be upheld as in any sense the ground and basis of all our knowledge, it must be understood as embracing the most important of all kinds of experience in the study of Nature—the experience we have of the laws of Mind. It is one of the most certain of those laws, that in proportion as the powers of the understanding are well developed, and are prepared by previous training for the interpretation of natural facts, there is no relation whatever between the time occupied in the observation of phenomena and the breadth or sweep of the conclusions which may be arrived at from them. A single glance, lasting not above a moment of time, may awaken the recognition of truths as wide as the universe and as everlasting as Time itself. Nay, it has often happened in the history of science that such recognitions of general truths have been reached by no other kind of observation than that of the mind becoming conscious of its own innate perceptions. Conceptions of this nature have perpetually gone before experiment—have suggested it, guided it—and have received nothing more than corroboration from it. I do not say that these conceptions have been reached without any process. But the process has been to a large extent as unconscious as that by which we see the light. I do not say they have been reached without "experience," even in that narrow sense in which it means the observation of external things. But the experience has been nothing more than the act of living in the world, and of breathing in it, and of looking round upon it. These conceptions have come to Man because he is a Being in harmony with surrounding Nature. The human mind has opened to them as a bud opens to the sun and air. So true is this, that when reasons have been given for the conclusions thus arrived at—these reasons have often been quite erroneous. Nothing in the history of philosophy is more curious than the close correspondence between many ideas enunciated by the ancients as the result of the speculation, and some, at least, of the ideas now prevalent as the result of science. It is true that the ancients expressed them vaguely, associated them with other conceptions which are wide of the truth, and quoted in support of them illustrations which are often childish. Nevertheless the fact remains that they had attained to some central truths, however obscured the perception may have been by ignorance of the more precise and accurate analogies by which they can be best explained, and which only the process of observation has revealed. "They had in some way grasped," says Mr. Balfour Stewart,* "the idea of the essential unrest and energy of things. They had also the idea of small particles or atoms; and finally of a medium of some sort, so that they were not wholly ignorant of the most profound and deeply seated of the principles of the material universe." There is but one explanation of this, but it is all-sufficient. It is that the mind of Man is a part, and one at least of the highest parts, of the system of the universe—the result of mechanism most suited to the purpose of catching and translating into thought the light of truth as embodied in surrounding Nature.

We have seen that the foundations of all conscious reasoning are to be found in certain propositions which we call self-evident. That is to say, in propositions the truth of which is intuitively perceived. We have seen, too, as a general law affecting all manifestations of Life or Mind, even in its very lowest forms, that instinctive or intuitional perceptions are the guide and index of other and larger truths which lie entirely beyond the range of the perception or intuition which is immediately concerned. This law holds good quite as much of the higher intuitions which are peculiar to Man as of the mere intuitions of sensation which are common to him and to the animals beneath him. The lowest savage does many things by mere instinct which contain implicitly truths of a very abstract nature—truths of which, as such, he has not the remotest conception, and which in the present undeveloped condition of his faculties it would be impossible to explain to him. Thus, when he goes into the forest to cut a branch fit for being made into a bow, or when he goes to the marsh to cut a reed fit for being made into an arrow, and when in doing so he cuts them off the proper length by measuring them by the bows and arrows which he already has, in this simple operation he is acting on the abstract and most fruitful truth that "things equal to the same thing are equal to one another." This is one of the axioms which lie at the basis of all mathematical demonstration. But as a general, universal, and necessary truth the savage knows nothing of it—as little as he knows of the wonderful consequences to which it will some day lead his children or descendants. So in like

* "Conservation of Energy," p. 135.

manner when the savage designs, as he often does, most ingenious traps for the capture of his prey, and so baits them as to attract the animals he desires to catch, he is counting first on the constancy and uniformity of physical causation, and, secondly, on the profoundly different action of the motives which determine the conduct of creatures having Life and Will. But of neither of these as general truths does he know anything, and of one of them at least, not even the greatest philosophers have reached the full depth of meaning. Nevertheless, it would be a great error to suppose that the savage, because he has no conception of the general truth involved in his conduct, has been guided in that conduct by anything in the nature of chance or accident. His intuitions have been right, and have involved so much perception of truth as is necessary to carry him along the little way he requires to travel, because the mind in which those intuitions lie is a product and a part of Nature—a product and part of that great system of things which is held together by laws intelligible to Mind—laws which the human mind has been constructed to feel even when it cannot clearly see. Moreover, when these laws come to be clearly seen, they are seen only because the mind has organs adjusted to the perception of them, and because it finds in its own mechanism corresponding sequences of thought.

It was the work of a great German metaphysician towards the close of the last century to discriminate and define more systematically than had been done before some at least of those higher elements of thought which, over and above the mere perception of external things, the mind thus contributes out of its own structure to the fabric of knowledge. In doing this he did immortal service—proving that when men talked of "experience" being the source of knowledge, they forgot that the whole process of experience presupposes the action of innate laws of thought, without which experience can neither gather its facts nor reach their interpretation. "Experience," as Kant most truly said, is nothing but a "synthesis of intuitions"—a building up or putting together of conceptions which the access of external Nature finds ready to be awakened in the mind. The whole of this process is determined by the mind's own laws—a process in which even observation of outward fact must take its place according to principles of arrangement in which alone all explanations of them consists, and out of which any understanding them is impossible.

And yet this great fact of a large part of our knowledge—and that the most important part—coming to us out of the very furniture and constitution of the mind itself, has been so expressed and presented in the language of philosophy as rather to undermine than to establish our confidence in the certainty of knowledge. For if the mind is so spoken of and represented as to suggest the idea of something apart from the general system of Nature, and if its laws of thought are looked upon as "forms" or molds into which, by some artificial arrangement or by some mechanical necessity, everything from outside must be squeezed and made to fit—then it will naturally occur to us to doubt whether conceptions cut out and manufactured under such conditions can be any trustworthy representation of the truth. Such, unfortunately, has been the mode of representation adopted by many philosophers—and such accordingly has been the result of their teaching. This is the great source of error in every form of the Idealistic philosophy, but it is a source of error which can be perfectly eliminated, leaving untouched and undoubted the large body of truths which has made that philosophy attractive to so many powerful minds. We have only to take care that in expressing those truths we do not use metaphors which are misleading. We have only to remember that we must regard the mind and the laws of its operation in the light of that most assured truth—the Unity of Nature. The mind has no "molds" which have not themselves been molded on the realities of the universe—no "forms" which it did not receive as a part and a consequence of a unity with the rest of Nature. Its conceptions are not manufactured; they are developed. They are not made; they simply grow. The order of the laws of thought under which it renders intelligible to itself all the phenomena of the universe, is not an order which it invents, but an order which it simply feels and sees. And this "vision and faculty divine" is a necessary consequence of its congenital relations with the whole system of Nature—from being bone of its bone—flesh of its flesh—from breathing its atmosphere, from living in its light, and from having with it a thousand points of contact visible and invisible, more than we can number or understand.

And yet so subtle are the suggestions of the human spirit in disparagement of its own powers—so near and ever present to us is that region which belongs to the unsatisfied Reserve of Power—that the very fact of our knowledge arising out of our organic relations with the rest of Nature has been seized upon as only casting new discredit on all that we seem to know. Because all our knowledge arises out of these relations, therefore, it is said, all our knowledge of things must be itself relative; and relative knowledge is not knowledge of "things in themselves." Such is the argument of metaphysicians—an argument repeated with singular unanimity by philosophers of almost every school of thought. By some it has been made the basis of religious proof. By some it has been made the basis of a reasoned skepticism. By some it has been used simply to foil attacks upon belief. The real truth is that it is an argument useless for any purpose whatever, because it is not itself true. The distinction between knowledge of things in their relations, and knowledge of things "in themselves," is a distinction without a meaning. In metaphysics the assertion that we can never attain to any knowledge of things in themselves does not mean simply that we know things only in a few relations out of many. It does not mean even that there may be and probably are a great many relations which we have not faculties enabling us to conceive. All this is quite true, and a most important truth. But the metaphysical distinction is quite different. It affirms that if we knew things in every one of the relations that affect them, we should still be no nearer than before to a knowledge of "things themselves." "It is proper to observe," says Sir W. Hamilton, "that had we faculties equal in number to all the possible modes of existence, whether of mind or matter, still would our knowledge of mind or matter be only relative. If material existence could exhibit ten thousand phenomena,—if we possessed ten thousand senses to apprehend these ten thousand phenomena of material existence, of existence absolutely and in itself we should then be as ignorant as we are at present."* The conception here that there is something to be known about things in which they are not presented as in any relation to anything else. It affirms that there are certain ultimate entities in Nature to which all phenomena are due, and yet which can be thought of as having no relation to these phenomena, or to ourselves, or to any other existence whatever. Now as the very idea of knowledge consists in the perception of relations, this affirmation is, in the purest sense of the word, nonsense—that is to say, it is a series of words which have either no meaning at all or a meaning which is self-contradictory. It belongs to the class of propositions which throw just discredit on metaphysics—mere verbal propositions, pretending to deal with conceptions which are no conceptions at all, but empty sounds. The "unconditioned," we are told, "is unthinkable;" but words which are unthinkable had better be also unspeakable, or at least unspoken. It is altogether untrue that we are compelled to believe in the existence of anything which is "unconditioned"—in Matter with no qualities—in Minds with no character—in a God with no attributes. Even the metaphysicians who dwell on this distinction between the Relative and Unconditioned admit that it is one to which no idea can be attached. Yet, in spite of this admission, they proceed to found many inferences upon it, as if it had an intelligible meaning. Those who have not been accustomed to metaphysical literature could hardly believe the flagrant unreason which is common on this subject. It cannot be better illustrated than by quoting the words in which this favorite doctrine is expressed by Sir William Hamilton. Speaking of our knowledge of Matter he says: "It is a name for something known—for that which appears to us under the forms of extension, solidity, divisibility, figure, motion, roughness, smoothness, color, heat, cold," etc. "But," he goes on to say, "as these phenomena appear only in conjunction, we are compelled by the constitution of our nature to think them conjoined in and by something; and as they

* "Lectures," vol. i. p. 145.

are phenomena, we cannot think them the phenomena of nothing, but must regard them as the properties or qualities of something that is extended, figured, etc. But this something, absolutely and in itself—*i. e.*, considered apart from its phenomena—is to us as Zero. It is only in its qualities, only in its effects, in its relative or phenomenal existence, that it is cognizable or conceivable; and it is only by a law of thought which compels us to think something absolute and unknown, as the basis or condition of the relative and known, that this something obtains a kind of incomprehensible reality to us." The argument here is that because phenomena are and must be the "properties or qualities of something else," therefore we are "compelled to think" of that something as having an existence separable from any relation to its own qualities and properties, and that this something acquires from this reasoning a "kind of incomprehensible reality!" There is no such law of thought. There is no such necessity of thinking nonsense as is here alleged. All that we are compelled to think is that the ultimate constitution of Matter, and the ultimate source of its relations to our own organism, are unknown, and are probably inaccessible to us. But this is a very different conception from that which affirms that if we did know or could know these ultimate truths, we should find in them anything standing absolutely alone and unrelated to other existences in the Universe.

It is, however, so important that we should define to ourselves as clearly as we can the nature of the limitations which affect our knowledge, and the real inferences which are to be derived from the consciousness we have of them, that it may be well to examine these dicta of metaphysicians in the light of specific instances. It becomes all the more important to do so, when we observe that the language in which these dicta are expressed generally implies that knowledge which is "only relative" is less genuine or less absolutely true than some other kind of knowledge which is not explained, except that it must be knowledge of that which has no relation to the mind.

There is a sense (and it is the only sense in which the words have any meaning) in which we are all accustomed to say that we know a thing "in itself," when we have found out, for example, its origin, or its structure, or its chemical composition as distinguished from its more superficial aspects. If a new substance were offered to us as food, and if we examined its appearance to the eye, and felt its consistency to the touch, and smelt its odor, and finally tasted it, we should then know as much about it as these various senses could tell us. Other senses, or other forms of sensation, might soon add their own several contributions to our knowledge, and we might discover that this substance had deleterious effects upon the human organism. This would be knowing, perhaps, by far the most important things that are to be known about it. But we should certainly like to know more, and we should probably consider that we had found out what it was "in itself," when we had discovered farther, for example, that it was the fruit of a tree. Chemistry might next inform us of the analysis of the fruit, and might exhibit some alkaloid to which its peculiar properties and its peculiar effects upon the body are due. This, again, we should certainly consider as knowing what it is "in itself." But other questions respecting it would remain behind. How the tree can extract this alkaloid from the inorganic elements of the soil, and how, when so extracted, it should have such and such peculiar effects upon the animal body; these, and similar questions, we may ask, and probably we shall ask in vain. But there is nothing in the inaccessibility of this knowledge to suggest that we are absolutely incapable of understanding the answer if it were explained to us. On the contrary, the disposition we have to put such questions raises a strong presumption that the answer would be one capable of that assimilation by our intellectual nature in which all understanding of anything consists. There is nothing in the series of phenomena which this substance has exhibited to us—nothing in the question which they raise which can even suggest the idea that all these relations which we have traced, or any others which may remain behind, are the result of something which can be thought of or conceived as neither a cause nor a consequence—but solitary and unrelated. On the contrary, all that remains unexplained is the nature and cause of its relations—its relations on the one hand to the elements out of which vegetable vitality has combined it, and its relations on the other hand to the still higher vitality which it threatens to destroy. Its place in the unity of Nature is the ultimate object of our search, and this unity is essentially a unity of relations, and of nothing else. That unity everywhere proclaims the truth that there is nothing in the wide universe which is unrelated to the rest.

Let us take another example. Until modern science had established its methods of physical investigation, Light and Sound were known as sensations only. That is to say, they were known in terms of the mental impressions which they immediately produce upon us, and in no other terms whatever. There was no proof that in these sensations we had any knowledge "in themselves" of the external agencies which produce them. But now all this is changed. Science has discovered what these two agencies are "in themselves;"—that is to say, it has defined them under aspects which are totally distinct from seeing or hearing, and is able to describe them in terms addressed to wholly different faculties of conception. Both Light and Sound are in the nature of undulatory movements in elastic media—to which undulations our organs of sight and hearing are respectively adjusted or "attuned." In these organs, by virtue of that adjustment or attuning, these same undulations are "translated" into the sensations which we know. It thus appears that the facts as described to us in this language of sensation are the true equivalent of the facts as described in the very different language of intellectual analysis. The eye is now understood to be an apparatus for enabling the mind instantaneously to appreciate differences of motion which are of almost inconceivable minuteness. The pleasure we derive from the harmonies of color and of sound, although mere sensations, do correctly represent the movement of undulations in a definite order; whilst those other sensations which we know as discords represent the actual clashing and disorder of interfering waves. In breathing the healthy air of physical discoveries such as these, although the limitations of our knowledge continually haunt us, we gain nevertheless a triumphant sense of its certainty and of its truth. Not only are the mental impressions, which our organs have been so constructed as to convey, a true interpretation of external facts, that the conclusions we draw as to their origin and their source, and as to the guarantee we have for the accuracy of our conceptions, are placed on the firmest of all foundations. The mirror into which we look is a true mirror, reflecting accurately and with infinite fineness the realities of Nature. And this great lesson is being repeated in every new discovery, and in every new application of an old one. Every reduction of phenomena to ascertained measures of force; every application of mathematical proof to theoretical conceptions; every detection of identical operations in diverse departments of Nature; every subjection of material agencies to the service of mankind; every confirmation of knowledge acquired through one sense by the evidence of another—every one of these operations adds to the verifications of science, confirms our reasonable trust in the faculties we possess and assures us that the knowledge we acquire by the careful use of these is a real and substantial knowledge of the truth.

If now we examine the kind of knowledge respecting Light and Sound which recent discoveries have revealed to us, as compared with the knowledge which we had of them before these discoveries were made, we shall find out that there is an important difference. The knowledge which we had before was the simple and elementary knowledge of sensation. As compared with that knowledge, the new knowledge we have acquired respecting light and sound, is a knowledge of these things "in themselves." Such is the language in which we should naturally express our sense of that difference, and in so expressing it we should be expressing an important truth. The newer knowledge is a higher knowledge than the older and simpler knowledge which we had before. And why? Wherein does this higher quality of the new knowledge consist? Is it not in the very fact that the new knowledge is the perception of a higher kind of relation than that which we had perceived before? There is no difference between the two kinds of knowledge in respect to the mere abstract character of relativity. The old was as relative as the new; and the new is as relative as the old. Before the new discoveries sound

was known to come from sonorous bodies, and light was known to come from luminous bodies. This was a relation—but a relation of the vaguest and most general kind. As compared with this vague relation the new relation under which we know them is knowledge of a more definite and of a higher kind. Light and Sound we now know to be words or ideas representing not merely any one thing or any two things, but especially a relation of adjustment between a number of things. In this adjustment Light and Sound, as known to sense do "in themselves" consist. Sound becomes known to us as the attunement between certain aerial pulsations and the auditory apparatus. Light becomes known to us as a similar or analogous attunement between the ethereal pulsations and the optic apparatus. Sound in this sense is not the aerial waves "in themselves," but in their relation to the ear. Light is not the ethereal undulations "in themselves," but in their relation to the eye.

It is only when these come into contact with a pre-arranged machinery that they become what we know and speak of as Light and Sound. This conception, therefore, is found to represent and express a pure relation; and it is a conception higher than the one we had before, not because it is either less or more relative, but because its relativity is to a higher faculty of the intellect or the understanding.

And, indeed, when we come to think of it, we see that all kinds of knowledge must take their place and rank according to this order of precedence. For, as all knowledge consists in the establishment of relations between external facts and the various faculties of the mind, the highest knowledge must always be that in which such relations are established with those intellectual powers which are of the highest kind. Hence we have a strictly scientific basis of classification for arranging the three great subjects of all human inquiry—the What, the How, and the Whence or Why. These are steps in an ascending series. What things are, how they come to be, and for what purpose they are intended in the whole system of Nature—these are the questions, each rising above the other, which correspond to the order and the rank of our own faculties in the value and importance of their work.

It is the result of this analysis to establish that, even if it were true that there could be anything in the Universe existing out of relation with other things around it, or if it were conceivable that there could be any knowledge of things as they so exist, it would be no higher knowledge, but infinitely lower knowledge than that which we actually possess. It could at the best be only knowledge of the "What," and that, too, in the lowest conceivable form—knowledge of the barest, driest, nakedest existence, without value or significance of any kind. And further, it results from the same analysis that the relativity of human knowledge, instead of casting any doubt upon its authenticity, is the very characteristic which guarantees its reality and its truth. It results further, that the depth and completeness of that knowledge depends on the degree in which it brings the facts of Nature into relation with the highest faculties of Mind.

It must be so if Man is part of the great system of things in which he lives. It must be so, especially if in being part of it, he is also the highest visible part of it—the product of its "laws" (as regards his own little corner of the Universe) the consummation of its history.

Nor can there be any doubt as to what are the supreme faculties of the human mind. The power of initiating changes in the order of Nature, and of shaping them from the highest motives to the noblest ends—this, in general terms, may be said to include or to involve them all. They are based upon the ultimate and irresolvable power of Will, with such freedom as belongs to it; upon the faculty of understanding the use of means to ends, and upon the Moral Sense which recognizes the law of righteousness and the ultimate Authority on which it rests. If the Universe or any part of it is ever to be really understood by us—if anything in the nature of an explanation is ever to be reached concerning the system of things in which we live, these are the perceptive powers to which the information must be given—these are the faculties to which the explanation must be addressed. When we dedesire to know the nature of things "in themselves," we desire to know the highest of their relations which are conceivable to us: we desire, in the words of Bishop Butler, to know "the Author, the cause and the end of them."

ASTRONOMY.

ELEMENTS OF SWIFT'S COMET.

COMPUTED BY PROFESSOR E. FRISBY, U. S. NAVAL OBSERVATORY, WASHINGTON.

[Communicated by Rear Admiral John Rodgers, U. S. Navy, Superintendent.]

To the Editor of SCIENCE:

The following elements of Swift's comet have been computed by Professor Frisby from three observations made with the Transit Circle at Washington by Professor Eastman on the nights of October 25th, November 7th and 20th, with these results. No assumptions about any periodic time have been made.

Epoch—Perihelion passage.
November 7.77568d, Wash. M. T.

$$\left.\begin{array}{rl} \Omega = & 296^\circ\ 48'\ 19''.9 \\ \pi = & 42\ \ 59\ \ 15\ .8 \\ \phi = & 42\ \ 26\ \ 48\ .5 \\ i = & 5\ \ 30\ \ 35\ .9 \\ \log a = & 0\ .517002 \\ \log \mu = & 2''.774504 \end{array}\right\}\ \text{Mean equinox 1880.0}$$

The comet approached very near to the Earth on November 20th, its distance being less than $\frac{1}{7}$th of the Sun's distance. We have for the dates given:

	log r	log Δ
October 25	0.035328	9.221510
November 7	0.029018	9.141693
" 20	0.034558	9.119295

Its perihelion distance thus appears to be a little greater than the distance of the Earth; and its aphelion lies just beyond Jupiter's orbit. The periodic time from these observations being about 2178d., or a little less than 6 years, there can be no doubt that the preiodic time of about 5½ years is the correct one.

U. S. NAVAL OBSERVATORY, WASHINGTON, D. C.,
January 6, 1881.

THE SOLAR ECLIPSE.—The last contact of the partial solar eclipse on the morning of December 31, 1880, was seen at Harvard College Observatory under quite favorable circumstances. The mean of six observations by as many different observers gives:

Last contact, December 30, 21h. 13m. 3s. Cambridge Mean Time.

At the United States Naval Observatory the last contact was observed by Prof. Hall, with a comet seeker of 4in. aperture and magnifying power of 19 diameters, as follows:

Last contact, December 30, 20h. 32m. 36s. Washington Mean Time. Owing to the extremely low temperature (11 degrees below zero, Fahr.) at Washington, the images were very poor and the observation somewhat uncertain. W. C. W.

NEW YORK MICROSCOPICAL SOCIETY.

The annual meeting for the selection of officers for the year 1881, took place on the 31st ultimo, when the following officers were elected: President, Romyn Hitchcock; Vice-President, E. C. Bogart; Recording Secretary, W. H. Mead; Corresponding Secretary, Benjamin Braman; Treasurer, W. C. Hubbard; Curator, Dr. Deems.

This Society will shortly give a public *conversazione*, when a variety of interesting objects will be exhibited, and an opportunity afforded to Microscopists to examine many new forms of Microscope stands which have been recently produced. Those who desire to assist or be present on this occasion should address Professor Romyn Hitchcock, 53 Maiden Lane, N. Y.

EPHEMERIS OF SWIFT'S COMET.

The following is a continuation of Mr. Upton's Ephemeris, which he has corrected by observations made at Washington up to Jan. 7, 1881. Mr. Wendell, at Harvard College Observatory, obtained an observation for position on Jan. 3, and Prof. Hall is of the opinion that the comet can be followed without great difficulty, even after the present moon.

EPHEMERIS—WASHINGTON MIDNIGHT.

1881	R. A. h. m. s.	Dec. ° ′
Jan. 11	6 0 26	+26 57.4
13	6 2 52	26 23.6
15	6 5 15	25 52 2
17	6 7 34	25 22.9
19	6 9 51	24 55 6
21	6 12 7	24 30.2
23	6 14 21	24 6.5
25	6 16 35	23 44 3
27	6 18 48	23 23 5
29	6 21 2	23 4 0
31	6 23 16	+22 45.8

WASHINGTON, D. C., January 8, 1881. W. C. W.

ECLIPSE OF THE SUN.

The partial eclipse of the Sun which occurred on December 31, 1880, was observed with the spectroscope at my private observatory.

For this purpose, the instrument was so adjusted that it would present its slit radially to the limb of the Moon; and the C line was placed in the centre of the field, in order to see any solar protuberance that might be at the place of observation.

At about the time of greatest obscuration, the slit was directed on the Moon's limb outside of the Sun, at some distance from its western cusp. Although the limb of the Moon was absolutely invisible in the telescope outside of the Sun, as ascertained before, yet, the presence of the satellite was immediately made known in the spectroscope, where it gave a very distinct broad grayish band spectrum, running along the brighter spectrum of the vicinity of the Sun.

The phenomenon became more apparent the nearer the slit was moved towards the Sun, and it vanished from sight when it was at a distance estimated at 3 or 4 minutes from the solar limb.

As the eclipse drew nearer the end, the phenomenon became less and less conspicuous on the western side, and at about 9 o'clock it had almost entirely ceased.

An unsuccessful attempt was made to observe the phenomenon taking place at the point of last contact, when the Moon's limb left that of the Sun. For this purpose the slit of the instrument was placed radially to the point of emergence. But either because no phenomenon was perceptible, or perhaps rather because the slit was not exactly at the right place, nothing was seen.

If the dull spectrum obtained when the slit of the spectroscope was placed in the immediate vicinity of the Sun was due only to the solar light, which is reflected by our atmosphere, it is plain that this spectrum would have been as bright on the Moon as it was outside of it, since the terrestrial atmosphere lies as necessarily between the observer and the Moon as it does between us and the Sun, and therefore no dark-band spectrum could have been seen. But as it was visible, it must be inferred that besides the spectrum given off by the solar light reflected by our atmosphere, there must have been some other light, either emitted or reflected, coming from a point situated beyond the Moon, which reinforced the spectrum given off by the solar light reflected by our atmosphere.

This light, undoubtedly, can be no other than that of the solar atmosphere, or Corona, visible during total eclipses of the Sun.

If this reasoning is sound, the conclusions to be drawn from these observations are that the Corona, or at least traces of it, was visible during this partial eclipse, and that it was much brighter in the northwest equatorial regions than it was in the East; and, furthermore, that in the West it was less and less brilliant as it was observed northward, until it was completely invisible in the northern regions of the Sun. L. TROUVELOT.

CAMBRIDGE, *December* 31, 1880.

JUPITER.

OBSERVATIONS OF THE GREAT RED SPOT.

Having devoted most of my observing time this year to the phenomenæ of Jupiter, I would respectfully submit a few observations of the great red spot, situated in the south temperate zone of the planet.

Up to December 14, (the last observation on account of cloudy weather,) I have observed forty transits of the red spot across the central meridian. Thirty-four of these have been complete transits, *i. e.*, the preceding end the middle and the following end being observed.

The following table contains twenty-nine of these transits and is given in Greenwich mean time. The first, third and fifth columns give the observed time of passage of the preceding end, the middle and the following end of the spot.

Columns two, four and six, contain the times by which each portion of the spot preceded the passage of an assumed meridian that has a rotation period of $9^h\ 55^m\ 27.^s08$ (an ephemeris of the transits of this meridian has been published at intervals in the *English Mechanic*, by Herr A. Marth of the Royal Astronomical Society, and is corrected for parallax, velocity of light and phase).

The last column (7) contains the duration of transit in minutes, that is, the interval between the passage of the *P* and *F ends.*

TRANSIT OF JUPITER'S GREAT RED SPOT.

GREENWICH m. t. 1880.	1 Transit of P. end.	2 Preceding Ass'd Meridian.	3 Transit of Middle.	4 Preceding Ass'd Meridian.	5 Transit of F. End.	6 Preceding Ass'd Meridian	7 Duration of Transit.
	h. m.	*h. m.*	*h. m.*	*h. m.*	*h. m.*	*m.*	*m.*
August 30	17 21.9	1 34.7	17 45.4	1 11.2	18 11.4	45.2	49.5
September 9	15 38 9	1 26.6	16 02.4	1 03.1	16 26.4	39.1	47.5
" 14	------	-----	15 14.4	55.4	------	----	----
" 16	16 19.4	1 27.2	16 49.4	57.2	17 11.4	35.2	52.0
" 18	18 01.4	1 22.0	18 24.9	58.5	18 50.4	33.0	49.0
" 25	18 45.0	1 19.5	19 09.7	54.9	19 32.3	32.2	47.3
" 28	16 15.5	1 16.5	16 40.1	51.9	17 03.9	28.1	48 4
" 30	17 51.5	1 17.3	18 19.5	49.3	18 44.0	24.9	52.5
October 1	13 44.3	1 15.2	14 12.0	47.5	14 39.0	20.5	54.7
" 6	12 48.7	1 15.1	13 16.7	47.1	13 44.7	19.1	56.0
" 7	18 32.2	1 17.7	19 00.2	49.7	19 27.0	22.9	54.8
" 10	16 00.7	1 16.7	16 26.7	50.7	16 55.7	21.7	55.0
" 13	13 31.2	1 13.8	13 57.2	47.8	14 23.0	22.0	51.8
" 20	14 16.0	1 10.5	14 40.0	46.5	15 03.8	22.7	47.8
" 22	------	-----	16 17.2	46.2	------	----	----
November 1	14 04.9	1 08 1	14 34.7	38.3	14 56.5	16.5	51.6
" 4	------	-----	12 01.0	39.9	12 25.2	15.7	----
" 8	14 52.0	1 03.3	15 16.7	38.5	15 43.1	12.2	51.1
" 10	16 30.2	1 02.2	16 55.0	37.5	17 19.4	13.0	49.2
" 11	12 20.1	1 03.2	12 42.9	40.4	13 11.2	12.1	51.1
" 18	13 03.2	1 02.8	13 28.0	40.0	13 54.8	11.4	51.4
" 20	14 44.2	59.1	15 08.7	34.6	15 34.0	9.4	49.8
" 22	16 25.9	54.8	16 47.2	33.5	17 15.5	5.2	49.6
" 23	12 18.2	53.4	12 39.5	32.1	13 06.2	5.4	48.0
December 2	14 42.9	49.5	15 02.2	30.2	15 27.9	4.5	45.0
" 5	12 12.9	48.1	12 32.2	28.8	12 57.2	3.8	44.3
" 7	13 46.5	52.0	14 10.0	28.5	------	----	----
" 9	15 24.9	50.2	15 49.3	25.8	16 12.7	2.4	47.8
" 14	14 33.2	49.3	14 56.0	26.5	15 21.4	1.1	48.2

The above table shows that the red spot varies considerably in length. These variations are shown in the last column, marked "Duration of Transit."

Assuming that the red spots period of rotation is 9^h 55^m $37^s.065$—which is probably very near the truth—we find that in one minute of time $0^\circ.604$ of the surface will pass a given meridian. Multiplying the minutes in the last, or column 7, by .604, we get the following table of lengths in longitude on the surface of Jupiter. The first nine are taken from a table of eleven transits observed by me previous to August 30, and published in *English Mechanic*, No. 809:

July 10, 1880	40°.45	Oct. 6, 1880	33°.82
" 17, "	26 .58	" 7, "	33 .10
" 24, "	27 .78	" 10, "	33 .22
" 29, "	32 .62	" 13, "	31 .29
" 31, "	30 .20	" 20, "	28 .87
Aug. 13, "	33 .82	Nov. 1, "	31 .17
" 16, "	23 .86	" 8, "	30 .86
" 17, "	27 .78	" 10, "	29 .72
" 23, "	27 .78	" 11, "	30 .86
" 30, "	29 .90	" 18, "	31 .05
Sept. 9, "	28 .75	" 20, "	30 .08
" 16, "	31 .41	" 22, "	29 .96
" 18, "	29 .60	" 23, "	28 .99
" 25, "	28 .57	Dec. 2, "	27 .18
" 28, "	29 .23	" 5, "	26 .76
" 30, "	31 .71	" 9, "	28 .87
Oct. 1, "	33 .04	" 14, "	29 .11

On July 10 the spot had a narrow strip running from its preceding end. To this is due the great length of the spot on that date. This does not indicate the true length of the spot proper, but as it was a portion of the spot, or continuation, I give the length on that date.

It must not be supposed that, because I have carried the lengths to two places in the decimals, I consider the length accurate to that degree, for the observations have been entirely eye estimations, yet they were very carefully made. I think a variation of one degree in the length of the spot would be easily detected, and probably a less amount, as the agreement between most of the figures is too close and regular to attribute to chance. As my method of observing may be of interest, I will give an example from my note book. First: I watch closely the first end of the spot, and imagine a line dropped from it to the equatorial belt and observe when this is central, for it is much easier to halve the straight edge-like line of the equatorial belt than to halve the disk on a parallel with the spots centre, because the spot itself being on one side of the meridian biases our judgment to a certain extent, while the clean edge of the equatorial belt is free of any obstacle to interfere with our judgment. Second: I compare the spaces between the limbs of the planet and the ends of the spot, when these are seen to be equal, of course the spots centre is in transit. For determining the transit of the following end, the same method as that in determining the preceding end is followed. At the observation of each part of the spot there exists for a short while a period of uncertainty. The beginning of this uncertainty I indicate by *u*, noting the time. In a minute or so I feel sure the time of true phase has arrived, this is noted by *t*, with its time. Shortly, I am certain the phase has passed, this I note as *c*, with its time. The mean of the three is taken for the true phase.

The following is an observation of the transit of the red spot on October 13, 1880, Nashville, *mt*, taken from my note book.

	h. m.		
	u 7 40	*h. m.*	
P	*t* 7 44	7 44	
	c 7 48		
	u 8 07	*h. m.*	*h m.*
M	*t* 8 10	8 10	**8 09.9**
	c 8 13		
	u 8 32.5	*h. m.*	
F	*t* 8 36 5	8 35.8	
	c 8 38.5		

The mean of the nine observations agrees with the observed middle transit to .1 m. This close agreement cannot, of course, be expected often. However, they generally agree to within a few fractions of a minute. In no case have I allowed myself to know beforehand what time any phase *should* occur, as this might influence the observations.

The variations in length of the spot are not only shown by the duration of transit, but are sensible to an observing eye. At each observation I estimate its length, comparing it with the breadth of the disk on the same parallel of latitude. These comparisons show changes in its length, as they vary from 1-3.5 to ⅓ the breadth of the disk, but it is generally the slightest bit less than one-third.

The variations in breadth are compared with the great equatorial band, but unfortunately this is a standard that probably varies itself. The spot's breadth is generally slightly less than ½ the width of the equatorial belt, sometimes it is probably fully half as broad as the belt, but I have never seen it broader than that.

Changes in the width of the space between the south edge of the equatorial belt and the north edge of the spot, are more readily detected, as the space can be easily compared with the breadth of the spot. This space is generally equal to ⅓ the spot's breadth, yet it is sometimes nearly one-half as broad as the spot. I have seen it diminished to one-sixth. These changes are due either to a swelling out of the spot or a broadening of the equatorial belt. It is more likely due to changes in the spot. I have on several occasions estimated that the distance between the southern edge of the equatorial band and the southern edge of the spot was about equal to one-third the distance from the south pole to the equatorial belt.

There are sometimes slight changes in the general form of the spot; at times the ends are blunt or rounded, again they are cigar shaped. One end has been seen rounded while the other was very much pointed. The sides are at times a little flattened, but are generally slightly rounded. On July 24 the south-side was curved or convex, while the north-side was somewhat flattened. It is sometimes long and lanky and then again it is fat and "chubby"—neither of these have been carried to extremes. Faint continuations, or trails, have been visible, sometimes from one end and then from the other. These have on several occasions been seen trailing from both ends at once, but are not always seen without close looking. At times the spot is a deep solid brick color; then again it is lightish red and pale. I have never, for certain, seen any detail on the surface of the spot, but I have sometimes thought that there *was* detail but just too indefinite for my aperture. The outline of the spot is always clean—no diffusion.

These observations are from notes and sketches which I have made this year with a 5-inch Byrne refractor.

E. E. BARNARD.

NASHVILLE, TENN., *December* 27, 1880.

NOTE.—The motions of the spots on Jupiter, in an article by me in "SCIENCE" No. 24, are referred to an assumed rotation period of 9h. 55m. 27.08s., which should have been stated in that article. E. E. B.

PENNULE'S COMET.

The following position of this comet was obtained by ring micrometer, on December 30, 1880, 7h. 01.2 m., Nashville m. t.:

R. A. 19h. 55m. 38.5s.

Dec. + 18° 52′ 39.6″

It is several minutes in diameter and very brightly condensed. E. E. BARNARD.

NASHVILLE, Tenn., Jan. 2, '81.

THE CAMBRIDGE OBSERVATORY.

The Annual Report of Prof. Pickering, Director of Harvard College Observatory, shows that the Observatory has been in a most prosperous condition during the past year, and if the same financial support is extended to it in the future that has been so generously offered in the past few years, it will be enabled to retain its place, inferior to no other Observatory in the country. The work carried on at the Cambridge Observatory consists of observations with the 15in. Equatorial, with the Meridian Circle and Meridian Photometer, together with the attendant reductions; and in the distribution of time-signals over the greater part of New England.

With the large equatorial, many important observations upon the satellites of Mars were made during the opposition of that planet. Employing the method of reducing the light of the planet, by colored glass (a method first used at this Observatory), the number of observed position angles of Deimos was 825; of Phobos, 278; and that of observed distances, 248. The probable errors of one setting were respectively 0.6°, 0.9° and 0.6″. Besides the micrometric work, many photometric observations were made, the results of which indicate that if we assume the satellites to have a capacity for reflecting sunlight equal to that of Mars itself, Deimos has a diameter of about six, and Phobos of about seven miles. The photometric observations upon the eclipses of Jupiter's satellites give reason to believe that by this method the determination of longitudes may be made as accurately as by occultations or lunar culminations. Measurements of the light of planetary nebulæ have been continued. The spectra of nebulæ are also observed through a direct vision prism placed between the object glass and eye-piece of the telescope. The planetary nebulæ retain their shape under these circumstances, obviously indicating that their light is monochromatic. The difference between monochromatic objects and ordinary stars is so marked when thus examined, that a method of detecting small nebulæ was at once suggested, and a comparatively short search revealed three such objects. The most remarkable discovery, however, was in the spectrum of the star Oeltzen 17681, R.A. 18h. 1m. 17s., Dec. −21° 1′, which shows that the light is concentrated in two points of the spectrum, one in the blue, the other in the yellow. A faint, continuous spectrum is also seen.

Between Sept. 24, 1879 and Nov. 1, 1880, observations were made with the Meridian Circle on 277 days, the work being confined to the determination of the absolute co-ordinates of 109 fundamental stars, in connection with which observations of the sun and of Polaris were made as often as possible. Up to Nov. 1, 1880, 183 observations of Polaris had been obtained, 131 of the Sun and 1760 of Fundamental Stars. To furnish the means of measuring the variation of the instrumental changes between one culmination of Polaris and the next, a collimator with focal length of 206 feet was constructed and has given excellent results.

A Meridian Photometer devised by Prof, Pickering has been used in continuing the measurement of the light of all stars visible to the naked eye between the north pole and the parallel of 30° south declination. Over 40,000 separate settings have already been made, and it is probable that the work will be completed in October next. The instrument, as its name implies, is mounted in the meridian and forms polarized images of the pole star and the star to be observed, which are brought to equality by turning a Nicol prism.

The time signals from the Observatory are distributed to the railroads and several prominent jewelers in Boston, and through the railroad companies to many of the neighboring towns. By the co-operation of the United States Signal Service Officer a time-ball is dropped in Boston at noon. The signals are also used in connection with those from the United States Naval Observatory, and the Allegheny City Observatory for the regulation of the New York time service.

During the past year, the second part of Volume XI of the Annals of the Observatory, containing a discussion of 25,000 photometric observations made with the great equatorial, and Volume XII containing the results of observations made by Prof. W. A. Rogers in 1874 and 1875 with the Meridian Circle have been completed and distributed, and six more volumes are in a more or less advanced state of preparation. W. C. W.

WASHINGTON, D. C.

ON THE THERMAL BALANCE.*

By Prof. S. P. Langley.

When the thermometer is not sufficiently sensitive for delicate investigation of radiant heat, scientific men have been accustomed, since the time of Melloni, to the use of the thermopile, an instrument which, employed in connection with the galvanometer, permits the making of numerous important measures. It has not been improved materially in the last fifty years. Meanwhile, many problems of both high theoretical and practical interest have arisen, which cannot be solved without a more sensitive and accurate instrument. One of these problems is the measurement of the distribution of radiant energy in a pure spectrum, when the rays have not passed through any prism. I could obtain no accurate results with the thermopile. I was forced to invent a more sensitive instrument for this special investigation, and, having done so, I believe it will be of general utility. The principle of the new apparatus has been applied by Dr. Siemens and others to other purposes. I spent several months in making it, as I hope, a useful working tool for the physicist and the physical astronomer. It is founded on the principle that, if a wire conveying an electric current be heated, less electricity flows through it than before. If two such wires, carrying equal currents from a powerful battery, meet in a recording apparatus (the galvanometer) the index of the instrument—pushed in two opposite ways by exactly equal forces—will remain at rest. If one current be diminished by warming ever so little the wire that conveys it, the other current causes the index to swing with a force due, not directly to the feeble heat which warmed the wire, but to the power of the battery which this feeble heat controls.

The application of this principle is thus made: Iron or steel is rolled into sheets of extreme thinness. I have succeeded in rolling sheets of steel made at the works of Miller & Parkin, Pittsburg, Penn., until it took 8000 of them to make the thickness of an inch. Of the platina sheets rolled at the United States Mint in Philadelphia, fifty laid one on another do not together equal the thickness of light tissue paper. Minute strips of these, 1-32 of an inch wide and ¼ of an inch long, were united so as to form a prominent part of the circuit, through which a part of a powerful battery passed to the galvanometer. Experiment proves that an almost inconceivably minute warming of a set of these strips reduced the passage of the electricity so as to produce very large indications on the registering instrument. I have in the course of my experiments thus far, found iron the most advantageous, though other metals are still under trial. The instrument thus formed is from ten to thirty times more sensitive than the most delicate thermopile; but this is almost a secondary advantage compared with its great precision and the readiness with which it is used. The thermopile is very slow in its action. This new instrument, the thermal-balance, takes up the heat and parts with it again in a single second. It is almost as prompt as the human eye itself.

With reference to its accuracy, experiments prove that the probable error of a single measurement made

* Read before the National Academy of Sciences, N. Y., 1880

with the instrument can be reduced to within 1 per cent. of the amount to be measured. It will register a change in the temperature of the strips just described, not exceeding 1-50,000 part of a Fahrenheit degree. When mounted in a reflecting telescope it will record the heat from the body of a man or other animal in an adjoining field, and can do so at great distances. It will do this equally well in the night, and may be said, in a certain sense, to give the power of seeing in the dark. A more valuable proof of its efficiency is shown in a series of measurements of the heat of the moon, made under varied circumstances, to guard against error, but each made in a few seconds. All these measurements show that the almost immeasurably minute amount of heat from the moon can be certainly measured by it, even with a common refracting telescope.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of SCIENCE :—

In a recent issue of "SCIENCE," "B. G. W." in a very instructive review of Marsh's monograph on the limbs of the *Sauranodon*, speaks of Darwin's hypothesis regarding *sexdigitism* in man, as reluctantly abandoned by that evolutionist, but as now standing some chances of rehabilitation owing to the discovery of *sexdigitism* as a normal feature of the extinct genus *Sauranodon.* Probably the reviewer has not met with a treatise, in in which a certain discovery of an embryonic peculiarity is detailed, and which explains not only the occurrence of *sexdigitism* but of *polydactylism* in man. As this treatise is in the hands of few comparative anatomists, I may refer to the facts here at some length. In figure 76 on page 137 of Schenk's *Lehrbuch der vergl. Embryologie der Wirbelthiere* (Vienna, 1874), is represented a section taken flatwise through the embryonic human paw. The chondrogenic elements of the mesoblast can be seen arranged in strands, indicating the metacarpophalangeal rays. A sixth ray seems very clearly present, and from some of the other rays lateral processes spring, which in the course of normal development become merged into the main ray, no doubt.

On this head, as well as some others related to the temporary presence of ancestral features in the extremities of the human embryo, I have written as follows in a series of lessons on embryology, published in the *St. Louis Clinical Record:*

At the points where the head and tail were respectively deflected from the trunk the peripheral protovertebrol masses are bulged out, as it were, and thus we have twa anterior and two posterior ill-marked eminences composed of mesoblast elements covered by the cutaneous epiblast. These are the anterior and posterior extremities. The posterior pair is the earliest to be discovered, but it is so rapidly outstripped in growth and development by the anterior extremities, that the belief has become current that the anterior are the first to appear, which is incorrect.

At the time when the hand has become demarcated from the forearm by the wrist constriction, the forearm has not yet become separated from the arm. And in like manner the foot is individualized before the leg and thigh are demarcated. The fingers are developed before the toes, and in both the hand and foot the digital segmentation is preceded by a stage in which there is a fold formed separating a main mass from the aggregate digital mass, and which persists in the adult.

If a surface section be made of an embryonic hand or foot before the digits are formed, we will find that the cell-strands which constitute the basis for each metacarpo-phalangeal ray are not five, as in the adult and developed fœtus, but are from seven to nine (at different periods) in number. This remarkable fact, discovered by my teacher, Prof. Schenk, of Vienna, points, in a manner, to the descent of the pentadactylous animals, to which man belongs, from the enaliosaurians or analogous groups of the jurassic and triassic periods of the earth's history whose fossilized remnants clearly show that they had seven or more fin rays.

To many, another and related fact will prove still more convincing in an evolutional point of view, although Schenk's observation is of more fundamental importance than the following to zoötomists:

Hensen, of Kiel, discovered that, in a ***human*** embryo of the seventh week, ***the fingers and toes are provided with claw-like appendages like the claws of carnivora,*** and that these structures are exfoliated to make way for the true nails. Further, he found plantar and palmar eminences like the ***foot-pads of the dog, cat and marsupial carnivores.****

E. C. SPITZKA.

NEW YORK Jan. 7, 1881.

BOOKS RECEIVED.

WAS MAN CREATED? By HENRY A. MOTT, JR., PH. D. Griswold and Company, New York.

The time is still distant when conclusions will be drawn on the subject of the Origin of Man and many other problems treated by the author of this book. Material is accumulating faster than it can be arranged, but in all probability, a thousand years hence we shall still be without sufficient data and be diligently searching for evidence.

The scientific man is not discouraged on this account, but is well content to work on, adding daily to the great store-house of knowledge, indifferent as to whether final results are arrived at in his own day or in the future.

There is, however, another class of persons in society, who, finding that certain scientific truths, which are undeniable, conflict with revealed religion, desire a more speedy solution of these questions.

Dr. Mott in his book attempts to outline a middle course for those who are forced by scientific discovery to renounce the Biblical teachings respecting the Origin of Man, by showing from a large number of authorities, that a belief in the dual existence of man may be held upon reasonable testimony.

Had Dr. Mott called his book "An Introduction to the Study of the Origin of Man and his Future Destiny," we think it would have been an appropriate title, and would have commanded a large class of readers who are unable to obtain the larger works consulted by the author; and the seventy-five illustrations, which are well selected, would have been of considerable service to such persons in grasping the subject which is naturally complicated to those who approach it for the first time.

DR. IRVINE, of Glasgow, recently exhibited and explained before the Mining Institute of Scotland, his new safety-lamp, which is constructed to emit a loud sound when an explosive mixture of gas and air enters it, and thus consequently indicates fire damp in colleries.

* Development of the Human Ovum Embryo, and Fœtus, *St. Louis Clinical Record*, (Lecture VIII.) June, 1880.

SCIENCE:

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JOHN MICHELS, Editor.

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NOTICE TO SUBSCRIBERS.

We consider it due to those subscribers who have favored us with their subscriptions, previous to the publication of our club rates, that they should have the privileges of the list. They can therefore send us subscriptions for any or all of the publications named at the reduced double rates, less $4, the subscription price of "SCIENCE."

The lecture of Dr. George F. Beard on what he prefers to call "Mesmeric Trance," delivered this week before the New York Academy of Sciences, in the hal of the New York Academy of Medicine, received the close attention of an audience the majority of which, apparently, witnessed the experiments for the first time.

Dr. Beard described, briefly, the various forms of trance with which neurologists are familiar, and was supported by eight trance subjects, who exhibited manifestations of trance phenomena, to the equal satisfaction of the lecturer and his appreciative audience.

In regard to the genuineness of Dr. Beard's demonstrations we have no doubt that, substantially, they were *bona fide*, but it seemed apparent that the miserable objects who did duty on the occasion overacted their parts, and it may be even now an open question whether Dr. Beard or his audience was more imposed upon. Without intending to assert that an imposition was intended or practiced on the occasion, it is not difficult to show, probably, that many of the experiments might have been illusions. Two of the so-called patients were evidently trained performers, if not professional actors; if merely amateurs they surely missed their vocation. One of these patients could throw himself from an erect position to the stage, on his face, with the ease of an acrobat; the other declaimed Shakespeare at short notice, with the energy and persistence of a barrel organ. Other experiments also developed phenomena, which were not part of the programme. The boy who ate Cayenne pepper in a trance, believing it to be sugar, appeared to be not inconvenienced in the least when he returned to a normal condition. But still more remarkable was the behavior of the patient who was made "stone deaf." Dr. Beard shouted in vain to this man, a tuning fork was sounded, a bell rung, and even a pistol fired close to his devoted head, while the patient remained eloquently silent and apparently oblivious to all external sounds.

To de-hypnotize the subject, Dr. Beard, unmindful of the fact that he was supposed to be addressing a deaf person, said, in an ordinary tone of voice: "It's all right!" that being the usual phrase employed. To the surprise of many present, the patient (perhaps not desiring a *contretemps* to mar the performance) took the cue and quietly resumed his seat.

To a popular audience Dr. Beard's theories and experiments might have partaken of the character of a revelation, but we believe that nearly all our present knowledge of the subject dates from Braid's book on Hypnotism, published more than twenty years ago.

The policy of such public exhibitions may be well questioned; in Vienna they have been prohibited, and as no new truth can be gained or science advanced by repeating these experiments in such a manner, why make them the subject for an evening's amusement before a scientific society?

The patients selected perform their parts constantly, and thus become finally, perhaps unconsciously, more and more trained to elaborate their antics, so that, even admitting the genuineness of the performance, the experiments may be, at least so far, manufactured.

The subjects of Dr. Beard are chiefly selected from the nervous classes of our population, and although they may be willing to air their peculiarities before a fashionable audience, it would appear to be a charitable course to keep them from such public exhibitions which can result only in aggravating their morbid tendencies.

NEW YORK ACADEMY OF SCIENCES.

The Committee on Lectures announces that the remainder of the course will embrace five lectures, to be delivered at the new Hall of the Academy of Medicine, No. 12 West Thirty-first street, New York City, on the third Monday of each month.

These lectures are free to the public, but admission is strictly confined to those holding tickets, which may be obtained of D. S. Martin, 236 West Fourth street; W. P. Trowbridge, School of Mines, East Forty-ninth street, and Alex. A. Julien, School of Mines, East Forty-ninth street.

The programme includes the following lectures: January 17th, Respiration, by Dr. J. W. S. Arnold; February 21st, The Reptilian Affinities of Birds, by Professor Edward S. Morse; March 21, Sensation and Pain, by Dr. Charles Fayette Taylor; April 18th, Temple Architecture of the Tenth to the Fifteenth Century, by Professor George W. Plympton; May 10th, The Organic Elements, by Professor Albert R. Leeds.

HUNGER THE PRIMITIVE DESIRE.

By S. V. Clevenger, M. D.

A paper on Researches into the Life History of the Monads by W. H. Dallinger, F. R. M. S., and J. Drysdale, M. D., was read before the Royal Micrscopical Society, Dec. 3d, 1873, wherein fission of the Monad was described as being preceded by the absorption of one form by another. One Monad would fix on the sarcode of another and the substance of the lesser or under one would pass into the upper one. In about two hours the merest trace of the lower one was left and in four hours fission and multiplication of the larger monad began. A full description of this interesting phenomenon may be found in the *Monthly Microscopical Journal* (London), for October, 1877.

Professor Leidy has asserted that the Amœba is a cannibal, whereupon Mr. J. Michels in the *American Journal of Microscopy*, July, 1877, calls attention to Dallinger and Drysdale's contribution and draws therefrom the inference that each cannibalistic act of the Amœba is a reproductive one, or copulative, if the term is admissible, The editor (Dr. Henry Lawson), of the English journal, Oct., 1877, agrees with Michels.

Among the numerous speculations upon the origin of the sexual appetite, such as Maudsley's altruistic conclusion, which always seemed to me to be far-fetched, I have encountered none that referred its derivation to *hunger*. At first glance such a suggestion seems ludicrous enough, but a little consideration will show that in thus fusing two desires we have still to get at the meaning and derivation of the primary one—desire for food.

The cannibalistic Amœba may, as Dallinger's Monad certainly does, impregnate itself by eating its own kind, and we have innumerable instances among Algæ and Protozoa of this sexual fusion appearing very much like ingestion. Crabs have been seen to confuse the two desires by actually eating portions of each other while copulating, and in a recent number of the *Scientific American*, a Texan details the *Mantis religiosa* female eating off the head of the male Mantis during conjugation. Some of the female Arachnidæ find it necessary to finish the marital repast by devouring the male, who tries to scamper away from his fate. The bitings and even the embrace of the higher animals appears to have reference to this derivation. It is a physiological fact that association often transfers an instinct in an apparently outrageous manner. With quadrupeds it is undoubtedly olfaction that is most closely related to sexual desire and its reflexes, but not so in man. Ferrier diligently searches the region of the temporal lobe near its connection with the olfactory nerve for the seat of sexuality, but with the diminished importance of the smelling sense in man the faculty of sight has grown to vicarate olfaction; certainly the "lust of the eyes" is greater than that of other special sense organs among Bimana.

In all animal life multiplication proceeds from growth, and until a certain stage of growth, puberty, is reached, reproduction does not occur. The complementary nature of growth and reproduction is observable in the large size attained by some animals after castration. Could we stop the division of an Amœba a comparable increase in size would be effected. The grotesqueness of these views is due to their novelty, not to their being unjustifiable.

While it would thus seem apparent that a primeval origin for both ingestive and sexual desire existed, and that each is a true hunger, the one being repressible and in higher animal life being subjected to more control than the other, the question then presents itself: What is hunger? It requires but little reflection to convince one of its potency in determining the destinies of nations and individuals, and what a stimulus it is in animated creation. It seems likely that it has its origin in the atomic affinities of inanimate nature, a view monistic enough to please Haeckel and Tyndall.

NOTES ON THE ANATOMY OF THE ENCEPHALON, NOTABLY OF THE GREAT GANGLIA.

By Edward C. Spitzka, M. D.

The anatomy of no portion of the brain is so obscure and so imperfectly known as that of the so-called Thalamus opticus. One of the first requisites to a comprehension of its relations is the establishment of a proper nomenclature, and the point to start from is the very name under which the great ganglionic mass is known. Since it is not exclusively or even in the main connected with the optic tracts in any animal or man, and, indeed, is in the lower sauropsidæ and amphibians not connected with them at all, the affix *opticus* should be dropped, and the first word involving that very uncompromising conception of an elevation at the ventricular floor may be retained: *Thalamus.*

The current conception that the Thalamus is an elevation at the floor of the *lateral* ventricle is incorrect. One of our leading comparative anatomists will shortly review this question, and it will therefore be but necessary for me to refer to the matter.

In the cat's brain it can be clearly seen, that (aside from membranous separations) the great mass of the Thalamus is excluded from the cavity of the lateral ventricle by the fusion of the lateral edge of the fornix with the corpus striatum, or rather with the ependyma of that ganglion. Consequently, the two thalami are included in the third ventricle, which cavity on cross section resembles an upright T, whose vertical branch descends between the thalami, as a deep ditch, the *vulva cerebri* of the old anatomists.[1]

Luys, who was unfortunately wedded to certain physiological prejudices as to the function of the thalamic centres, restricted the term *Thalamus* to the most external mass. Meynert called all the centres in the aggregate by that term as a collective designation. He excluded, however, that gray mass which lines the sides of the vertical slit of the third ventricle.

Now, the third ventricle, as shown by Hadlich and Wilder, extends over the entire thalami; it would be, therefore, incorrect to limit the designation "central tubular gray of the third ventricle" to that portion only which lines the vertical slit. Either this latter designation should be extended to the entire thalamic masses or the term thalamus should be extended to the so-called central tubular gray.

Thus interpreted there would be, strictly speaking, but a single thalamus, consisting of two main masses, and a commissural part. The commissure is double. The thalami are primitively united by the lower of these commissures, which I propose to term "basilar commissure."[2] Secondarily, and only in animals above marsupials (as far as I am aware), do we find another commissure produced at an advanced period of embryonic development by apposition of the main masses. This is the so-called middle commissure of the brain, the *commissura grisea, c. mollis.* I should consider the least ambiguous designation, "the *thalamic fusion.*"

In a manner similar to that which separates the caudate and lenticular nuclei from each other, and which divides the latter into subsidiary "articuli," the chief mass of each thalamus is separated into an inner and outer zone. The zones are separated from each other by

[1] The corresponding *penis cerebri* of the same anatomists has, by more fastidious colleagues, been rebaptized *pinus cerebri* and later *pineal gland*, now known as the *epiphysis cerebri*.

[2] Continuous in front with the *loci perforati antici*, behind with the infundibulum. Atrophic over the chiasm, it exhibits a set of transverse fibres and gray substance elsewhere.

a white intercalation, and especially the outer zone (also in part the inner) presents a beautiful alternation of gray and white laminæ.[3]

These two gray zones constitute the fundamental demarcation of the thalamus; they may be termed *zona grisea medialis* and *zona grisea lateralis*. In animals above the rank of marsupials we find added a round nodular mass, distinctly prominent at the ventricular floor, which lies anteriorly, while in still higher groups a second nodular prominence develops posteriorly. The latter is known as the posterior tubercle or pulvinarium, the former as the *anterior* or *snperior tubercle*. The former designation seems the best to me, for although what I call the undifferentiated parent mass of the thalami is visible in sections anterior to those in which the anterior tubercle is reached, yet the latter, which I propose to term the *anterior nodule* of the thalamus, is the first differentiated centre reached. In man the *zona grisea medialis* is faintly seen before the anterior nodule is reached, but the anterior nodule reaches its main development before the zones do, and is absent where these are most prominent. In the carnivora generally, the anterior nodule projects far in advance of the zones. In these animals, too, a more complex arrangement of this nodule is found than in man, inasmuch as the anterior part of the internal slope of the thalamus shows several elevations absent in the human thalamus.

The *zona grisea medialis* appears pretty equally diffused and exhibits its lamination evenly both in front and in the middle of its course. The same applies to the human brain for the *zona grisea lateralis*. In the cat,[4] however, the anterior part of the external zone appears as a beautiful round compact ganglionic mass, protruding boldly into the internal capsule, and which acquires the characteristic lamination only in posterior planes.

It is interesting to note that the ganglionic matter of the thalamus is continuous with that of the ventricular nucleus of the corpus striatum (nucleus caudatus). Indirectly it is connected with the extra-ventricular nucleus, through that great common basilar gray mass, which is the *rendezvous*, as it were, of all the gray categories of the forebrain.[5]

In an earlier publication (Architecture and Mechanism of the Brain—Journal of Mental and Nervous Diseases, 1879), I have called attention to the fact that the ventricular nucleus of the corpus striatum is the representative of the primordial cerebral gray, inasmuch as the nerve cells of the embryonic and lower amphibian hemisphere are concentrated immediately subjacent to the ependyma of the latter ventricle. The majority of these cells are crowded away from the ventricular floor by the white substance developed in higher animals, and only a portion of the primitive gray remains subependymal. This is precisely what constitutes the corpus striatum. Now the corpus striatum actually *lines* the ventricle; it not only lies at its floor! Any section transversely to the cerebral axis and striking the forepart of the lateral ventricle in the Hippopotamus, Horse, Dog or Cat, will show that an attenuated part of the corpus striatum is continued around *over* the ventricle, and constitutes a greater part of its roof.

A similar comparative study shows that the nucleus lenticularis is also a subcortical development, that is, it results from the individualization of a gray mass originally continuous with the cortex, by means of an irruption of white masses. These at first separate fasciculi (as in the dog) in higher animals coalesce to constitute the external capsule. The segmentation of the lenticular nucleus into three distinct *articuli* so characteristic of the human brain, is not found in the carnivora; only the outer articulus is demarcated, and that but imperfectly.

In the carnivora the *laminae medullares* or white streaks of the lenticular nucleus are conspicuously absent in the anterior half of that ganglion; in its posterior half they appear and they rapidly increase in bulk as we proceed backwards, so that in planes where the human lenticular nucleus is still quite massive, we have in the dog only slight ganglionic masses intercalated between the fibre tracts. The *claustrum* is, in the carnivora, not the thin expanded lamina found in man, but a low and massive accumulation, hardly separated from the cortex of the Island of Reil. This fact strengthens Meynert's view that the claustrum is but an individualized cortical layer.

In conclusion I would mention as an isolated fact, and disconnected from the main subjects dealt with in these notes, that the anterior pyramids of the brain of the large Ceylon fruit bat (Pteropus fuliginosus) undergo a superficial decussation, as patent, and more so, as that of the optic chiasm. The pyramidal tract after decussating is continued as a distinct fasciculus on the lateral aspect of the medulla oblongata. In the same brain the fibres of the fornix can be clearly seen to terminate in the thalamus without descending to the base of the brain. Whether this applies to the whole of that tract, I am not able to say.

I would also note that in the brain of a large Ara (*Ara ararauna*) obtained from the Superintendent of the Central Park Zoological Gardens, Mr. W. A. Conklin, I found what appeared to be a thin commissure uniting the two cerebral hemispheres in their posterior half. This (commissure! if the observation was correct) was not, like the Corpus callosum, a connection between the internal white matter of both hemispheres, but merely a union of the superficial white, which in lower animals is well-developed outside of the cortical gray.

In the *carnivora the Ganglion* of Soemmering (the *Substantia nigra* in the human brain) is continuous with the innermost part of the lenticular nucleus. This fact strengthens Meynert's proposition, that the Ganglion of Soemmering, like the caudate and lenticular nuclei, should be considered as parts of one system, whose ganglia are connected with the fibres of the *pes pedunculi*.

In the elephant, whose brain, both in its mass, the preponderance of the hemispheres, and the concealment from view of the so-called "trapezium," takes a high rank as regards the grade of development, I had the opportunity to make and examine transverse microscopic sections from the Pons Varolii. The remarkable discovery was made that the descending (longitudinal) fibres of the Pons are wanting. Nothing but transverse fasciculi are seen in the field. Since the former fibres constitute part of the pyramidal tract, it follows that the tract of the voluntary impulses, the "will-tract," must take another course in the elephant, one which may be considered aberrant; for in all other placental animals so far examined by myself, the pyramidal tract runs through the Pons Varolii, as in man.

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[3] And yet the latest pretended description of these Ganglia, admitted, notwithstanding numberless glaring errors, into a journal of the standing of "Brain" (that by Dalton), has the Thalamus "homogenous."

[4] As seen in a series of transverse sections prepared by Dr. Graeme Hammond.

[5] Here meet the olfactory gray, the cortex, the *basis capitis nuclei caudati*, the *nucleus lenticularis*, the *claustrum*, the thalamic axial gray, etc., etc.

ON WALDIVINE.—Waldivine, $C_{36}H_{24}O_{20}$, is a neutral principle, without rotatory power, very sparingly soluble in cold water, freely soluble in chloroform, insoluble in ether, and remarkable for the ease with which it is decomposed by alkalies.

CERTAIN OPTICAL AND VISUAL PHENOMENA—If the flame of a lamp is viewed through a narrow slit, the lustre of the flame and the effects of diffraction vary much according as the slit is vertical or horizontal, the light being much more considerable in the latter case.—M. TREVE.

EFFECT OF PRESSURE ON THE FUSION POINT.

Dr. Carnelly recently read a paper before the Chemical Society of London, in which he thus explains the device which he has adopted in order to secure and maintain a vacuum in a case of ice. For the success of the experiment the tension must be below 5 millimetres. The apparatus consists of a wide glass tube ¾ inch in diameter, and about 5 to 6 feet high. This is placed in a vertical position, and is connected at its upper end with a strong glass flask placed horizontally, and surrounded with a freezing mixture. The apparatus having been inverted and filled with mercury, the lower end of the tube is closed with the thumb, and placed under the surface of a layer of mercury about 10 inches deep. On withdrawing the thumb the mercury sinks in the tube to the barometric height, and a large Torricellian vacuum is obtained, which is surrounded, as far as the flask is concerned, with a freezing-mixture. A small quantity of boiled water is now introduced, which rises to the top of the mercurial column, and surrounds the bulb of a thermometer suspended inside of the tube. The water is then frozen, and the depth of the layer of mercury in which the tube stands reduced to about 3 ins.; in consequence the mercury in the tube sinks, and leaves a detached column of ice with the thermometer bulb in its centre. This column acts as a cork, shutting off the large vacuous space above from the small vacuum below. By carefully heating the tube the ice is melted round the circumference of the plug, and a fine annular opening is made between the ice and the inside of the glass tube. This restores the communication between the upper and lower portion of the vacuum. As soon as this is effected, any aqueous vapor which is formed is at once condensed by the freezing-mixture, and the vacuum is kept intact. Under these circumstances the author has made the ice so hot that the thermometer in the centre of the cylinder stood at 180° C. before the ice melted. In the experiment shown to the Society the thermometer only rose to 30° C. when the cylinder (which was too large and therefore too heavy) dropped off the thermometer. To prove that the ice was really hot Dr. Carnelly has contrived and carried out some experiments, in which the cylinder of hot ice was dropped into a small calorimeter filled with water; the temperature rose when the ice was introduced, whereas if ordinary ice it would of course have been lowered. He then showed two experiments with camphor and mercuric chloride, which were perfectly successful. The camphor was contained in a glass tube closed at one end and connected at the other with a Sprengel pump. On heating the tube the camphor melted, but on starting the Sprengel pump the camphor, as the pressure decreased, solidified, though the heating was continuous. The mercuric chloride was similarly raised many degrees above its ordinary melting-point, when kept under diminished pressure, without liquefying; but on allowing the atmospheric pressure to enter, by cutting the tube, the solid mass immediately melted and began to boil.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

We are informed by Professor Cleveland Abbe that the following are the newly elected officers of the Philosophical Society of Washington: President, Dr. J. J. Woodward; Vice-Presidents, Dr. G. K. Barnes, J. E. Hilgard, J. C. Welling, William Taylor; Secretaries, J. N. Gill, C. E. Dutton; Treasurer, Cleveland Abbe.

HYPNOTISM.

A writer in the *Medical Record* sums up the result of his experiences of Hypnotism and its phenomena as follows:

First. Impressions cannot be communicated to individuals in the hypnotic condition, except through the external senses. The mind of the operator cannot influence that of the subject by a purely mental effort. He must either speak, write, or gesticulate to convey his ideas.

Second. Remembrance of what has passed, during the hypnotic state, in the mind of the subject, is very slight, but if he is told to remember any particular thing while so affected, he will recollect it when he awakens.

Third. Although I pursued the method used by others, I am satisfied that the employment of any means that will induce a temporary abstraction of the mind is all that is required to induce the peculiar condition.

Fourth. Although the subjects seem to be entirely oblivious to all that is going on, they are not perfectly so. In the case of a young lady, who was told that she was a bird, and thereupon commenced to hop, her dress became disarranged, and, although continuing to hop like a bird, she was careful to keep her dress in its proper condition.

Fifth. It is not necessary that the operator nor the one operated upon believe in the truth of hypnotism, or the success of the trial. If the necessary conditions are complied with the effect will follow. One case mentioned above proves this to be true.

All the strange psychical conditions under the names of hypnotism, magnetism, braidism, mesmerism, trance, somnambulism, ecstacy, etc., come under the same category, and I believe that clairvoyance and spiritualism can be included in the list.

As far as I have seen, I have never observed contraction of muscles, areas of hyperæsthesia, or other disorder of sensibility, or any unnatural condition or action of any part of the body in the persons affected, unless the operator should direct their attention to themselves by speaking or motioning to them; for example, he would indicate that their faces were away, that their arms or fingers were stiff, or that they had a pain in the head, back, or some other part. In such a case what was told them would be the basis on which they would feel or act.

If I should venture an explanation, or more properly a description of the phenomena of hypnotism, I would say that they resulted from a suspension of function of the centre for ideas in the brain of the subject, and also of his will, while the infra-cortical ganglia remain free to act from a reflex excitation imparted by the voice, gestures, or manners of the operator.

THE HAMMOND PRIZE.

The American Neurological Association offers a prize of five hundred dollars, to be known as the "William A. Hammond Prize," and to be awarded, at the meeting in June, 1882, to the author of the best essay on the *Functions of the Thalamus in Man.* The conditions under which the prize is to be awarded are as follows: 1. The prize is open to competitors of all nationalities. 2. The essays are to be based upon original observations and experiments on man and the lower animals. 3. The competing essays must be written in the English, French, or German language; if in the last, the manuscript is to be in the Italian handwriting. 4. Essays are to be sent (postage prepaid) to the Secretary of the Prize Committee, Dr. E. C. Seguin, No. 41 West Twentieth street, New York City, on or before February 1, 1882; each essay to be marked by a distinctive device or motto, and accompanied by a sealed envelope bearing the same device or motto, and containing the author's visiting card. 5. The

successful essay will be the property of the Association, which will assume the care of its publication. 6. Any intimation tending to reveal the authorship of any of the essays submitted, whether directly or indirectly conveyed to the Committee or to any member thereof, shall exclude the essay from competition. 7. The award of the prize will be announced by the undersigned Committee; and will be publicly declared by the President of the Association at the meeting in June, 1882. 8. The amount of the prize will be given to the successful competitor in gold coin of the United States, or, if he prefer it, in the shape of a gold medal bearing a suitable device and inscription.

Signed, F. T. MILES, M.D., *Baltimore.*
J. S. JEWELL, M.D., *Chicago.*
E. C. SEGUIN, M.D., *New York.*

CHESAPEAKE ZOOLOGICAL LABORATORY.

Dr. W. K. Brooks, Director of the Chesapeake Zoological Laboratory, established under the auspices of the the Johns Hopkins University, in his report for 1880 states: By the liberality of the Trustees, it was possible to spend a much longer period than hitherto at the seaside, and provided with a more liberal outfit, including a steam launch which was built, for our use in the last spring, at Bristol, R. I., and has proved a very efficient auxiliary. The necessary books, dredges, and other instruments were also provided by the University. In addition to the opportunities afforded to three of the members of our own academic staff, three other gentlemen, devoted to the study of Zoology, were invited to avail themselves of the scientific facilities of the station.

The laboratory was opened at Beaufort, N. C., on April 23, 1880, and closed on September 30, after a session of twenty-three weeks. It was supplied with working accommodations for six investigators, and the facilities which it afforded were used by the following six persons: W. K. BROOKS, PH. D., Director; K. MITSUKURI, PH. B., Fellow in Biology; E. B. WILSON, PH. B., Fellow in Biology; F. W. KING, A. M., Professor of Natural Science, Wisconsin State Normal School; H. C. EVARTS, M. D., Academy of Natural Sciences, Philadelphia; H. F. OSBORNE, PH. D. Fellow of the College of New Jersey.

Beaufort was selected for our third season's work because it is the nearest accessible town, south of Baltimore, which is favorably situated for zoological study. The advantages of a location in a town are well shown by the fact that the expenses of a session of twenty-three weeks this year were considerably less than those of a ten weeks session the year before.

The scientific advantages of Beaufort are very great; the most important is the great difference between its fauna and that of our northern Atlantic coast.

The configuration of our coast line is such that Cape Hatteras, the most projecting point south of New York, deflects the warm water of the Gulf Stream away from the coast, and thus forms an abrupt barrier between a cold northern coast and a warm southern one. The fauna north of this barrier passes gradually into that of southern New England, while the fauna south of this barrier passes without any abrupt change into that of Florida, but the northern fauna is sharply separated by Cape Hatteras from the southern.

As the laboratory of the U. S. Fish Commission and Mr. Agassiz's laboratory at Newport afford opportunites for work upon the northern fauna, it seemed best for us to select a point south of Cape Hatteras in order to study the southern fauna with the same advantages, and as Beaufort is the only town near the Cape which can be reached without difficulty, it was chosen as the best place for the laboratory.

The situation of this town is exceptionally favorable for zoölogical work, for the surrounding waters present such a diversity of conditions that the fauna is unusually rich and varied.

Close to the town there are large sand bars, bare for miles at low tide, and abounding in animal life. From these we could collect an unfailing supply of *Amphioxus*, *Renilla*; *Limulus*, *Balanoglosus*, Sea Urchins, and a great variety of Molluscs and Crustacea.

The mud flats furnished us with another fauna, and yielded a great variety of Annelids, a new set of species of Crustacea and Molluscs, Gephyreans, Echinoderms, and Polyps. The large salt marshes gave us a third fauna, and a short distance inland large swamps of brackish and fresh water furnished still other conditions of life.

As the town is situated at the point where Gore Sound connects Pamlico Sound with Bogue Sound we were within easy reach of a continuous sheet of landlocked salt water more than a hundred miles long, and these Sounds furnished still another collecting and dredging ground, abounding in Corals, Gorgonias, Ascidians, Star Fish, Sea Urchins, and a new set of Molluscs and Crustacea.

As most of the shores are flat and sandy, those animals which live upon a sandy bottom are much more abundant than those which attach themselves to solid bodies, but the stone breakwaters at Fort Macon, the wharves at Beaufort and Morehead City, and the large oyster beds which are found in the sounds furnish a proper habitat for many fixed animals, and yielded us a rich supply of Hydroids, Corals, Ascidians, Sea Anemones, Sponges, Cirrhipeds, &c. The ocean beach, within a short distance of the town, furnished still another fauna, and a soil of three miles from the laboratory carried us to a good locality for ocean dredging.

The greatest advantage of the locality is the richness of its pelagic fauna. There are very few points upon land which are so situated that the surface animals of mid-ocean can be procured in abundance for laboratory work, and as careful work is very difficult on shipboard, a laboratory which can be furnished with a good supply of living pelagic animals presents opportunities for work in an extremely interesting and almost new field.

The Gulf Stream is constantly sweeping these animals northwards along the North Carolina coast, and as the tide sets in through Beaufort Inlet into the Sounds the floating animals are carried with it. Such oceanic animals as *Physalia* and *Porpita* were frequently thrown, uninjured and in perfect health, upon the beach within twenty feet of the laboratory, and during the season we found nearly all the Siphonophoræ which are known to occur upon our Atlantic coast.

With all these advantages we enjoyed a mild and uniform climate which enabled us to work in perfect comfort during the hottest months of summer.

The zoölogical resources of Beaufort have not escaped the attention of American naturalists, and there are few places upon our coast, outside of New England, where more zoölogical work has been done. In 1860, Drs. Stimpson and Gill spent a season in dredging and collecting in the vicinity of Beaufort, Cape Lookout and Cape Hatteras, and an account of their work was published in *The American Journal of Science.* Dr. Coues, who was stationed at Fort Macon during the war, occupied himself for two years in collecting the animals which are found here, and he published a series of papers on the "Natural Histsry of Fort Macon and Vicinity" in the Proceedings of the Academy of Natural Sciences of Philadelphia.

These papers, which were continued by Dr. Yarrow, contain copious and valuable notes on the habits and distribution of the animals which were observed, and we found them a great help to us. These two naturalists found four hundred and eighty species of animals in the vicinity of Beaufort. Of these four hundred and eighty, two hundred and ninety-eight are vertebrates, and one

hundred and eighty-two are invertebrates. Of the vertebrates twenty-four are mammals, one hundred and thirty-three are birds, twenty-seven are reptiles, six batrachians, ninety-seven fishes and eleven selachians. Of the invertebrates, one hundred and forty-seven are molluscs, twenty-one are crustaceans. The list of vertebrates is very nearly exhaustive, and we made no additions to it, but the list of invertebrates is obviously very imperfect, and, although we made no attempt to tabulate the species which we observed, there would be no difficulty in enlarging the list twenty or thirty fold.

Among other naturalists who have spent more or less time at Beaufort, I may mention Professor L. Agassiz, Professor E. S. Morse, Dr. A. S. Packard, Professor Webster, and Professor D. S. Jordan. Professor Morse procured most of the material for his well known paper on the Systematic Position of the Brachiopoda on the sand bars in Beaufort Inlet.

I will now attempt to give a very short statement of some of the leading points in our own summer's work. Much of our time was spent in studying the development of the Crustacea, since this is one of the most important fields for original work upon our southern coast. The supply of mat·rial is almost nexhaustible, and would employ a number of students for imany years. The life history of the Crustacea is of great interest in itself, and the recent species are so numerous and diversified that there is no group of animals better adapted for studying the general laws of embryonic development in their relation to the evolution of the group.

These considerations have led us to devote especial attention to this group during this and the preceding seasons. One of the published results of the first season's work was an illustrated account of the metamorphosis of *Squilla*, a representative of a somewhat aberrant group of Crustacea. During the second season, a member of our party, Professor Birge, made a very thorough study of the development of *Panapæus*, one of our crabs, and the account of his observations, with drawings, was ready for publication several months ago. At Beaufort, we spent most of our time upon this subject, and figured more than eight hundred points in the development of various Crustacea.

Among these, I wish to call especial attention to our observations upon the development of the Sergestidae; the least specialized of the stalk-eyed Crustacea. This very peculiar group was not known to occur upon our coast until we found a few specimens of one genus at Fort Wool, and the same genus —*Lucifer*—in great abundance at Beaufort, associated with another genus which is also new to North America. As nothing whatever was known of the development of *Lucifer*, we made every effort to obtain the eggs and young, and after four months of almost fruitless labor we finally succeeded in finding all the stages of the metamorphosis, and figured them in a complete series of ninety-nine drawings. We also obtained a somewhat less complete series of figures of stages in the life history of the second Sergestid.

Our only motive in this work was the desire to fill a gap in our knowledge of crustacean development, by supplying the life history of a very interesting group of animals, but the result was found to have a very unexpected value, since it contributes to the discussion of a number of problems in general embryology and morphology, and is the most significant crustacean life history which has ever been studied.

The following are some of the more important points:

The egg undergoes total regular segmentation.

There is no food yolk, and cleavage goes quite through the egg.

There is a true segmentation cavity.

Segmentation is rhythmical.

There is an invaginate gastrula.

The larva leaves the egg as a Nauplius, and passes through a protozoan stage, and a schizopod stage.

The fifth thoracic segments and appendages are entirely wanting at all stages of development.

Another interesting group which was studied is the Porcellanidae, the least specialized of the true crabs. The adults of our American species are almost restricted to our southern waters, although the swimming larvæ are carried north by the Gulf Stream. Within the last two years two northern naturalists have studied these floating embryos upon the south coast of New England, but as they were working upon stragglers so far from home their accounts are incomplete and somewhat contradictory. Our advantages at Beaufort enabled us to contribute towards the solution of this confused subject by raising one species of *Porcellana* from the egg.

We also raised six other species of crabs from the egg, and made drawings of the more important stages of development. One of the species which was thus studied is the edible crab. Its metamorphosis has never been figured, and although it presents no unusual features, its economic importance gives value to exact knowledge of its life history.

Mr. Wilson also studied the development of one species of Pycnogonida, a group of very peculiar Arthropods, distantly related to the spiders. As he has paid especial attention to the systematic study of this group, and is now engaged in describing the Pycnogonids collected in the Gulf Stream by Mr. Agassiz, the opportunity to study them alive in the laboratory has been a great advantage to him.

Another important investigation is the study, by Mr. Wilson, of the embryology of the marine Annelids. Although the representatives of this large group are abundant and widely distributed, little was known of the early stages of their development until he procured the eggs of several species and studied them at Beaufort. This investigation has shown, among other things, that the accepted division of Annelids into two great groups, the Oligochæta and Polychæta, is not a natural method of classification. The work upon the development of marine Annelids was supplementary to an investigation which Mr. Wilson carried on last spring at Baltimore, and which he will continue this winter, upon the development of land and fresh water Annelids.

As much time as possible was given this season to the study of the hydroids and jelly-fish of Beaufort. The life histories of several of them were investigated, a thorough anatomical study of some of the most important forms was carried on, and nearly two hundred drawings were made. It is almost impossible to complete a study of this kind in a single season, but if one or two more summers can be given to the work, we have every reason to hope for valuable results; for although the North Carolina coast is the home of many species which are only found as stragglers upon our northern coast, and of other species which are not known to occur anywhere else, and of some genera and families which are new to the North American coast, this field has suffered almost total neglect.

Nearly three months of the time of two members of our party, Mitsukuri and Wilson, were given to the study of the habits, anatomy and development of *Renilla*, a compound Polyp very much like that which forms the precious coral, but soft and without a stony skeleton. The animals which form the community are so intimately bound together that the community, as a whole, has a well marked individuality distinct from that of the separate animals which compose it. The compound individuality of *Renilla* is quite rudimentary as compared with that of a Siphonophore, and as there is no trace of it in the closely allied Gorgonias, it furnishes an excellent field for studying the incipient stages in the formation of a compound organism by the union and specialization of a community of independent simple organisms. With this end in view the anatomy of the fully developed community was care-

fully studied, and the formation of a community was traced by rearing a simple solitary embryo in an aquarium until a perfect community has been developed from it by budding. During the process of development the law of growth by which the characteristics of the compound organism are brought about was clearly exhibited, and it is fully illustrated by nearly one hundred drawings.

One of the most interesting results of our work is the explanation, by Mr. Wilson, of the origin of the metamorphosis of the larva of *Phoronis*, a small Gephyrean worm which lives in a tube. Several of the most noted embryologists of Europe have studied the development of *Phoronis*, and our knowledge of its life history is due to their combined labors. Last summer Mr. Wilson reviewed the subject, and added some important points, and during the present season he has shown by the comparison of a great number of allied forms, that the very peculiar metamorphosis admits of an extremely simple explanation. The adult is sedentary and confined to its sand tube, while the larva is a swimming animal totally different in structure. The change from the larva to the adult is very rapid and violent. It occupies only a few minutes, and during the change the larva becomes turned wrong side out, so that what was internal is external. Mr. Wilson's comparison shows that *Phronis* was originally a free animal, and that the structural peculiarities which fit the adult for sedentary life in a tube are of recent acquisition. The larva has, however, retained its ancestral adaptation to a swimming life in order to provide for the distribution of the species. There must have been a time, in the evolution of the species, when the adult was imperfectly adapted to a sedentary life, and also imperfectly adapted to a swimming life, and if the development of the individual were a perfect recapitulation of all the stages in the evolution of the species, we should have, between the swimming larva and the sedentary adult, a stage of development during which the adaptation is not quite perfect for either mode of life. It is clearly an advantage for the animal to pass through this stage as quickly as possible, or to escape it altogether. The peculiar metamorphosis enables the larva to remain perfectly adapted to a locomotor life until the occurrence of the sudden change which fits it for life in a tube, and Mr. Wilson has pointed out the manner in which the metamorphosis has been acquired in order to bridge over the period of imperfect specialization. This explanation is somewhat similar to that which Lubbock has given of the origin of the metamorphosis of insects, and we may hope that the same method of investigation will throw light upon the significance of other remarkable instances of metamorphosis in the Invertebrates.

THE MATERIALISTIC ORIGIN OF THE SEXES.

By Andrew Dewar.

Materialism is yet in its infancy. Born of human learning, weaned in scientific research, and cradled in the toleration of an enlightened civilization, its advent marks an epoch in the history of humanity. Should there be fearful shadows in its progress, where loiter grim doubts and gloomy forebodings, these are only consequent to its youth, and the necessary result of the light from a sun whose slanting rays only reach us. But even as the noonday sun chases away the shadows in its splendor, so we are assured that no doctrine in these enlightened days will ever be accepted which does not in its maturity shine on the human race for true knowledge and good.

"All knowledge is our province," said Bacon, and we would be less than men if any phenomenon in nature was considered inscrutable by us, the highest outcome of Nature. Thinking thus, one of the most curious problems is that of the sexes; and the value of the doctrine of Materialism is apparent when we come to question its cause, for no natural law professes even to offer an hypothesis on the subject.

It may here be asked, what is the doctrine of Materialism? As enunciated by the most advanced physicists, it is that "Matter contains within itself the promise and potency of every form and quality of life." This, it will be correctly said, is only a statement, not a *cause*—an assumption that requires proof, not a proposition of fact which may be demonstrated with the facility of a problem in Euclid. Granted; but it will be admitted that if we can show how the sexes originate from matter and its inherent properties, Materialism must be more than an assertion. This without further introduction we now propose to discuss.

Taking matter and its properties as the only foundation we can build on with safety, we ask What is Matter?

After long years of experiment and failure we answer this question with a firm assurance in several things:

First. The Indestructibility of Matter. This involves both the eternity of matter and the eternity of the properties of matter. Nothing exists outside of matter. Nothing but matter and its properties exist. Nothing can be taken from matter, nothing can be added to it. Whatever properties matter may have had, matter must have now; and, *vice versa*, whatever properties matter has now, matter has always had.

Secondly. Matter is composed of elements of which sixty-four are known. Everything consists of those elements, their combinations, changes, and properties. Whatever form they take now, under similar circumstances they would either in the past or future also assume.

This is the foundation of Materialism, and so far as it goes is perfectly clear and logical. Presuming that no force exists outside of matter, the *properties of matter* must account for every phenomenon in matter, and should they fail the premises fail also, and the fact is made certain that a force exists outside of matter, and *ergo* that Materialism is dead.

What, then, are the properties of Matter?

Here there is confusion and disagreement. Gravitation, cohesion, and chemical attraction are the three forces which have been popularly supposed to control matter; but when Huxley pertinently asked what these forces are, he found them not forces at all, but mere names or effects of a cause or causes unknown. Even Evolution, from which so much was expected and preached, has fallen into disgrace, and proved to be no force or cause either, but merely an "orderly sequence of phenomena" from some cause or causes unknown. How are we, then, to discover those unknown causes? If Materialism be true, they must exist; but Materialism cannot be maintained as a doctrine until we show that they do exist and what they are.

We are thus led back to our premises again—to matter and the elements—and we say, according to materialistic doctrine, if sex exists in matter now, sex must always have existed. Consequently, if matter was once a sheer chaos, or, as the most daring of physicists assert, a universal firemist, then sex in some form or another existed in that chaos or in that mist. As, assuredly, it did not exist in the form of any kind of life we are acquainted with, we are led to ask *if matter does not contain within itself some inherent sexual or dual qualities*. If it does, Materialism is alive; if not, Materialism is dead.

Matter is composed of sixty-four elements, more or less; are these elements all alike in kind, or can we trace a sex or duality in them? Fortunately for our doctrine we can. Although stated by eminent chemists to be of no importance, and made "solely for the sake of simplicity," the elements have long been divided into metallic and non-metallic classes. All the elements belong either

to one class or the other. So far success seems to favor us. Doubt is the mainspring of progress, and this doubting of a fact which has long been maintained to be of *no importance* may be the key to open up unknown vistas of research.

It will, however, be conceded in a matter of no importportance that this dual classification may be incorrect. This we believe to be the case, for one very important element—hydrogen—is given in every classification among the non-metallic elements, while the element itself is admitted to be metallic; a strange and incomprehensible misplacement. Whether the others are right or not only extensive experiments will determine. With this rectification, however, they are so far correct that the movements of Nature are opened to us as by a miracle. The lock cleared of this obstruction opens readily to the key, and Materialism rules triumphant. We seem premature; how does the duality of the elements solve all mysteries?

The object of this paper was to prove the materialistic origin of the sexes—that sex had its origin in matter. That matter is dual is part confirmation of it, but, like its antitype, we must also prove dual matter to be productive. Two females will not produce, neither will two males. If a production can be formed from the non-metallic elements only, or metallic only, then our theory is false; production should only ensue from the connection or interaction of opposite sexes and elements. Chemical analysis in this particular shows that we are right. *No natural production can be found containing the elements of only one class;* both metallic and non-metallic are essential to a formation. In simple laboratory experiments the opposite elements will combine readily with one another, while combination cannot be produced among the elements of either class alone. Even the simplest natural productions, such as air and water, are of dual combinations. Air composed of oxygen, nitrogen, carbonic acid gas, *hydrogen*, etc. Water, composed of oxygen and *hydrogen*, is the great medium also of life and production. Even the old *element*, fire or combustion, can only be produced from oxygen and *hydrogen*, with other opposing dual elements. All rock formations, crystals, stratas, are produced from combinations of the dual elements. All plants and vegetation are of dual formation and dual in sex, while all animals are undoubtedly male and female.

Our premises being thus clear and true regarding the elements of the matter, it follows that—as all plants and animals are composed of the same elements, of oxygen, hydrogen, etc., in different proportions and combinations—the conclusion we have been seeking is inevitable, namely, *sex in either animal or vegetable life is derived from and had its origin in the duality of matter.*

What causes dual matter to combine and be productive would lead us into another inquiry as to the origin of life from matter; but this we reserve for future consideration.—*Journal of Science, England.*

THE MECHANICS OF BIRD-FLIGHT.

The mechanics of the flight of birds have been much studied, and considerable space has been recently given to the subject in the columns of the *English Mechanic.* A new contribution has been recently made to a Silesian Society by MM. Legal and Reichel, whose researches deal with the relations of the size of the muscles of flight, and the size and form of a wing-surface, to the power of flight, and a short account may be of interest. (An abstract of the authors' observations appears in a recent number of *Naturforscher.*)

The authors begin by considering the question, whether the absolute size of a bird is of importance with regard to its flying power, *i. e.*, whether two birds, which considerably differ in size from each other, but are geometrically similar in their whole bodily structure, fly equally well. The final answer to this is (as we shall see) a distinct negative. The authors have measured in a great number of birds, the weight of the muscles of flight, especially the most important of these, the great breast muscle, as also its antagonist, the wing raising *musculus subclavius*, and compared it with the body-weight. The ratio of weight of the right and left large breast-muscle to the body-weight varied in the different bird species that were examined, from 1 : 3.4 in the pigeon, to 1 : 10.5 in the gull. But if the bird-species are arranged according to the amount of this quotient, neither the equally good flyers come together, nor birds of equal absolute size; *e. g.*, the partridge stands pretty well forward in the series with 1 : 4.8, near and before the hawk 1 : 5; while the sparrow, stork, and eagle, stand with about 1 : 6 near one another. Certainly, with increasing body-weight, the muscular system concerned in flight does not become relatively greater. The size of the muscles of flight is only one factor in flying-power.

A second, and very important factor is the surface presented by the outspread wing (the wing-surface); and here, again, it is not immaterial in which direction the surface extends. With equal wing-surface, a long narrow surface has more effect than a short and broad one, as a long rudder is more powerful than a short one. The authors have therefore given drawings of the form of the outspread wings for 37 different bird species, and indicated in figures the surface and length (wing configuration). A calculation of the mechanical action showed that where the ratio of the surface and length of the wing to the size of the bird remained the same, the angle of the wing motion and the angular velocity of the wing also remain the same; also that with the absolute size of the bird the air-resistance against the wings increases in the fourth power, but the body-weight only in the third. In order to compare the significance of wing-configuration for flight in large and small birds, one must therefore introduce into the numbers, expressing wing-configuration, a correction according to the absolute size of the bird, and the result of this correction the authors name the wing-number. Now, if the various birds be arranged in series according to wing-number, *i. e.*, according to wing-configuration, with comparative preference of the smaller, the good flyers are found to be at one end of the series, the bad at the other, *e. g.*, partridge 4, wild duck 10, jackdaw 20, sparrow hawk 24, sea-swallow 50. If we now multiply this wing-number with the ratio of the weight of the breast-muscle to the body-weight, *i. e.*, combine the consideration of the actual wing-configuration with that of the relative size of the muscles of flight, which are related to the effectiveness and velocity of wing-beat, we obtain the flight number as measure of the flying power, and this appears, *e. g.*, as follows: Sparrow 0.43, partridge 0.48, wild duck 0.98, jackdaw, 1.72, gull 2.15, kibits 2 92, eagle 2.95, stork 2.97, sea-swallow 3.28.

A comparison of the series thus obtained with the actual flying power, shows that the flight-number in general rises and falls with the flying power and in particular corresponds the better where birds of equal body-size are considered; and less well, the more different the size of the birds compared, so that in larger birds the actual flying power falls behind the comparative flight-number; that even appears, *e. g.*, from a comparison of the partridge with the sparrow. Or conversely, when we compare birds of equal flying power, but different size, *e. g.*, larger and smaller, but adult examples of a species, or species of a genus, the flight-number increases with the body-size. It is indeed difficult and always somewhat erroneous, to measure the actual flying powers of different birds together, one bird accomplishes more in dexterous and quick movements, another in rapid flight in a short time, a third in duration of flight. Still, the result may in general (says the reporter), be regarded as correct. Now, as the flight-numbers express the combined mechanically measurable factors of flight, it follows tha.

with the absolute size of the bird, some flight-hindering element not yet therein contained, increases. We might therefore put the question, whether equally rapid, and (comparatively) equally great contraction in a small bird. In fact, too, it is chiefly the larger birds that present the phenomenon of soaring, a condition in which, the body being maintained at the same height for a certain time, muscular work is saved by special arrangements. If soaring be an advantage, it must, in alternation with periods of active rise by means of rudder-like mechanism, be extensively utilized for the problem of a flying machine.

COLOR RELATIONS OF METALS.

In a paper on the color relations of copper, nickel, cobalt, iron, manganese, and chromium, lately read before the Chemical Society, Mr. T. Bayley records some remarkable relations between solutions of these metals. It appears that iron, cobalt, and copper form a natural color group, for if solutions of their sulphates are mixed together in the proportions of 20 parts of copper, 7 of iron, and 6 of cobalt, the resulting liquid is free from color, but is gray, and partially opaque. It follows from this that a mixture of any two of these elements is complementary to the third, if the above proportions are maintained. Thus a solution of cobalt (pink) is complementary to a mixture of iron and copper (bluish green); a solution of iron (yellow) to a mixture of copper and cobalt (violet); and a solution of copper (blue) to a mixture of iron and cobalt (red). But, as Mr. Bayley shows, a solution of copper is exactly complementary to the red reflection from copper, and a polished plate of this metal, viewed through a solution of copper salt of a certain thickness, is silver-white. As a further consequence, it follows that a mixture of iron (7 parts) and cobalt (6 parts) is identical in color with a plate of copper. The resemblance is so striking that a silver or platinum vessel covered to the proper depth with such a solution is indistinguishable from copper.

There is a curious fact regarding nickel also worthy of attention. This metal forms solutions, which can be exactly simulated by a mixture of iron and copper solutions; but this mixture contains more iron than that which is complementary to cobalt. Nickel solutions are almost complementary to cobalt solutions; but they transmit an excess of very yellow light. Now, the atomic weight of nickel is nearly the mean of the atomic weight of iron and copper; but it is a little lower, that is, nearer to iron. There is thus a perfect analogy between the atomic weights and the color properties in this case. This analogy is even more general, for Mr. Bayley states that in the case of iron, cobalt, and copper, the mean wave length of the light absorbed is proportional to the atomic weight. The specific chromatic power of the metals varies, being least for copper. The specific chromatic power increases with the affinity of the metal for oxygen. Chromium forms three kinds of salts. Pink salts, identical in color with the cobalt salts; blue salts, identical in color with copper salts; and green salts, complementary to the red salts.

Manganese, in like manner, forms more than one kind of salt. The red salts of manganese are identical in color with the cobalt salts, and with the red chromium salts. The salts of chromium and manganese, according to the author, are with difficulty attainable in a state of chromatic purity. He thinks these properties of the metals lead up to some very interesting considerations.

Fire and Water-Proof Paper.—A mixture is made of two-thirds ordinary paper pulp, and one-third asbestos. The whole is then steeped in a solution of common salt and alum, and after being made into paper is coated with an alcoholic solution of shellac.

DETECTION OF STARCH-SUGAR MECHANICALLY MIXED WITH COMMERCIAL CANE-SUGAR.*

By P. Casamajor.

In a previous communication on the same subject,† read before the American Chemical Society at the meeting of March, 1880, I gave several processes for the detection of starch-sugar in commercial sugars. One of these consisted in adding to the suspected sugar a quantity of cold water, somewhat less than its own weight, and stirring the mixture for a few seconds. If starch-sugar is present, it will be seen in the shape of white chalky specks.

Quite lately a sample of yellow refined sugar was given to me which was supposed to be adulterated by being mixed with starch glucose. By applying the test just mentioned, there seemed to be left a few small chalky specks, which dissolved after standing a minute or two, making it very uncertain whether any starch glucose was present. Upon repeatedly trying the same test the result was always doubtful.

I was then lead to treat the suspected sugar by a liquid capable of dissolving sugar, but without any solvent action on starch-glucose. After many trials, I found that methylic alcohol of such density as to mark 50° by Gay-Lussac's alcohometer answered the purpose very well, if previously saturated with starch-sugar, as this solution dissolves cane-sugar, either white or yellow, very readily, but does not dissolve starch-glucose.

Methylic alcohol at 50°, saturated with starch-sugar, gives a solution of specific gravity = 1.25. 100 c.c. of methylic alcohol at 50° dissolves 57 grms. of dry starch-sugar, the volume of the solution being 133 c.c. A solution of starch-sugar in ethylic alcohol does not answer so well, because ethylic alcohol does not dissolve so readily the gummy matters found in soft sugars, which are those generally chosen for adulteration with glucose.

To test the presence of starch-sugar in a commercial cane-sugar, the suspected sugar should, in the first place, be thoroughly dried, as otherwise any water present will weaken the alcohol, and enable it to dissolve more starch-sugar. It should then be stirred for about two minutes with the saturated solution of starch-sugar in methylic alcohol. After this, the residue is allowed to settle, and the clear solution poured off. The residue may then be washed with a fresh quantity of the same solution. After stirring again and allowing the residue to settle, there will remain, if any starch-sugar is present, a certain quantity of chalky white specks, accompanied by a fine deposit, formed by the starch-sugar present in power of fine grains. These finer particles are never seen when water is used for detecting the presence of starch-sugar, as they dissolve in water very readily. It seems probable that by using this solution of starch-sugar in weak methylic alcohol, the starch-sugar in an adulterated sample could be estimated quantitatively by a process somewhat analogous to that of Payen for estimating cane-sugar.

Not having had any occasion for such a process I have not determined experimentally the degree of approximation obtainable in this way.

The methylic solution of starch-sugar should be poured on a filter, after it has dissolved all it can from a commercial sugar, and the residue should be washed out with the same solution, and everything poured on a weighed filter. After all the liquid has run off, the filter and the residue may be rapidly washed with the strongest methylic alcohol fuund in commerce, which tests 92½° by Gay-Lussac's alcohometer, and which dissolves starch-sugar with great difficulty.

By a dexterous use of this process it seems probable that very approximate results may be obtained, although what is said here is merely in the nature of a suggestion to those who may have use for quantitative results.

* A paper read before the American Chemical Society, Nov. 4, 1880.

† *Chemical News*, vol. xli., p. 221; *Journal of the American Chemical Society*, vol. ii., p. 111; *Sugar-Cane*, vol. xii., p. 283.

ASTRONOMY.

THE LATE PARTIAL ECLIPSE OF THE SUN, AND PENNULE'S COMET.

To the Editor of "SCIENCE:"

Though rather late in the day, I send the results of our eclipse observations on the morning of December 30 and 31: I observed the last contact with the diffraction spectroscope attached to the 9½ inch equatorial. The observation was made through the C line, the slit being *tangential* to the limb at the point of contact, and somewhat widely opened. Although the air was very unsteady, and the seeing simply "horrible," yet the instant of the moon's leaving the limb of the sun, as shown by the sudden reappearance of the chromosphere, was well marked. The time was $20^h\ 49^m\ 51^s.0 \pm 0^s.5$, Princeton mean time, or $20^h\ 40^m\ 16^s.5$ Washington mean time.

Mr. McNeill, with a telescope of 3 inches aperture and power of about 40, lost sight of the moon at $20^h\ 49^m\ 36^s$ P. M. T., 15 seconds earlier.

I may mention in this connection that Pennule's comet, as observed here December 18th, 19th and 22d, showed *two* faint tails. One of them was directed, as usual, very nearly opposite to the sun. The other was pointed roughly towards the sun, though deflected some degrees toward the north; the two streamers made an angle of about 150° with each other. Each was about 30′ long on the 18th, and neither was seen after the 22d.

C. A. YOUNG.

PRINCETON, N. J., January 12, 1881.

To the Editor of "SCIENCE:"

Mr. Edwin F. Sawyer has given a very interesting description ("SCIENCE" No. 19, p. 236), of the large bolide of October 25, and the special meteor stream, to which it probably owed its origin, is one which merits prominent notice from the fact that it supplies fireballs of the largest type.

I have collected accounts of no less than 26 bolides, seen during the interval October 26 to November 9, within the last 15 years, which distinctly radiated from this remarkable shower near ε Arietis.

I saw a large meteor belonging to it on Oct. 30, ($9^h 50^m$), 1880. While engaged in telescopic observation I was somewhat startled by two prolonged brilliant flashes, which caused me to turn quickly and I saw at once a very intense meteor streak projected on the sky just S. of α Arietis. It was broken in the middle and endured 25 seconds. Its position was from $38°+18°$ to $26°+22°$.

I received a letter the following day. from Mr. I. Baxendell, F. R. A. S., of Southport, saying he had observed a large meteor on October 29, at $9^h\ 50^m$, with a path from $31°—1\frac{1}{2}°$ to $16°—17°$. The time agreed exactly with that recorded at Bristol, and the two paths gave the radiant at $46°+15°$, which agrees fairly well with that of the notable shower alluded to by Mr. Sawyer.

In further confirmation I may add that on November 1, $10^h\ 50^m$, Mr. H. Corder, of Chelmsford, observed a bright meteor = Jupiter, which had an apparent path from $275°+56°$ to $257°+43°$ and obviously took its departure from the same radiant as that of the fireballs of October 25 and 30.

W. F. DENNING.

ASHLEY DOWN, Bristol, England.

The "Report of the Kew Committee for the year ending October 31, 1880," contains some interesting information connected with an institution which is engaged in a department of research not, as yet, covered by any observatory in this country. The work at Kew is divided into seven sections:—Magnetic observations; Meteorological observations; Solar observations; Experimental in connection with either of the above departments; Verification of instruments; Aid to other Observatories; Miscellaneous.

The Magnetic observations, embracing the automatically registered curves of the Magnetograph, and observations of Declination, Dip, Deflection and Vibration, seem to indicate the approach of a more disturbed period than has occurred for several years. In order to collect more accurate data relating to this subject, arrangements have been made with other magnetic observatories in different parts of the globe to carry on a series of synchronous observations, and the comparison of the results will probably throw some light upon the laws which govern many of these phenomena. In the Meteorological department, self-recording instruments for the continuous registration, respectively, of atmospheric pressure, humidity, wind (direction and velocity) and rain have been maintained in regular operation throughout the year, in addition to standard eye observations made five times daily for the control of the automatic records. Abstracts of the meteorological results are published weekly.

Observations of the sun were made on 246 days, and on only 27 of those days was the sun's surface found to be without spots. A complete copy of the solar drawings made by Schwabe between 1825 and 1867 having been obtained, the Observatory has now in its possession a complete record of the condition of the sun's surface from November, 1825, to the present date. Transit observations of the sun have also been obtained at intervals to correct the local time.

The Experimental department embraces work upon a "Winstaneley's Recording Radiograph," for registering the amount of radiation from the sky, a "Glycerine Barometer," a "Standard Air Thermometer," and various other instruments. A large number of meteorological instruments have been verified and their constants determined for other Observatories and for instrument makers, and facilities for study and experiment have been furnished to a number of individuals interested in the various branches of the institution.

The new observatory which is being erected at Nice under the auspices of the *Bureau des Longitudes*, will probably cost over two million francs. The buildings are partly finished, and Thollon has already done some excellent work there, in spectroscopy. Besides a small equatorial, a meridian circle, and accessory instruments, there is to be a large equatorial of 29.9 in. aperture and 59 ft. focal length, constructed by M. M. Henry, of the Paris Observatory.

W. C. W.

WASHINGTON, D. C., *January* 12, 1881.

THE OBSERVATORIES OF THE UNITED STATES,

I.

CARLETON COLLEGE OBSERVATORY, NORTHFIELD, MINN.

The United States is fortunate in possessing a greater number of well equipped astronomical observatories than any other country in the world. These are distributed over a wide extent of territory, ranging from the shores of the Atlantic to the Pacific coast, and extending from the tropical regions of the Gulf of Mexico, to Lake Superior on the North.

A brief description of some of these Observatories and the appliances at their command may be of interest to our readers, and we propose on this occasion to offer some interesting facts regarding one which has been more recently organized.

The course of instruction in Astronomy at Carleton College, Northfield, Minn., appears to be well organized, and, although the College was fully organized so recently as 1874, it appears to have a well equipped astronomical observatory and every requirement for teaching Astronomy. We are informed by Professor W. W. Payne, in

charge of the Observatory, that the instruments in use are a Clark equitorial telescope, focal length 10½ feet, aperture 8¼ inches; a portable equatorial made by John Byrne, of New York, aperture 4.3 inches; a Howard sideral clock; a Howard mean-time clock, a Bond sideral chronometer, a Fauth transit instrument with telescope of 3 inches aperture, a Clark chronogragh; meteorological apparatus, and a complete set of Johnson's large astronomical maps, recently imported. By courtesy of Lieut. Edw. Maguire, Chief Engineer of the Department of Dakota, the Observatory has also the use of an excellent zenith telescope for special work.

The time of the Observatory is the standard for the State of Minnesota and parts of those States adjoining, and given to the railroad companies daily by telegraph. The distribution of the time of the Northfield meridian by the aid of excellent instruments, is said to be easy, exact and reliable.

The object of erecting this Astronomical Observatory appears to have been three-fold. 1. To give instruction to undergraduate students. 2. To offer opportunities for a complete course of study in Theoretical and Practical Astronomy. 3. To aid in useful investigations.

ON THE LIMIT OF PLANETARY STABILITY.

By Professor Daniel Kirkwood.

Laplace, in his *Système du Monde*, pointed out the limit at which, according to his estimate, the moon's attraction could have retained an elastic atmosphere.* The question of a satellite's stability was also considered by the late Professor Vaughan, of Cincinnati.† I have seen no attempt, however, to obtain for the different members of our system any definite numerical results. In the present paper it is proposed to find the approximate limits of stability in the cases of the eight major planets and certain of the satellites, on the hypothesis that their primitive condition was either liquid or gaseous.

Let M = the mass of the larger or central body,
m = that of the dependent planet or satellite,
x = the distance from the centre of the former to the limit of stability of the latter,
a = the distance between their centres; then, since the disturbing or separating force of the larger upon the smaller mass is the difference between the attraction of the former on the nearest point of the surface of the latter and that on its centre of gravity, we have

$$\frac{M}{x^2} - \frac{M}{a^2} = \frac{m}{(a-x)^2}, \qquad (1)‡$$

or putting $a = 1$ and reducing,

$$x^4 - 2x^3 + \frac{m}{M}x^2 + 2x = 1. \qquad (2)$$

If we adopt the masses and distances given in Newcomb's Popular Astronomy and solve equation (2) for each of the eight principal planets we shall obtain the distance from the centre of each to its limit of stability, as given in the second column of the following table. If, moreover, the planets, with their present masses, be reduced to the sun's mean density their radii as stated in the third column are found by the formula

$$r^n = 430{,}000 \left(\frac{m_n}{M}\right)^{1/3},$$

and the respective ratios of the limits of stability to these radii are seen in column fourth.

* Syst. du Monde, B. IV., Ch. X.

† Pop. Sci. Monthly for Sept. 1878. See also the Proc. of the A. A. A. S. for 1856.

‡ We neglect the centrifugal force due to the planet's rotation, as the modification would be slight and we propose to obtain merely approximate results.

TABLE.

PLANET.	R_n	r_n	$\frac{R_n}{r^n}$
Mercury	165,165 ms	2,514 6 ms	65.7
Venus	701,746	5,719.2	122.7
Earth	1,059,386	6,242.7	169.7
Mars	764,900	2,951.1	259.2
Jupiter	37,354,287	42,335	882.35
Saturn	45,859,381	28,317	1619.48
Uranus	49,512,900	15,209	3255.51
Neptune	81,663,510	16,009	5101.10

On the assumption that in each case the mean density of the separated mass was equal to that of the central body, the sun's present radius multiplied by the respective numbers in column fourth will give the radii of the solar nebula when the planets extended to their respective limits of stability. These radii are less than the mean distances of the planets in the ratio of 1 to 1.265. This fact may have some significance in regard to the former oblateness of the solar nebula or the law of its density.

The Earth and the Moon.—For the moon, which in perigee approaches within 221,500 miles of the earth, the limit of stability is about 38,000 miles. Were the moon's density reduced to that of the earth its radius would be 916 miles, the ratio of which to the limit of stability is 1 : 41.6. The moon's least distance diminished by 38,000 miles is 183,500 miles. If our satellite originally extended to the limit, and if the moon and the earth had the same form and density, the radius of the latter was 165,000 miles.

The Martian System.—The diameter of Phobos, according to Prof. Pickering, is 5.57 miles. If its density, therefore, be equal to that of Mars the limit of stability is about two miles exterior to the surface; or, if the density be to that of the primary in the same ratio as the density of the moon to that of the earth, the limit is less than a mile from the surface of the satellite; and finally if the density were no greater than that of water the satellite, if fluid, would be unstable, the limit being actually within the surface. Since, therefore, the satellite could never have existed at its present distance in a nebular state, it must follow, if any form of the nebular hypothesis is to be accepted, that its original distance was much greater than the present. Can we find a probable cause for this ancient disturbance?

If we suppose the former period of Mars to have been very nearly one-sixth that of Jupiter the close commensurability would render the orbit of Mars more and more eccentric. The planet in perihelion would thus pass through the sun's atmosphere, or rather through the outermost equatorial zone of the solar nebula. This resisting medium would not only accelerate the motion of Mars but also in a much greater degree that of his extremely small satellite. The solar mass contracting more rapidly than the orbit of Mars would finally leave the latter moving in an eccentric path without sensible resistance.

Other Secondary Systems.—For the first satellite of Jupiter the limit is 5250 miles, or 4½ times the radius of the satellite. For Mimas, the innermost satellite of Saturn, it is less than twice the radius. The rings of Saturn, in all probability, could not exist as three satellites, the limits of stability being interior to the surface.*

The effect of preturbation in the dismemberment of comets is known to all astronomers. The nucleus of the great comet of 1880, which approached within less than 100,000 miles of the sun's surface, must have had a den-

* It has been recently shown that Bessel's mass of the ring is much greater than the true value.

sity greater than that of granite, as well as a strong cohesive force between its parts, in order to withstand the tendency to disintegration during its perihelion passage. Had the nuclus been either liquid or gaseous, or even a cluster of solid meteorites, the difference between the sun's attraction on the central and the superficial parts would have pulled the comet asunder, spreading out the fragments into somewhat different orbits, like the meteoric streams of August and November.—*The Analyst.*

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

I have much pleasure in enclosing you a copy of the particulars respecting the formation of our "Field Club," which I believe to be the third in England of similar pretentions, its original founders being Mr. Thomas Kiddie and myself, both being science students, and by profession analysts in manufactories on Coaly Tyne. We were at first inclined to restrict the Club to those of our own class, namely, Students in Chemistry, but knowing the intimate connection between all the other branches of Science and that of Chemistry, we determined to throw it open to all science students, and we are now pleased to find that our efforts have had such a successful issue so far, and met with such general approval throughout the whole district, as we have at present, after two months establishment, about 100 members, some living as far as fourteen miles from our centre; also having the countenance of fourteen gentlemen interested in scientific education as honorary members. The officers consist of students or teachers (under the Science and Art Department, London). Should you consider our club worthy a comment in your excellent journal, which latter must act as a valuable adjunct to the aims of scientific education, I shall be exceedingly obliged if you will forward me a copy to read at one of our excursions.

M. THEODORE DIXON, *Hon. Sec.*

5 BRANDLING PARK, Newcastle-on-Tyne, Eng.

[We print the above letter in the hope that it may suggest the formation of Field Clubs in the United States. The value of such organizations cannot be overrated, and we shall be glad to hear that some of our subscribers have taken the initiative in such an agreeable enterprise. We shall send information to those who desire it.—ED.]

CHEMICAL NOTES.

THE MARQUIS TOMMASSI has succeeded in sending a message across the Atlantic with two Minotto elements.

AN APPLICATION OF ACCIDENTAL IMAGES.—J. Plateau, from some experiments performed by his son, concludes that the *apparent* distance of the full moon is only 50 metres from the observer.

PROPAGATION OF LIGHT.—M. Gouy has shown that there is not, for a given homogeneous source, a determined speed of light independent of the manner in which the amplitude is caused to vary.

PHYLLOXERA IN FRANCE.—It appears that more than a third part of the vines in France have been already destroyed by the phylloxera. The departments of Haute Savoie and Jura are now attacked.

SPONTANEOUS OXIDATION OF MERCURY AND OF METALS.—Mercury, as well as iron, zinc, cadmium, lead, copper, and tin, undergoes on exposure to the air a superficial oxidation, very slight, and restricted by the difficulty of renewing the surfaces and by the want of contact which results from the layer of oxide formed at the outset. For the oxidation to continue this layer must be constantly removed, as in the case with rust of iron formed in moist air, or for each hydrocarbonate produced in distilled water.—M. BERTHELOT.

WINES MIXED WITH GRAPE SUGAR.—The non-fermentable part of the grape sugar which is introduced into wines, if administered to dogs by way of subcutaneous injection produced vomiting and other morbid symptoms. A. Schmitz claims that these residues contain a poison similar to that present in potato-oil.

ACTION OF PHOSPHOROUS UPON HYDRIODIC AND HYDROBROMIC ACIDS.—With hydriodic acid and white phosphorous the latter melts and becomes covered with a reddish layer of biniodide, while phosphonium iodide sublimes. With red phosphorous even at 100°, there is produced merely a small quantity of phosphonium iodide. Upon dissolved hydrobromide acid, phosphorus does not react in the cold. At from 100° to 120°, phosphonium bromide sublimes, but no phosphorous bromide is produced.—A. DAMISEAU.

THE SOCIETE D'ENCOURAGEMENT POUR L'INDUSTRIE NATIONALE has awarded the Le Blanc prize of 1000 francs for the utilization of manufacturing refuse to M. Vincent, for his process for obtaining methyl chloride from the *vinasses* of the beet-root sugar manufacture. A sum of 1000 francs has also been awarded to M. J. A. Martin for his mixtures for rendering textile articles, paper, &c., uninflammable. His ordinary mixture for light goods is: Pure ammonium sulphate, 8 kilos; ammonium carbonate, 2 kilos, (5); boric acid, 3; pure borax, 2; starch 2 (for which may be substituted 0.400 kilo. dextrine, or the same weight of gelatine), and water 100 kilos. A silver medal has been awarded to M. Idrac for his process of drying timber.

A NEW ELECTRIC PROPERTY OF SELENIUM, AND THE EXISTENCE OF TRIBE-ELECTRIC CURRENTS PROPERLY SO-CALLED.—R. Blondlot has observed a new electric property of selenium which may be shown by the following experiment: To one of the poles of a capillary electrometer there is attached, by means of a platinum wire, a fragment of selenium which has been recently heated, and to the other pole a platinum foil. If the selenium is brought in contact with the platinum, holding it by means of an isolating handle, the electrometer remains at zero, as might be expected from the symmetry of the circuit; but if the selenium is rubbed against the surface of the metal the electrometer deviates strongly, the deviation obtained being equal to that produced by a sulphate of copper element.

ANALYSIS OF SUPERPHOSPHATES.—In acting upon a superphosphate made of bone-black or from the phosphate of Caceres with a solution of ammonium citrate of sp. gr. 1·09, there is no occasion to take into account the time of action or the fluctuations in the temperature of the laboratory. In the analyses of a bone-black superphosphate, an excess of citrate must be avoided—20 c.c. are sufficient for 2 grms. of the sample. An excess of the reagent dissolves part of the phosphoric acid of such tricalcic phosphate as has escaped the action of sulphuric acid in the manufacture of the superphosphate. The phosphate of Caceres is much less sensitive to the action of the citrate than the phosphate of bone-black, and here from 20 to 100 c.c. may be taken to 2 grms. of the sample.—L. CHEVRON.

DETERMINATION OF CHICORY IN GROUND COFFEE.—M. Prunier suggests the following method: Two grms. are weighed out and separated from the finer powder by sifting through fine silk. This powder which, as microscopic examination proves, is composed entirely of pure coffee, is set aside. That which remains on the sieve is macerated with a few grms. of water in a test glass. After some hours it is thrown upon a piece of cloth stretched out and crushed with the fingers. The grains of coffee resist the pressure, whilst those of chicory, reduced to a paste by soaking in water, penetrate into the cloth and adhere to it. On drying the cloth it is easy to detach the coffee, which, after dessication at 100° and addition of the fine powder separated at first, gives the weight of pure coffee. The chicory is calculated as loss.

SCIENCE:

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Lieutenant Schwatka still remains a prisoner in his quarters on Governor's Island in New York harbor, in consequence of his recent accident. Surrounded by many of the trophies of his arctic experiences, he relieves the monotony of his situation by preparing for the press his forthcoming history of the expedition, with which his name must be forever associated.

Any reference to arctic expeditions at this moment naturally recalls to mind the fact, that a brave American officer and his crew are now locked in the firm embraces of the frozen solitudes of that desolate region, heroically struggling to accomplish a service to humanity.

The gallant De Long may be safe in winter quarters with the Jeannette, waiting for the moment when a channel may be opened for his ship's return; but all past experiences in the polar regions suggest that none but the over-sanguine should rely on such a fortunate conclusion of the voyage, and the common instincts of humanity demand that a relief expedition be immediately organized, to sail at the earliest possible moment to carry succor to De Long and his party, and to report on his condition.

There are other reasons for immediately sending aid to the Jeannette; the attempt to reach the North Pole entails a colossal task, and it is perhaps vain to expect any expedition to reach it by a sudden and unexpected stroke of success; probably nearly four hundred miles of sleigh travelling over rugged and almost impassable hummocks of ice will have to be accomplished at an average speed of six to eight miles a day; this would occupy fifty consecutive days, and then, if all went well, would come the return journey with equal dangers and difficulties. Captain Nares pronounced such travelling impossible.

Lieutenant Schwatka has, however, shown that with better organization and different methods, the dangers of a sleigh expedition can be much reduced. Unfortunately, De Long has not the benefit of "Schwatka's" experiences, and has probably, like "Nares," harnessed his men to the sleigh and not depended upon dogs to drag it over so many tedious miles of dreary wastes. It would, therefore, appear obvious that even should the Jeannette expedition be actually safe and intact, the arrival of new supplies and general aid at the side of DeLong would be most opportune, and may even lead to accomplishing the great object in view.

Possibly some of our readers may consider that the time for sending a relief expedition to the Jeannette has not arrived, and that it may be prudent to await tidings of disaster before help is sent. We have somewhat anticipated such reasoning, but would add that the consequences of such a course in the case of the lost Franklin Expedition led to a final outlay of $10,000,000 by the English nation withnegative results.

We now know that had a relief expedition been sent immediately to the rescue of Franklin, the brave officers and crew might have been easily saved.

Lieutenant Schwatka strongly urges the necessity of sending immediate relief to the Jeannette expedition, and at our request will state in our next issue some of the reasons which lead him to that conclusion. No names have been so far mentioned to take a part in this undertaking, but we trust the services of Lieutenant Schwatka may be secured, as his past experience and great success would give us hope of the best results being accomplished.

The excellent work accomplished by Lieutenant Schwatka, an officer of the United States Army, in arctic explorations, would appear to teach us one lesson, that too great reliance on Naval men reaching the North Pole unaided should not be entertained. Sailors proverbially stick to their ships, are out of their element on shore, and appear unable to cope with difficulties when away from the base of their supplies. Compare the sleigh expeditions of Nares and Schwatka, and note how differently they were managed, the former starting without necessary material, making his men beasts of burden, and failing miserably from the collapse of all his resources.

Schwatka, on the contrary, so contrived that the necessaries of life were always available. Forty dogs merrily drew his sleigh, and with the instincts of a military man he carefully husbanded his resources, and accomplished sufficient to make his expedition a memorable success.

It seems on this account possible that the two arms of the service may profitably combine in the next effort to solve the great Polar problem, for the best results may be anticipated by such united action.

Mr. H. H. Warner, of Rochester, N. Y., offers a prize of $200 in gold for the discovery of any comet during this year. The conditions are that the comet must be unexpected and telescopic, excepting the comet of 1812, and the first discovery must be made in the United States or Canada, and immediate notification telegraphed to Professor Lewis Swift, of Rochester.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

At the 191st meeting of the Philosophical Society of Washington, January 8th, the following papers were read: (1). On a Simple Method of deriving some Equations used in the Theory of the Moon and of the Planets, by Mr. W. F. Mc K. Ritter, of the Nautical Almanac office. (2). On the Orbit of Swift's Comet, by Professor Edgar Frisby, U. S. Naval Observatory.

The elements of Swift's Comet were computed by Professor Frisby from three places observed by Professor Eastman with Washington Transit Circle, on the nights of October 25th, November 7th, and November 20th. They werer ecorded in "SCIENCE," January 8, 1881.

From these elements it is readily inferred that it was moving very nearly towards the earth at the time of its discovery (October 10th) by Professor Swift. On November 8th it came very near the earth's orbit, its distance from it being then about 0.069, the mean distance of the earth from the sun. Its perihelion lies a little outside of the earth's orbit, and its aphelion a little outside of Jupiter's orbit. Its perturbations therefore must at some time become immense, but for a long period it will reach its aphelion at times when Jupiter is in a remote part of his orbit. The periodic time from the above elements is about 2178^d, or a little less than six years. Different periodic times have heretofore been deduced for this comet. Some were deduced at 11 years, others at 5½ years, and still others $11 \div 3 = 3\frac{2}{3}$ years. The period of 5½ years is undoubtedly correct, the slight discrepancy being due to insufficient data. At each alternate return to its perihelion it cannot be seen, since the earth is then in the opposite part of its orbit, and the sun is between the earth and the comet. It passed nearest to the earth about the 18th of November.

The logarithms of radii vectores and distances from the earth on the given dates are—

	log. r.	*log.* Δ.
October 25th	0.035328	9.221510
November 7th	0.029018	9.141693
November 20th	0.034557	9.119295

No theory about any periodic time was assumed in these calculations.

THE ROCHESTER MICROSCOPICAL SOCIETY.

At the annual meeting of the Rochester Microscopical Society, held on the 10th instant, the following officers were elected: President, the Rev. Myron Adams; Vice-President, H. F. Atwood; Secretary, H. C. Maine; Treasurer, Dr. C. E. Rider.

This Society now numbers one hundred and nineteen active members, and is stated to be in a flourishing condition. We trust we may occasionally hear from the Society, and that the record presented in our columns may show that a real advance in microscopical studies has been accomplished.

ELECTRIC FISH.*

By the Marchioness Clara Lanza.

The science of electricity and magnetism is clearly acknowledged to be an acquisition belonging to modern times, we might say to the last century. To the ancients this great world of ideas was completely unknown, with the exception of a few individual facts which must have appeared to them in a very puzzling light, as philosophy and physics were in a wholly powerless and perplexed condition.

If we pass over the electric phenomena of our own atmosphere, thunder and lightning, the only facts concerning electricity known to the ancients were the capacity of the magnet to attract iron, the attractive power arising between two pieces of amber when rubbed together, and the peculiar effects exhibited by electric fish.

One of the most numerous and common fish found in the Mediterranean Sea is the torpedo, which is able to eject electric shocks of such force that a man's arm has often been lamed by them. The knowledge of this fact can be traced back to the farthest antiquity. These fish are so often seen on the coasts of Italy and Greece that the effects produced by them must have led to the first experiments, made, in all probability, by fishermen and people directly inhabiting the coasts of these countries; at all events, the knowledge of this fact is much older than that relating to the magnet and amber, which certainly extends to a remote pre-historic period.

The Greek designation of these words, magnet and amber, contains no etymological relations to the qualities peculiar to these bodies, which must have appeared so mysterious to the ancients. The Greek term for magnet, *Heraklea* and *Magnetis*, denotes simply a stone found in the City of Heraklea or Magnesia—while the Greek word for amber, *Electron*, relates merely to the color of the substance. In this way it is evident that both the magnet and amber were long known to the Greeks and named by them before their peculiar physical properties were ascertained.

With the torpedo it is different. The ancient Greeks called it *Marhe*, and the verb derived from this substantive signifies to stun. In the same way the Latin name, *Torpedo*, denotes something which produces numbness and lameness. In the fish markets of Marseilles and Toulon, the torpedo is called *torpille*, and thus the word torpeur, (derived from the Latin *torpor*), is used in French to denote numbness and stupefaction. The Italian fishermen call it *Tremola* on account of the characteristic trembling sensation which its touch produces. In the Arabic *patois* of the Maltese, *Haddaila* is the name applied to an electric fish.[1] Thus we find that everywhere the name of the torpedo is etymologically allied to its electric capacity, which makes it evident that the knowledge of its peculiarities extends to the most distant period in the construction of language.

Perhaps of no more recent date is the practical and most interesting use which the inhabitants of the Mediterranean countries made of the torpedo's electric capacity, thus undoubtedly representing the beginning of electro-therapeutics. As a certain cure for headache one or more living torpedos placed upon the affected part was strongly recommended—just as at the present time a constant galvanic current is used as the most reliable manner of curing the same complaint.

Aside from this, the numerous passages in the Greek and Roman authors which relate to the torpedo and its effects are mostly of a subordinate interest. Their language, like ours, struggles to express that numb and trembling sensation occasioned by the electricity proceeding from the fish, and which we ourselves are unable to describe and simply designate by the name of "electric

* Translated for "SCIENCE" from the German of Prof. Franz Boll.

1 John Davey, *Anatomical and Physiological Researches*. London, 1839. Vol. I.

shock." We are informed that the fish makes use of this power to defend itself against its enemies, and also as an offensive weapon towards its victims. This means of defense, however, does not only take effect in consequence of immediate contact, but likewise when the fish is quite removed from all association with any object. It is well known that the shock has been clearly felt, as if by close contact, (harpoons, etc.), by fishermen who drew towards land a net in which was found a torpedo. When a stream of water was poured from a vessel upon the fish, the men's hands were powerfully affected, just as the frightful, stunning sensation is conveyed to the unsuspecting angler through the medium of his fishing line.

It is greatly to be regretted that the most celebrated natural investigator of antiquity did not turn his attention to torpedoes. Among the writings which are attributed to Aristotle only very brief mention is made of this extraordinary creature. This is the more to be wondered at inasmuch as Aristotle speaks of the torpedo in reference to something else, and appears to have a complete knowledge of its anatomy. He, therefore, knew that it belongs to a viviparous species, a fact which, in our own time, has been disputed by Cuvier.

Aristotle having failed to give any explanation of the torpedo, it is, perhaps, not astonishing that the rest of antiquity did not enter upon the subject. Indeed, among all the writings of the ancients which treat of the torpedo, there cannot be discovered the slightest attempt to throw any light upon this wonderful phenomenon and reduce it to purely natural causes. The best and most intellectual article found among ancient writings in regard to this point is from the pen of the Greek physician, Galenus, who lived in Rome about 200 years after Christ. He compares the effects of the torpedo to the working of the magnet.[2]

Besides the torpedo there is another electric fish belonging to the realm of antiquity, and in exact opposition to the former, a fresh water fish. In reality, in all the rivers of Africa, especially the Nile and its neighboring streams, from mouth to source, one of the most common fish to be met with is the ***Malopternus electricus***, the electric Silurus. In size and electric power this fish is almost equal to the torpedo, but in other respects it is totally unlike the wonderful salt water creature. It is exceedingly interesting to observe that in this fish the same etymological relations and the same remedial representations in regard to nervous maladies, are found as in the torpedo. Unfortunately we are not acquainted with the name applied to it by the old Egyptians, nevertheless, we know that from the invasion of 638, which laid the foundation of the Arabian language and culture to the present day, the fish has been called ***raâdah*** or electric fish. The Jesuit Godigno, who in the sixteenth century undertook a journey to Abyssinia, tells us that the Ethiopians made use of this fish for the purpose of "casting out devils," or in other words to cure nervous diseases.

A passage in Athenæus where the ***Marhe*** is represented among the Nile fish proves that the Greeks were aware that electric fish were found there. Yet, the electric Nile fish appears to have been unintentionally identified by them with the Mediterranean torpedo, as mistakes and misconceptions concerning them extend far into the past century.

Abd-Allatif, a physician of Bagdad who lived in the twelfth century, writes as follows in his history of Egypt: "Among the animals peculiar to Egypt, we should not forget the fish called ***raâdah*** for the simple reason that while it lives no one can touch it without experiencing an irresistible trembling sensation. This impression is accompanied by cold, numbness, a crawling feeling and lameness in the limbs, so that it is impossible for one to remain in an upright position or hold anything. This extraordinary stupefaction extends through the arm, the shoulder and entire side, however superficial and light the contact with the fish may have been. A fisherman assured me that when such a fish is caught in a net, the effect produced is distinctly felt by the man, although his hand does not touch the fish, and even remains some distance from it. When dead the *raâdah* loses this power. People accustomed to bathe in water where this fish is found say that the mere breath (?) of the *raâdah* is sufficient to produce such numbness of the body that the person affected can scarcely keep from sinking." [3]

We see that the knowledge of Arabian physicians concerning the Malopternus amounts to about the same thing as that obtained by the Greek and Roman authors in regard to the torpedo, and in both cases we are unable to discover the slightest trace of any inclination to analyse the mysterious effects of the fish.

Four hundred years later than the celebrated Arabian physician, the Jesuit Godigno, journeyed on the Nile. He speaks of the electric silurus in exactly the same terms as his predecessors, and it would be useless to mention it here in any special way, if his travels did not refer for the first time to a fact, which as an unconscious example in the study of animal electricity, deserves a place in the history of science. Godigno says:

"The Ethiopians assert (I myself have never witnessed the fact) that if a living electric fish is placed upon a heap of dead fish and allowed to move among them, the fish thus brought in contact with it are seized with an inward and inexplicable trembling to such an extent that they actually appear to be alive. The cause," he continues, "may be authenticated by those who investigate the nature of things in general, and I leave it to them to decide as to what this force of motion communicated by the electric fish to the dead ones may be."

The next author upon this theme, Francesco Redi, distinguished alike as physician, natural investigator and poet, gloriously opens the path for earnest and systematic research.

The beginning of this new era in the history of electric fish can be ascertained even to the day and hour. On the 14th of March, 1666, a freshly caught torpedo was brought to Redi, and to the examination and dissection of this one specimen we owe his masterly physiological and anatomical description of the torpedo, which no anatomist can read without the utmost admiration, and which casts the collected wisdom of antiquity into complete shade.

The most important advancement attributed to Redi, is the discovery of peculiar symmetrical organs situated on each side of the torpedo's head. To-day they go by the name of "electric organs," although the discoverer called them falciform muscles or bodies, and they were known to anatomists as such for more than a century. "It seemed to me," said Redi, in relating his experiments, "as if the painful sensations caused by the torpedo had their origin in these two falciform muscles more than in any other place." Thus the first hints were given towards the correct understanding of the fish, and the inexplicable strength of its electric power pointed out together with its peculiar organs.

Redi's suppositions were soon proved to a certainty by Lorenzini, one of his pupils, who in the year 1678 published an anatomy of the torpedo. From that period until the present day, the study of electric fish has always taken the anatomy, physiology and physics of the falciform organs for its subject, and by this means has been able to obtain deeper and more extensive knowledge. Now the torpedo is no very mysterious object, but at the same time the following question remains more or less open to discussion: How is this wonderful electric organ made, and how is the creature able to produce such extraordinary effects by means of it?

2 *Galeni opera ed. Kuehn.*—Vol. VIII., p. 421.

3 Relation d'Egypte, par Abd-Allatif, médecin Arabe de Bagdad. Traduction de M. Silvestre de Sacy, Paris, 1810.

The first attempts at solution given in regard to this inquiry were of a singularly mistaken character. It has already been said that Redi designated the electric organs as falciform muscles or bodies. This unintentional comparison of the electric organs with muscles has played an important, though by no means wholesome, part in the history of electric fish.

With that broad comprehension, that massive reproduction which subordinate intellects often accord to the master's ideas, Redi's direct successor and pupil, Lorenzini, plainly indicated the electric organs as "falciform muscles," thus entirely overlooking his teacher's apt precaution on this point. And from Lorenzini every anatomist of the following century calls these organs distinctly, and unalterably muscles, although this appellation is completely voluntary, and notwithstanding the fact that the electric organs, outwardly as well as inwardly, are totally unlike muscles of any kind.

If these organs, however, *were* muscles, it was but natural that effects analogous with those produced by muscles should be ascribed to them. There followed, consequently, a purely mechanical theory respecting the action of the organs, which was best set forth by Borelli, in 1685. His opinion was that they contracted several times in quick succession, thus giving a number of violent repulsions to the object brought in contact. This, he thought, was followed by a cramp or spasm of the same nature as that experienced by a person who strikes his elbow sharply. This theory met with universal applause. The most prominent natural investigators, Linné, Réaumur and Hallere agreed perfectly with Borelli, and we may say that until the year 1750 the idea was recognized everywhere as the only possible and complete explanation ever given.

From the time of Redi to Réaumur the investigators limited their experiments to the electric fish most accessible to them—the torpedo. The electric silurus of Africa is scarcely mentioned, and if spoken of at all, it is merely to mistake it for, or identify it with, the torpedo. However, during this period the third and last known electric fish was discovered—the electric eel (*gymnotus electricus*)—found in South American rivers, and possessing the greatest bodily dimensions and the most powerful electric properties of any.

The first news of the gymnotus reached Europe about the year 1672. Later, Alexander von Humboldt made the fish famous, by describing in his book of travels, its fierce struggles with horses. Humboldt tells us that the name "Arimua," given to the eel by the South American Indians, denotes a creature that deprives of motion. He also states that in former times the gymnotus was used as a cure for paralysis. We may judge of the tremendous violence of the eel's electric discharges by quoting a fact related by Humboldt.

"On one occasion the inhabitants of a certain town were obliged to turn a street in an opposite direction because the electric eels had increased to such an extent in the rivers that every year they killed quantities of mules which were accustomed to wade through the water heavily laden."

However, before more complete information concerning the gymnotus could reach Europe, the youthful study of electricity had undergone an important modification which was destined to bear direct influence upon the theory regarding electric fish. The discovery of the Leyden jar (1745), spread through the world, and attracted universal attention. Experiments were made in all parts of the country, and everyone was anxious to learn the effects of this new natural force by his own experience and sensations.

Under these circumstances, it is not surprising that Adamson, who had studied the effects of the Leyden jar in Paris, on becoming acquainted with the electric eel in Senegal (1751), compared the latter with the former, and remarked that the shocks could be communicated like electricity through an iron wire. Dutch investigators state the same thing in regard to the gymnotus. Its electric shocks can be conducted through a chain composed of several persons, but it is maintained that only the electric conductors transmit the shock, while insulators can touch the fish without any effect being perceived.

Nevertheless, serious doubts arose concerning the correctness of this new theory, until the year 1772, when John Walsh, an Englishman, irrefragably demonstrated the electric nature of the torpedo in a series of experiments given at La Rochelle, the old Huguenot town, in the house of Marie Seignette, the discoverer of Seignette salt. He showed simultaneously that the moment the shock occurs the back and stomach of the torpedo are differently situated in regard to the electricity. Walsh considers the "falciform muscles" as mere electric machines that are put in motion at the will of the animal. Soon afterward this old term disappeared from science altogether, and the more appropriate appellation of electric organs was bestowed in its stead. A few years later, this same Walsh undertook to make a series of experiments upon the gymnotus, several living specimens having been brought to London at his request. The conformity of the shocks was perfectly demonstrated by an electric discharge; indeed, Walsh even succeeded in causing the gymnotus to emit distinct electric sparks. Cotemporary with these experiments by Walsh, the correctness of his theory was demonstrated in another interesting manner. The eminent natural philosopher, Cavendish, sunk under water a wooden board covered on each side with tin foil, and succeeded in imitating the electric phenomena of the torpedo as demonstrated by Walsh, by simply connecting the two sides of the board with a Leyden battery. He thus showed the curved current of the water, which completely agreed with that produced by the electricity of the torpedo. Finally he demonstrated the fact that a hand thrust into the water, although not coming in contact with the fish, must yet be affected by the electric shock proceeding from it in proportion to the distance.

Investigators received a fresh impulse through the discovery of galvanic electricity and electro-magnetism. It remained to be shown, however, that the electricity produced by the fish really possessed all the distinguishing features of galvanic electricity. Alexander Bolta planned numerous experiments, which, unfortunately, were never put into execution. At the instigation of the celebrated chemist, Sir Humphrey Davy, however, one of his brothers, John Davy, performed extensive experiments in Malta upon the torpedo. He observed the diversion of the multiplicator, the magnetizing of a steel rod, visible sparks, decomposition of water and nitric acid, the reduction of iodine from iodide of potassium, and, in short, the complete register from a galvanic current to the production of physical effects. He also maintained, in regard to the direction of the galvanic current, as observed in the torpedo, that at the time of the shock the creature's back is positively situated the same as the stomach. It is to the efforts made by the first natural philosophers, Faraday, Schoenbein, Colladon, E. du Bois-Reymond, and others, that science owes the explanations given in regard to the two other electric fish, the gymnotus and the silurus. It has been ascertained that the current in the former flows from head to tail, while in the latter it takes an exactly opposite direction.

Anatomists were no less prompt by exploring the construction of the electric organs than natural philosophers were the physics. The most distinguished names in anatomical science consecrated themselves, so to speak, to this particular theme. An account of these investigations can be read in the works of John Hunter, Etienne Geoffroy, St. Hilaire, Pacini, and Max Schultze. The descriptions of the torpedo and silurus written by Paolo Savi and Th. Bilhary are masterpieces of anatomical research. Unfortunately, we are still without a like description of the gymnotus.

In order to obtain a thorough comprehension of the electric organs and their action, it is necessary to have re-

course to a third science, experimental physiology, to unite anatomy with natural philosophy, and thus make the result of one answer for the other. Whoever wishes to acquire a clear idea of the three electric fish and their organs must moreover keep the following points always uppermost in his mind:

The electric fish already mentioned as representing three different species are by no means individual fish as may, perhaps, be supposed. On the contrary, they are in form very similar to those belonging to the same families. For instance, the electric eel resembles, to an extreme degree, the common eel, the electric silurus those of its species inhabiting our rivers and lakes, and the torpedo all others of the ray family. Ancient zoology associated the electric fish with others of its kind which possessed no electric capacity whatever.

In each of these fish the electric organ is constructed in a unique and highly interesting manner. The most simple of all is that of the torpedo. This creature possesses two electric organs, symmetrically placed, one on each side. Like all fish of the ray species it is distinguished by a flat, broad body. However, while the ray usually has a somewhat pointed head, the torpedo has a remarkably wide one. This arises from the fact that in addition to the gills on each side of the head, the falciform electric organs discovered by Redi are also situated there. They extend along the body, one directly under the skin of the back, the other beneath the skin of the stomach. The dimensions of these organs is especially worthy of attention. In a torpedo of ordinary size (35 centimetres in length) the electric organ is about 11 centimetres long, the greatest width 5 centimetres, and the height, (the skin being removed from the back and stomach), 2 centimetres. When prepared, the organ is of the consistency and not unlike a gray, semi-transparent gelatinous substance.

In the electric silurus, as in the torpedo, a symmetrically placed organ is found on each side. It lies on the skin which assumes the thickness of a tough rind. With the exception of the extremities, (the tip of the head and tail), the substance of these organs is embedded in the entire skin of the fish. In the middle line of the back and stomach the two organs come in direct contact, so that we may say the body of the silurus is placed in a tube composed of the two electric organs which unite like two gutter tiles. From the ends of this tube the head and tail of the fish alone project, as the skin covering them contains no electric substance. When in a fresh condition the material composing the organs presents very much the same appearance as that of the torpedo. The size is considerable, the weight being more than one-fourth that of the entire body.

The electric organs of the gymnotus are by far the most extensive. This creature, the largest specimen of whose species measures the length of a man and the thickness of a good-sized thigh, does not possess the same strongly-developed, muscular constitution which distinguishes our common eel. Almost the whole body from the back of the head to the end of the tail is composed of electric organs which are situated along the vertebral column in two pairs, a large one above and a small one beneath. Above these organs, and aside from the vertebral column, are the muscles (reduced to very insignificant dimensions) which move the powerful body.

This, then, is the way in which the electric organs are distributed among the three fish, without causing them to differ in any other respect from the remainder of their species. Yet we must make one limited addition to this assertion.

The electric organs are all distinguished by an extraordinary abundance of nerves. To each individual organ is attached an immense number of nerve-fibres, which complete it in some remarkable way yet to be spoken of, and which must be regarded as a constituent part of the organ itself.

It is, of course, understood that these nerve-fibres (called electric nerves) are entirely wanting in the organization of the non-electric fish, and nothing analagous with them can be found in the latter's entire construction.

But we must go a step further. It is a fixed law, governing the entire vertebrate world, that every nerve shall spring from a particular point, or rather from a certain group of cells belonging to the organs of the nerve centres (brain and spinal cord). This group is called the centre of origin of these nerves. In many cases the individual nerve-fibres have been successfully traced to the separate ganglion groups (nerve cell groups) of a like number of cells. In these cells single apophyses have been well authenticated and make it evident that the latter become nerve-fibres in course of time; or rather the nerve fibres have been carefully observed as to their position in regard to the centre of origin, and thus warrant the conclusion that they unite with the apophyses of the ganglion cells.

In the torpedo the powerful electric nerves (five on each side) unite with the centre organ between the brain and spinal column, and form their centre of origin on each side in a massive lobe first described by Alexander von Humboldt, and called the lemon lobe on account of its peculiar color. Now the name has been changed to electric lobe.

A close microscopic examination has shown that this consists entirely of ganglion cells and nerve fibres. These excited special interest for some time among anatomists, as it was discovered that their dimensions much surpassed those of all other nerve fibres and ganglion cells.

Then Bilhary disclosed the fact that in the electric silurus, the numberless nerves which support the electric organ originate from the subdivision of a single collossal nerve; also that this enormous fibre springs from an equally large ganglion cell visible to the naked eye, which lies imbedded in a substance of its own not far from the upper end of the spinal column, and forms the electric centre of origin of the fish.

The nerves which constitute the electric organ of the gymnotus are unusually great in number (200—230 on each side). They are situated along the entire length of the spinal column and at each interval between two dorsal vertebra a nerve projects. Their origin has not yet been explained in a satisfactory manner, but the most probable supposition is that they spring from certain large ganglion cells which are found along the spinal column.

The above details show us that the three electric fish possess specific electric organs and electric nerves, and that the construction and situation of the central organs differs exceedingly from that of the nerves. If a closer examination is made, it will be seen that the three electric organs harmonize perfectly with the essential parts of their structure, and that an anatomical principle binds them together.

This anatomical principle is nothing more than the construction of the electric organs out of many thousands of perfect, symmetrically arranged layers, the so-called electric strata in which the nerves terminate. Apart from these layers there is no other demonstrable formula of the electric organs except blood vessels and tissues.

In the electric organ of the torpedo which lies between the paralleled flat portions of the back and stomach, the electric layers are arranged in a corresponding manner. They are distinguished by having a rough and a smooth side. The latter is turned towards the back, the former towards the stomach. The rough side is so called from the countless nerve ramifications which spread themselves on all sides, and at last become so diminutive that they appear to melt into the electric layers composed of a pithy, albumen-like substance. It is very much to the point that these thousands of layers should be identified as a summary of electro-motory units, and that the construction of each individual layer should be investigated in order to discover, if possible, the reason for the phen-

omenon. The fact that the nerves of the layers turn towards the negative flat portions of the fish when the shock takes place, appeared to give a by no means unimportant hint, and to indicate a relation existing between the disposition of the nerves and that of the shock.

With the gymnotus all the different proportions harmonize completely with each other. The electric layers are situated vertically along the body. A rough and a smooth side are quite distinct. In the latter numberless nerves are embedded. The rough side corresponds to the tail and the smooth side to the head. The same relations exist here as in the torpedo. With the gymnotus, however, the current goes from head to tail, and when the electric shock takes place, the nerves of the strata turn towards the negative pole of the fish.

This harmony between the nerves and the electric shock leads us to suppose that an effectual and universally recognized law is in question. Yet, while the whole world was expecting a like agreement in regard to the malopternus, anatomical and physical investigations of its organs showed a considerable difference existing between them and the two first described. It is true that electric layers are there, and also situated vertically along the creature's body. The nerves too, unite in the same manner and on the same side with the layers. But a rough and a smooth side cannot be distinguished here as in the torpedo and gymnotus, for the layers are not furnished with so great a number of nerve fibres. On the contrary, one single nerve fibre is implanted in the centre and corresponds with the tail of the fish, as is the case of the gymnotus. When the shock takes place, however, this is the positive and *not* the negative pole.

So far, no one has succeeded in removing this contradiction, and it is more than questionable whether the resemblance agreed upon by Pacini between the anatomical and physical relations of the electric layers, has really the value of a natural law or not. In laying down this principle an important anatomical fact is first to be considered which later years have succeeded in ascertaining. It has been said that the electric layers in the gymnotus possess an identical microscopical structure, which formerly was only known in the torpedo and silurus. Unfortunately, the gymnotus has not been successfully examined in regard to this, therefore the question concerning the physical explanation of the electric shock has not much significance.

These are the facts which Anatomy is capable of producing in regard to the structure of the electric organs, especially in all that concerns their relations with the nervous system. No less great is the number of facts ascertained by the means of experimental physiology, which first facilitated a thorough understanding of the electric organs and their import.

Before these facts are individualized, however, it is necessary to make a few general remarks concerning the physiology of the nervous system.

All the organs contained in the body with which those springing from the centre of origin of the nervous system unite, can be divided into two classes, according to the relation they bear to the nervous system.

First class: Organs with centripetal nerves (commonly called organs of sense). These are characterized by the fact that any influence directed upon them through the nerves is conducted to the organs of the nervous system, and there produces a sensation. This perception arises from the peculiar nature of the nerve in question. If the retina of the eye is attracted by anything, a sensation of light is produced, which influences the nerves of the skin. Any excitation of the organs of hearing causes a resounding sensation upon the nerves. Indeed, it is not even necessary that the corresponding organ should be directly attached at all. The same effect is produced when the nerve of the organ is cut and any excitation made upon the end which does not unite with the central organ. If the influence is directed upon the periphery end of the divided nerve which is attached to the organ of sense, no natural effect will be perceived.

Second class: Terminal organs with centrifugal nerves. These do not include the organs of sense, according to any general signification. The muscles, the organs of sight, and probably the glands, all belong here. These organs, differing so completely one from the other, bear the same general relations towards the nervous system. Every influence which effects the centre of origin, particularly one made at will by the animal in question, is conducted through the nerves of the final organ, and, according to the nature of the latter, produces muscular contraction. Here also it is unnecessary that the excitation should proceed directly from the centre of origin. The same effects appear when the nerve of the terminal organ is cut and any influence directed upon it. If the excitation be directed upon the end of the nerve in connection with the centre of origin, there is no result.

It is to the second class that the electric organs belong. They are under the direct influence of the nervous system just as other organs are. They are distinguished merely by their peculiar properties, and they develop under the controlling power of the nervous system just as the muscles contract and expand by the same means.

The proof of this analogy has been given in a most complete manner by Physiology, and we will repeat it here, or, rather, give the principal details.

If the centre of origin of a muscle or any group of muscles is excited in an animal, contraction takes place. A needle thrust into the electric strata of a living torpedo occasions an immediate electric discharge.

The same results are observed when the excitation is made upon the end of the nerve connected with a muscle instead of the centre of origin.

If an electric excitation is selected for this experiment it will be seen that every irritation, however superficial, made upon the nerve proceeding from the muscle is followed by an electric shock.

If the irritations follow each other in quick succession the convulsions will be reduced to an apparently invariable state of contraction which Physiology designates as tetanus. The same excitations directed upon the electric organ produce the same effects. This is called electric tetanus.

It has been observed that a short space of time ($\frac{1}{100}''$) elapses between the nerve excitation and the beginning of the convulsion during which the muscle remains perfectly motionless. When the nerve of the electric organ is irritated the same result occurs.

The muscles and electric organs of animals which have been poisoned by strychnine produce very interesting effects. The nature of this poison consists (as physiology expresses it), in a condition of complete reflex irritability, that is, a state in which every excitation experienced by the nerves of sense is answered by some modification of the centrifugal nerves. If, for instance, we take a rabbit or frog which is poisoned by strychnine and shake it violently or scream loudly, you will see that spasmodic muscular contraction follows each successive irritation. If an electric fish is poisoned by strychnine the same contractions are produced, followed by an electric discharge.

We must not forget to mention that the electric organs as well as the muscles are liable to fatigue, and that just as the latter lose their capacity for contraction after continuous labor, so are the former incapable of producing any effect after repeated discharges have taken place.

The immense number of concordant facts which can be proved in regard to the electric organs and muscles, has induced physiologists to assume that an especial anatomical and physiological relation exists between them, and many consider the electric organs to be muscles remodelled in some peculiar way in which the development of electricity instead of force, and the electric shock instead of contraction, had taken place by some inexplicable means. Such suppositions as these stand in direct

opposition to physiological, chemical and anatomical facts, which recognize a vast difference between these two organs.

The preference certainly is due to that mode of investigation which casts away all artificial proofs of closer relationship between the muscles and the electric organs and regards them as independent and well-authorized members. Indeed, the reasons given by those who accept an especial connection between the two are formed merely upon the identity of this relationship with the nervous system, and not upon any similarity to the actual qualities peculiar to these organs. Such grounds, however, can have no importance as regards this question and can bring the electric organs no nearer to the muscles than to the organs of sight or any other organ.

But are the electric organs really so independent and isolated in the animal organization? And to what freak of nature are we indebted for the remarkable fact that out of all the fish that exist, only three are distinguished by such powerful weapons? The theory of evolution which now rules organic natural sciences, always has a well-tried domestic remedy on hand for such questions. This theory discovers in formations like the electric organs which stand out as prominent exceptions to the conformity of animal construction, the distinct remains of a powerfully developed species belonging to an early epoch of geology, or, in other words, the solitary descendants of a once mighty family. According to this, the appearance of the electric organs in the three fish may seem much less mysterious, and the great anatomical diversities which exhibit themselves throughout these organs are, perhaps, best explained by the idea that in these fish we have before us the final issue of a powerful species, the last remains of an extinct family. That such a family did exist is proved by the discovery of a petrified torpedo in the tertiary strata of Monte Bolca in Verona.

But also in the cotemporary creation the electric organs are not so badly devoloped as a superficial observer might suppose. In the non-electric torpedo of the *Kaja* species, and also those which are found in the African rivers, peculiarly constructed organs have been discovered from which an electric effect cannot be produced, but which, nevertheless, are composed of strata similar to the real electric organs. These may, perhaps, be correctly termed electric organs, which are either newly constructed or else in a state of incomplete development.

The materials so far collected by anatomists and physiologists concerning this question do not admit of a marked decision. The organs present many things in common with the electric strata it is true, but beyond this further investigation seems useless.

In one other respect physiology is likewise unable to give a definite explanation. E. du Bois-Reymond was the first to ask how it happened that the electric fish was not the victim of its own power, and how it was possible that the forcible electric discharges which killed other fish completely escaped the electric fish itself.

Now we all know that the nerves and muscles of the electric fish are excited by means of an electric current, and a much stronger one is perhaps required here than would be the case with other animals, yet the electric discharges, although of such force, produce no effect whatever upon the fish. There are influences at work here, which so far we are unable to understand. We naturally suppose, however, that the great dimensions of the nerve fibres and ganglion cells, together with a vigorous nervous system, have a great deal to do with it.

In conclusion, it still remains for us to put the greatest question of all concerning the electric fish, namely: what is the origin of that powerful force which at the creature's will so suddenly appears and departs with equal rapidity, and also what is the precise mechanism of the electric organs?

It has been shown that as science advanced, the electric fish became better known and more carefully studied. The ancients were only aware that such a thing existed; a conviction, however, that they were incapable of analysing further. Redi taught us to consider the electric organs as the apparatus which produced the effect. E. du Bois-Reymond put the electric strata in place of the electric organs, by proving that the mechanism of the latter was reduced to the combined action of countless analogous electro-motory monads, which was explained by the supposition that when the electric discharge occurred one part of the strata was positive and the other negative. By this means our question concerning the mechanism of the electric organs is partially answered. It now remains to ask what takes place when the electric discharge occurs?

Now, in order to imitate the effects produced by the malopternus, it requires the strongest electro-motor apparatus that can be found. The natural philosopher must use the most powerful batteries contained in his labratory, if he wishes to *approach* the force which causes 2½ pounds of water, salt and albumen to come under its influence.

The muscles are no less powerful. The dorsal muscle of a frog consists of a few grammes of water, salt and albumen, and yet it is capable of lifting a kilometre. In both cases an extraordinary development is apparent, mechanical in one and electric in the other.

Hitherto, no one has succeeded in correctly establishing the facts relating to this mechanism. Nevertheless, concerning the electric eel there is an accepted theory, which explains all the phenomena in a most satisfactory manner.

This theory originated with Colladon and E. du Bois-Reymond, and states that in the electric substance, dispolary electro-moter molecules are to be found.

In a state of repose they turn towards their pole in every direction, or else in two ways opposed to each other, so that the electricity arises on all sides and disappears without. When the shock takes place, the possitive pole is turned quickly towards the electric organ whence the positive current proceeds.

OBSERVATIONS ON ICE AND ICEBERGS IN THE POLAR REGIONS.*

By Lieutenant F. SCHWATKA, U. S. N.

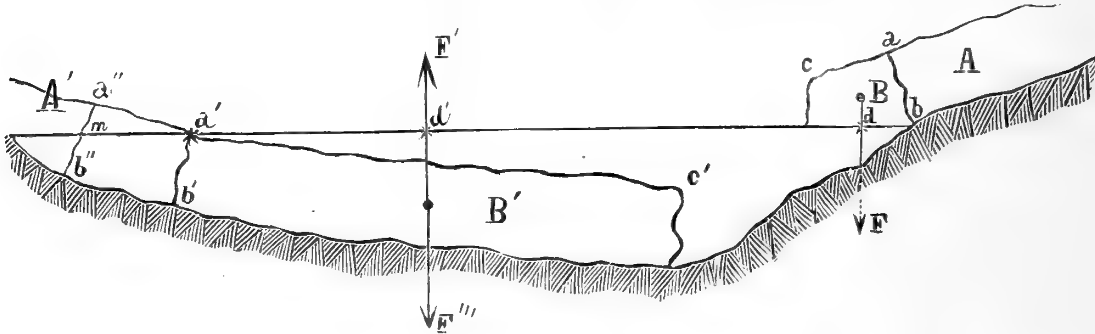
The formation of icebergs, from the terminal fronts of glaciers, has long been a disputed point among *savants*, some contending that they derive their origin from the corroding action of the water, undermining their projecting faces until the weight of the superincumbent mass, acting as a lever, overcame the cohesive power of the glacier along some line of least resistance, when the berg fell into the sea, and was wafted away by the tide-winds and currents. Others can only account for such huge mountains of ice by supposing that the glacier, slowly crawling into the sea, and plunging beneath a denser fluid, has a bouyant effort or tendency to rise, which, at last, becomes so great that it overcomes the line of least resistance, near the shore, and the berg rises into the sea, to be at the further mercy of its uncertain elements. Both theories have proved to be correct. The former generally occurs where currents, heated in more temperate climes, pour their tepid waters northward, and expend their thermal forces in contending with the vast packs, floes, and glaciers of ice, that obstruct their polar march, and whose fast corroding action has the slow glacier only a comparatively short time in its embraces before it has undermined it. The latter results where the chilled waters from the Pole have but little effect upon the glacial front; and slow as it is, it has time to crawl into the sea to give forth its mighty masses. Sometimes both

* Read before the National Academy of Sciences, New York, 1880.

kinds of forces are acting simultaneously upon the same glacier, and while huge icy mountains are at intervals of centuries rising from their dense, watery bed, other and smaller ones are more frequently dropping from its seaward face, for those formed by dropping are far smaller than those which rise into the sea, as the following diagram will serve to show. Although about seven-eighths of an iceberg is submerged, it must not be inferred that, when its height has been determined, seven times that height is its depth below the sea level. If of a tabular shape, this proportion becomes more nearly correct; but if of a pyramidal or conoidal cross section, which is far oftener the case, the lineal proportions of height to depth approach each other more closely, while the volumes, necessary to hydrostatic equilibrium, remain invariable. Their great height, as compared with their breadth shows that these lineal proportions do not obtain beneath the sea level,or the mass,if homogeneous could not be in a state of stable equilibrium, and would topple over, which sometimes happens when the conditions of equilibrium are disturbed by the unsymetrical decrease of its different faces.

The height of bergs, estimated or measured by various Arctic voyagers, varies greatly. During the warm months of summer, when they are most frequently encountered by navigators, they are often surrounded by a hazy mist, due to the condensation of the surrounding moisture by their chilly faces, and the effect is to make them appear much higher than they really are, and to render estimates of their height particularly unreliable.

As about seven-eighths of an iceberg is under water, the curious spectacle, which has often been seen in Polar latitudes, of these monsters ploughing their way against a rapid current, loaded with heavy pack-ice, and in the very teeth of a strong gale of wind, can be readily understood on the theory that the surface current is shallow, and the drifting colossus is only obeying the mandates of a deeper and more powerful agent.



ON HEAT CONDUCTION IN HIGHLY RAREFIED AIR.*

By William Crookes, F.R.S.

The transfer of heat across air of different densities has been examined by various experimentalists, the general result being that heat conduction is almost independent of pressure. Winkelmann (*Pogg. Ann.*, 1875, 76) measured the velocity of cooling of a thermometer in a vessel filled with the gas to be examined. The difficulty of these experiments lies in the circumstance that the cooling is caused not only by the conduction of the gas which surrounds the cooling body, but that also the currents of the gas and, above all, radiation play an important part. Winkelmann eliminated the action of currents by altering the pressure of the gas between 760 and 1 millim. (with decreasing pressure the action of gas currents becomes less); and he obtained data for eliminating the action of radiation by varying the dimensions of the outer vessel. He found that, whereas a lowering of the pressure from 760 to 91.4 millims. there was a change of only 1.4 per cent. in the value for the velocity of cooling, on further diminution of the pressure to 4.7 millims. there was a further decrease of 11 per cent., and this decrease continued when the pressure was further lowered to 1.92 millim.

About the same time Kundt and Warburg (*Pogg. Ann.*, 1874, 5) carried out similar experiments, increasing the exhaustion to much higher points, but without giving measurements of the pressure below 1 millim. They enclosed a thermometer in a glass bulb connected with a mercury pump, and heated it to a higher temperature than the highest point at which observations were to be taken; then left it to itself, and noted the time it took to fall through a certain number of degrees. They found that between 10 millims. and 1 millim. the time of cooling from 60° to 20° was independent of the pressure: on the contrary, at 150 millims. pressure the rate was one and a half times as great as at 750 millims. Many precautions were taken to secure accuracy, but no measurements of higher exhaustions being given the results lack quantitative value.

It appears, therefore, that a thermometer cools slower in a so-called vacuum than in air of atmospheric pressure. In dense air convection currents have a considerable share in the action, but the law of cooling in vacua so high that we may neglect convection has not to my knowledge been determined. Some years ago Professor Stokes suggested to me to examine this point, but finding that Kundt and Warburg were working in the same direction it was not thought worth going over the same ground, and the experiments were only tried up to a certain point, and then set aside. The data which these experiments would have given are now required for the discussion of some results on the viscosity of gases, which I hope to lay before the Society in the course of a few weeks; I have therefore completed them so as to embody the results in the form of a short paper.

An accurate thermometer with pretty open scale was enclosed in a 1½ inch glass globe, the bulb of the thermometer being in the centre, and the stem being enclosed in the tube leading from the glass globe to the pump.

Experiments were tried in two ways:—

I. The glass globe (at the various exhaustions) was immersed in nearly boiling water, and when the temperature was stationary it was taken out, wiped dry, and allowed to cool in the air, the number of seconds occupied for each sink of 5° being noted.

II. The globe was first brought to a uniform temperature in a vessel of water at 25°, and was then suddenly plunged into a large vessel of water at 65°. The bulk of hot water was such that the temperature remained sensibly the same during the continuance of each experiment. The number of seconds required for the thermometer to rise from 25° to 50° was registered as in the first case.

* Abstract of a Paper read before the Royal Society, Dec. 16, 1880.

It was found that the second form of experiment gave the most uniform results; the method by cooling being less accurate, owing to currents of air in the room, etc.

The results are embodied in the following Table:—

(Rate of Heating from 25° to 50 .)

TABLE I.

Pressure.	Temperature.	Seconds occupied in rising each 5°.	Total number of seconds occupied.
760 millims.	25°	0	0
	25 to 30	15	15
	30 to 35	18	33
	35 to 40	22	55
	40 to 45	27	82
	45 to 50	39	121
1 millim.	25°	0	0
	25 to 30	20	20
	30 to 35	23	43
	35 to 40	25	68
	40 to 45	34	102
	45 to 50	48	150
620 M.*	25°	0	0
	25 to 30	20	20
	30 to 35	23	43
	35 to 40	29	72
	40 to 45	37	109
	45 to 50	53	162
117 M.	25°	0	0
	25 to 30	23	23
	30 to 35	23	46
	35 to 40	32	78
	40 to 45	44	122
	45 to 50	61	183
59 M.	25°	0	0
	25 to 30	25	25
	30 to 35	30	55
	35 to 40	36	91
	40 to 45	45	136
	45 to 50	67	203
23 M.	25°	0	0
	25 to 30	28	28
	30 to 35	33	61
	35 to 40	41	102
	40 to 45	55	157
	45 to 50	70	227
12 M.	25°	0	0
	25 to 30	30	30
	30 to 35	37	67
	35 to 40	41	108
	40 to 45	58	166
	45 to 50	86	252
5 M.	25°	0	0
	25 to 30	38	38
	30 to 35	43	81
	35 to 40	54	135
	40 to 45	71	206
	45 to 50	116	322
2 M	25°	0	0
	25 to 30	41	41
	30 to 35	51	92
	35 to 40	65	157
	40 to 45	90	247
	45 to 50	165	412

There are two ways in which heat can get from the glass globe to the thermometer—(1) By radiation across the intervening space; (2) by communicating an increase of motion to the molecules of the gas, which carry it to the thermometer. It is quite conceivable that a considerable part, especially in the case of heat of low refrangibility, may be transferred by "carriage," as I will call it to distinguish it from convection, which is different, and yet that we should not perceive much diminution of transference, and consequently much diminution of rate of rise with increased exhaustion, so long as we work with ordinary exhaustions up to 1 millim. or so. For if, on the one hand, there are fewer molecules impinging on the warm body (which is adverse to the carriage of heat), yet on the other the mean length of path between collisions is increased, so that the augmented motion is carried further. The number of steps by which the temperature passes from the warmer to the cooler body is diminished, and accordingly the value of each step is increased. Hence the increase in the difference of velocity before and after impact may make up for the diminution in the number of molecules impinging. It is therefore conceivable that it may not be till such high exhaustions are reached that the mean length of path between collisions becomes comparable with the diameter of the case, that further exhaustion produces a notable fall in the rate at which heat is conveyed from the case to the thermometer.

The above experiments show that there *is* a notable fall, a reduction of pressure from 5 M. to 2 M. producing twice as much fall in the rate as is obtained by the whole exhaustion from 760 millims. to 1 millim. We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such vacua as exist in planetary space the loss of heat—which in that case would only take place by radiation—would be exceedingly slow.

*M=millionth of an atmosphere.

PROFESSOR HUXLEY ON EVOLUTION.

At a recent meeting of the Zoological Society, among the papers read was one by Professor Huxley on the application of the laws of evolution to the arrangement of the vertebrata, and more particularly mammalia. The illustrations adduced were those of the history of the horse, principally, so far as is known, from the work of Professor Marsh on the Eocenes of North America. The announcement of the paper had drawn together an unusually large attendance, as it was expected that the marshalling of the facts in Professor Huxley's hands would have great interest in practically substantiating the theory of evolution, which, though foreshadowed by others, took practical shape in the work of Darwin twenty-one years ago.

Professor Huxley began by saying:—There is evidence, the value of which has not been disputed, and which, in my judgment, amounts to proof, that between the commencement of the tertiary epoch and the present time the group of the equidæ has been represented by a series of forms, of which the oldest is that which departs least from the general type of structure of the higher mammalia, while the latest is that which most widely differs from that type. In fact, the earliest known equine animal possesses four complete sub-equal digits on the fore foot, three on the hind foot; the ulna is complete and distinct from the radius; the fibula is complete and distinct from the tibia; there are 44 teeth, the full number of canines being present, and the cheek-teeth having short crowns with simple patterns and early-formed roots. The latest, on the other hand, has only one complete digit on each foot, the rest being represented by rudiments; the ulna is reduced and partially anchylosed with the radius; the fibula is still more reduced and partially anchylosed with the tibia; the canine teeth are partially or completely suppressed in the females; the first cheek-teeth usually remain undeveloped, and when they appear are very small; the other cheek-teeth have long crowns, with highly complicated patterns and late-formed roots. The equidæ of the intermediate ages exhibit intermediate characters. With respect to the interpretation of these facts two hypotheses

and only two, appear to be imaginable. The one assumes that these successive forms of equine animals have come into existence independently of one another. The other assumes that they are the result of the gradual modification undergone by the successive members of a continuous line of ancestry. As I am not aware that any zoologist maintains the first hypothesis, I do not feel called upon to discuss it. The adoption of the second, however, is equivalent to the acceptance of the doctrine of evolution so far as horses are concerned, and in the absence of evidence to the contrary, I shall suppose that it is accepted. Since the commencement of the eocene epoch, the animals which constitute the family of the equidæ have undergone processes of modification of three kinds:—1, there has been an excess of development of one part of the oldest form over another; 2, certain parts have undergone complete or partial suppression; 3, parts originally distinct have coalesced. Employing the term "law" simply in the sense of a general statement of facts ascertained by observation, I shall speak of these three processes by which the eohippus form has passed into equus as the expression of a three-fold law of evolution. It is of profound interest to remark that this law or generalized statement of the nature of the ancestral evolution of the horse, is precisely the same as that which formulates the process of individual development in animals generally, from the period at which the broad characters of the group to which an animal belongs are discernible onwards. After a mammalian embryo, for example, has taken on its general mammalian characters, its further progress towards its special form is affected by the excessive growth of one part or relation to another, by the arrest or suppression of parts already formed, and by the coalescence of parts primarily distinct. This coincidence of the laws of ancestral and individual development creates a strong confidence in the general validity of the former, and a belief that we may safely employ it in reasoning deductively from the known to the unknown. The astronomer who has determined three places of a new planet calculates its place at any epoch, however remote; and, if the law of evolution is to be depended upon, the zoologist who knows a certain length of the course of that evolution in any given case may with equal justice reason backwards to the earlier but unknown stages. Applying this method to the case of the horse, I do not see that there is any reason to doubt that the eocene equidæ were preceded by mesozoic forms, which differed from eohippus in the same way as eohippus differs from equus. And thus we are ultimately led to conceive of a first form of the equine series, which, if the law is of general validity, must need have been provided with five sub-equal digits on each plantigrade foot, with complete sub-equal antebrachial and crural bones, with clavicles, and with, as at present, 44 teeth, the cheek-teeth having short crowns and simple ridged or tuberculated patterns. Moreover, since Marsh's investigations have shown that the older forms of any given mammalian group have less developed cerebral hemispheres than the later, there is a *primâ facie* probability that this primordial hippoid had a low form of brain. Further, since the existing horse has a diffuse allantoic placentation, the primary form could not have presented a higher, and may have possessed a lower condition of the various modes by which the fœtus derives nourishment from the parent. Such an animal as this, however, would find no place in any of our systems of classification of the mammalia. It would come nearest to the lemuroidea and the insectivora, though the non-prehensile pes would separate it from the former, and the placentation from the latter group. A natural classification is one which associates together all those forms which are closely allied and separates them from the rest. But, whether in the ordinary sense of the word "alliance," or in its purely morphological sense, it is impossible to imagine a group of animals more closely allied than our primordial hippoids are with their descendants. Yet, according to existing arrangements, the ancestors would have to be placed in one order of the class of mammalia, and their descendants in another. It may be suggested that it might be as well to wait until the primordial hippoid is discovered before discussing the difficulties which will be created by its appearance. But the truth is that that problem is already pressing in another shape. Numerous "lemurs," with marked ungulate characters are being discovered in the older tertiaries of the United States and elsewhere; and no one can study the more ancient mammals with which we are already acquainted without being constantly struck with the insectivorous characters which they present. In fact, there is nothing in the dentition of either primates, carnivores, or ungulates, which is any means of deciding whether a given fossil skeleton, with skull, teeth, and limbs almost complete, ought to be ranged with the lemurs, the insectivores, the carnivores, or the ungulates. In whatever order of mammals a sufficiently long series of forms has come to light, they illustrate the three-fold law of evolution as clearly, though, perhaps, not so strikingly, as the equine series does. Carnivores, artiodactyles, and perissodactyles all tend, as we trace them back through the tertiary epoch, towards less modified forms which will fit into none of the recognized orders, but come closer to the insectivora than to any other. It would, however, be most inconvenient and misleading to term these primordial forms insectivora, the mammals so-called being themselves more or less specialized modifications of the same common type, and only, in a partial and limited sense, representatives of that type. The root of the matter appears to me to be that the palæontological facts which have come to light in the course of the last ten or fifteen years have completely broken down existing taxonomical conceptions, and that the attempts to construct fresh classification upon the old model are necessarily futile. The Cuvieran method, which all modern classifiers have followed, has been of immense value in leading to the close investigation and the clear statement of the anatomical characters of animals. But its principle, the association into sharp logical categories defined by such characters, was sapped when Von Baer showed that, in estimating the likenesses and unlikeness of the animals, development must be fully taken into account; and if the importance of individual development is admitted, that of ancestral development necessarily follows. If the end of all zoological classification is a clear and concise expression of the morphological resemblances and differences of animals, then all such resemblances must have a taxonomic value. But they fall under three heads:—(1) those of adult individuals: (2) those of successive stages of embryological development or individual evolution; (3) those of successive stages of the evolution of the species, or ancestral evolution. An arrangement is "natural," that is, logically justifiable, exactly in so far as it expresses the relations of likenesses and unlikenesses enumerated under these heads. Hence, in attempting to classify the mammalia, we must take into account not only their adult and embryogenetic characters, but their morphological relations, in so far as the several forms represent different stages of evolution. And thus, just as the persistent antagonism of Cuvier and his school to the essence of Lamarck's teachings (imperfect and objectionable as these often were in their accidents) turns out to have been a reactionary mistake, so Cuvier's no less definite repudiation of Bonnet's "*échelle*" at the present day, the existence of a "scala animantium," is a necessary consequence of the doctrine of evolution, and its establishment constitutes, I believe, the foundation of scientific taxonomy. Many years ago, in my lectures at the Royal College of Surgeon, Is particularly insisted on the central position of the insectivora among the higher mammalia; and further study of this order and of the rodentia has only strengened my conviction that any one

who is acquainted with the range of variation of structure in these groups possesses the key to every peculiarity which is met with in the primates, the carnivora, and the ungulata. Given the common plan of the insectivora and of the rodentia, and granting that the modifications of the structure of the limbs, of the brain, and of the alimentary and reproductive viscera which occur among them may exist and accumulate elsewhere, and the derivation of all eutheria from animals which, except for their diffuse placentation, would be insectivores, is a simple deduction from the law of evolution. I venture to express a confident expectation that investigation into the mammalia fauna of the mesozoic epoch will, sooner or later, fill up these blanks.

RECENT DISCOVERIES RELATING TO THE DOUBLE STARS OF THE DORPAT CATALOGUE.

By S. W. Burnham.

The distinguished Russian astronomer, Struve, published in 1837 the results of a thorough examination of the heavens for the discovery of double stars between the north pole and 15° south declination. This great catalogue, *Mensuræ Micrometricæ*, included all the double stars within these limits known prior to the observations of Struve, mainly due to the researches of Sir William Herschel, and at the time of its publication presented all that was known on this subject of astronomy. The whole number of double stars catalogued and measured by Struve was about 3000. The superiority of the telescope used at Dorpat for this class of work, over the much larger reflectors employed by the Herschels, is repeatedly shown by the observations. Many of the Herschel pairs, observed with apertures from eighteen inches to four feet, were found by Struve with the 9.6-inch refractor to be really triple, one of the components being a close pair. When Sturve's great work was published, it seemed as though there was little left for subsequent observers to do except in the way of re-observing the Struve stars. So complete and systematic had been his scrutiny of the northern heavens, it was considered that new discoveries among the stars found by Struve to be single would necessarily be of rare occurrence, and particularly after the publication, in 1850, of the Pulkowa Catalogue of 500 stars, which comprised omitted stars and later discoveries, principally by Otto Struve, the successor of his father as Director of the new Imperial Observatory. This last mentioned catalogue was much more interesting, with respect to the class of stars it contained, than the other. The Pulkowa 15-inch refractor was in every respect superior to the Dorpat glass, as well as larger. Substantially all the wide and comparatively easy pairs had been collected in *Mensuræ Micrometricæ*, so that later discoveries were necessarily either very close pairs, or the components were very unequal, and, therefore, this catalogue furnishes a much larger proportion of binary and other interesting systems. In the twenty-five years following this epoch, the whole number of double star discoveries by all observers would not exceed fifty; but many important series of measures of the Struve stars were made by English, German and Italian astronomers, and this work was steadily continued at Pulkowa, resulting in showing the periods and motions of many of the more rapid binary systems, and the relations of other double stars.

That these catalogues were really very incomplete, with reference to the number of double stars actually existing, is apparent from the fact that the writer in the last ten years has discovered at least 900 new pairs, and more than half of them with a telescope greatly inferior in size to the smallest of the instruments used by the Russian astronomers. That there was left much that was new to discover in the Struve stars will appear from the number which have been again divided by later observers. In some instances, doubtless, the close pair was missed by Struve because it was single or much closer at that time, but certainly in the great majority of instances this is improbable, and the true explanation will probably be found in the improved defining power of the later refracting telescopes. For double star work more than any other, perfect definition is of the first importance. Something may be done in observing the moon, planets, nebulæ, etc., with a large instrument of poor definition, but for the discovery or measurement of close and difficult double stars it is practically useless. It should be mentioned as a fact that every star in the following table was discovered with a refracting telescope.

The follow ng list comprises all the stars of the Dorpat Catalogue where a closer component has been discovered since the observations of Struve. More than half of these

No.	Σ	Star.	Struve's Pair.	New Companion.	Discoverer.
1..	17	....	27".06	2".04	Burnham
2..	26	....	13 .29	0 .60	O. Struve
3..	39	....	19 .90	0 .40	Dembowski
4..	157	....	12 .40	0 .85	Burnham
5..	171	....	29 .69	3 .69	Burnham
6..	205	γ Andromedæ.	10 .33	0 .50	O. Struve
7..	258	....	70 .30	1 .20	Burnham
8..	318	20 Persei.....	14 .04	0 .34	Burnham
9..	366	....	48 .97	1 .99	Burnham
10..	439	....	23 .70	0 .40	Burnham
11..	610	7 Camelopardi	25 .64	1 .24	Dembowski
12..	668	β Orionis.....	9 .14	0 .2?	Burnham
13..	692	Orionis 82	34 .86	0 .48	Burnham
14..	707	....	27 .77	1 .11	Burnham
15..	721	....	24 .32	0 .46	Burnham
16..	808	....	16 .06	2 .60	Dembowski
17..	888	....	2 .83	0 .27	Burnham
18..	1019	Canis Maj. 136	37 .84	6 .12	Dembowski
19..	1026	Canis Maj. 139	17 .85	0 .48	Burnham
20..	1057	....	15 .87	0 .69	Burnham
21..	1097	....	29 .34	5 .93	Dembowski
22..	1179	....	19 .75	3 .76	Burnham
23..	1481	....	20 .20	0 .80	Burnham
24..	1516	....	7 .90	7 .61	O. Struve
25..	1780	86 Virginis (AC)	26 .94	1 .61	(AB) Burnham
				1 .72	(CD) Burnham
26..	1812	....	14 .02	0 .47	O. Struve
27..	2005	Libræ 213	28 .54	1 .47	(AB) Burnham
28..	2214	....	19 .49	1 .43	Dembowski
29..	2220	μ Herculis....	31 .09	0 .96	Alvan Clark
30..	2287	....	22 .33	1 .71	Burnham
31..	2306	....	12 .81	0 .95	Dembowski
32..	2342	....	28 .80	8 .86	Burnham
33..	2435	(AC)	10 .73	1 .43	(AB) Burnham
				2 .90	(CD) Howe
34..	2479	Cygni 4	6 .72	0 .57	Dembowski
35..	2481	....	4 .03	0 .40	Secchi
36..	2535	....	26 .31	1 .22	Dembowski
37..	2538	....	52 .81	4 .37	Burnham
38..	2539	....	5 .60	4 .78	Burnham
39..	2549	....	22 .86	1 .93	Burnham
40..	2570	....	4 .16	0 .29	A. G. Clark
41..	2589	ζ Sagittæ.....	8 .77	0 .25	A. G. Clark
42..	2607	Cygni 116....	3 .23	0 .3	O. Struve
43..	2630	(AD)	11 .30	6 .47	(AB) Burnham
				7 .75	(AC) Burnham
44..	2657	....	11 .71	0 .60	O. Struve
45..	2690	....	14 .88	0 .50	Dawes
46..	2704	β Delphini....	35 .06	0 .20	Burnham
47..	2777	δ Equulei....	37 .98	0 .35	O. Struve
48..	2793	....	26 .51	0 .56	Burnham
49..	2815	....	7 .50	0 .90	Dembowski
50..	2824	κ Pegasi......	11 .76	0 .27	Burnham
51..	2959	....	13 .77	8 .31	Burnham
52..	2966	....	30 .72	0 .41	O. Struve
53..	3130	....	2 .86	0 .3?	O. Struve

stars have been discovered within the last eight years, and it is very probable that many new additions will be made as the large refractors now in use engage in this work. This list would be much extended by including stars to which more distant companions have been detected, but most, if not all of them, are too distant to make any physical relation probable, and are of very little interest. The first column gives a number for reference; the second column, Struve's number; the third, the name of the principal star, when found in Flamsteed or Bode; the fourth, the distance of the stars recorded by Struve; the fifth, the distance of the new star; and sixth, the name of the discoverer.

Many of the close pairs are known to be binaries, and in some cases it is probable the three stars form one system. When any change has occurred, the most recent measures of distance are given.

ASTRONOMY.

SWIFT'S COMET.

A new determination of the orbit of Swift's periodic comet has just been made by Mr. Winslow Upton of the U. S. Naval Observatory, based upon observations made at Washington, October 25, November 23, and December 22, 1880. No assumption was made with regard to the period of resolution or the eccentricity. The following are the elements obtained, and communicated to the *Astronomische Nachrichten:*

Epoch, 1880, Oct. 25. 5 Washington mean time.

$$\begin{array}{rrrrl} M & 357^\circ & 48' & 49.3'' & \\ \Omega & 296 & 41 & 55.4 & \left.\vphantom{\begin{array}{c}1\\1\\1\end{array}}\right\} 1880.0 \\ \omega & 106 & 18 & 13.8 & \\ i & 5 & 31 & 3.5 & \\ \phi & 42 & 31 & 39.7 & \\ \log a & & 0.518438 & & \\ \mu & & 592.0373'' & & \end{array}$$

The period obtained from these elements is 2189 days, which confirms the fact already announced by Mr. Chandler and others that the comet has made two revolutions since its appearance in 1869. The period obtained is also nearly identical with that given by Prof. Frisby in "SCIENCE," which he derived from observations separated by intervals of only 13 days. The comet could not have been seen at its return in 1875, as the sun was between it and the earth, and it is probable that its next return in 1886 will be unobserved for the same reason, though a careful computation which shall take into account the perturbations of the comet due to the action of the planets will be necessary to determine the question.

Professor E. S. Holden, of the Naval Observatory at Washington, has accepted the managership of the Washburn Observatory in Madison, Wis., the position made vacant by the recent death of Professor Watson. Professor Holden will enter upon his duties in a few weeks.

ASTRONOMICAL MEMORANDA:— (Approximately computed for Washington, D. C., Monday, January 24, 1881.)

Sidereal time of Mean Noon. 20h. 16m. 37s.
Equation of time. 12 29
mean noon *preceding* apparent noon.

The *Sun*, having passed the winter solstice, has reached a declination of 19° 3' south.

The *Moon* reached its Last Quarter on Jan. 22d 16h., or 4 A. M. of Jan. 23.

New Moon comes on Jan. 29d. 8h., and the First Quarter on Feb. 5d. 8h. On the morning of the 24th the Moon crosses the meridian at about a quarter of seven.

Mercury, still invisible, comes into superior conjunction with the sun on the 26th, passes to his eastern side, and becomes evening star. Mercury is in conjunction with the Moon on the morning of Jan. 30.

Venus is evening star, and throughout the month increases her distance from the sun as she approaches the earth. She follows the sun by nearly three hours and is 3° south of the equator.

Mars is morning star, rising about six o'clock, and slowly traveling away from the sun.

Jupiter, evening star, crosses the meridian about half past four:—R. A. oh. 53m., Dec. 4° 21' north.

Saturn also is evening star, having reached quadrature, or halfway from opposition to conjunction, on the 12th, when he was on the meridian at six. Saturn and Jupiter, it will be noticed, are still steadily approaching each other.

Uranus crosses the meridian at about 3 o'clock in the morning, at a declination of 7° 21' north, and cannot claim any especial attention at present.

Neptune is in R. A. 2h. 39m; Dec. 13° 36' north. It reaches quadrature on the 30th, and will be found in conjunction with the Moon on—Feb. 4th.

IN the *Popular Science Monthly* for January, 1881, Dr. Leonard Waldo gives an interesting description of the method employed at the Yale Observatory, for comparing with the standards of that institution, thermometers which have been sent there for verification by physicians, instrument makers and others. He calls attention to the fact that thermometers, even if from makers of established reputation, are liable to errors much greater than is commonly supposed, and he points out the necessity of having such errors carefully determined.

WE learn from the *Comptes Rendus* that Janssen has made preparations at Meudon to repeat Dr. Draper's experiments on the photography of the Nebula in Orion, and that for this purpose he proposes to construct upon a large scale a telescope of short focus quite similar to the one with which he obtained a very luminous spectrum of the Corona, in 1871. Janssen has also made some experiments in photographing the chromosphere. The exposure is continued so long that the solar image becomes positive to the very circumference, without going beyond it. The chromosphere is then shown in the form of a dark ring with a thickness of 8" or 10". He has compared positive and negative solar photographs taken on the same day and with the same instrument, and the measurement of the diameter shows that the dark ring in question is wholly outside of the solar disk.

DR. WARREN DE LA RUE has been elected a corresponding member of the Paris Academy of Sciences in the section of Astronomy, and M. Sella a corresponding member in the section of Mineralogy.

THE Rumford medal of the Royal Society has been awarded to Dr. William Huggins for his work on celestial spectroscopy, and the Copley medal to Prof. J. J. Sylvester of Johns Hopkins University for researches in pure mathematics. W. C. W.

ECLIPSE OF THE SUN.

To the Editor of "SCIENCE:"

I would like to add a sentence to the fourth paragraph of my letter in last week's "SCIENCE" giving my observations of the recent partial eclipse of the sun. After the words "solar limb" I would add, "on the eastern side of the sun the phenomenon was considerably less prominent and only visible at the time of greatest obscuration, and when the slit was quite close to the sun's limb."

L. TROUVELOT.

CAMBRIDGE, January 12, 1881.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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SATURDAY, JANUARY 29, 1881.

THE advantages of having a good public library in a large city are so obvious that it appears incomprehensible that the most important city in the United States is practically without such an institution.

The city of New York appears to have been fortunate in being made the recipient of munificent testimentary gifts for the purpose of founding a great public library suited to the needs of such a community, but also unexceptionally unfortunate in the disposition of the funds so bequeathed.

The Astor Library contains a collection of books which have been most judiciously selected to form the nucleus of a good public library, and one peculiarly suited to the needs of those residing in such a city as New York. Unfortunately, the trustees of the library permit its use only between the hours of 10 A.M. and 4 P. M., thus practically shutting out the majority of those who desire to consult the literary treasures it contains.

Of the Lennox Library, recently bequeathed to the citizens of New York, it may be premature to speak; possibly in time its doors may be open to the public; but under what conditions and restrictions can only be conjectured from the eccentric formalities of the past.

Thus with the Astor Library open for a few fashionable hours during the day, and the Lennox Library closed altogether, the public of New York finds itself after four o'clock, P. M., daily, and during the whole of Sunday, without a free public library. Such a state of things is not creditable to the largest and most important city in this Republic, and should not continue a day longer.

The good policy of establishing a public library for New York city, which shall be under the full control of the city authorities, is daily becoming more apparent, and we trust the time is not distant when the wishes of the people in this respect may be fully realized.

A letter will be found in another page of this issue relating to our notice of Dr. Beard's lecture on "Mesmeric Trance." The writer is not correct in stating that we threw a doubt on the genuineness of the "phenomena, as a whole," as on the contrary our remarks questioned the integrity of the "subjects" produced by Dr. Beard. These men and boys, since the lecture in question, have been nightly performing the same tricks in a room on Sixth avenue, the advertisement for which is headed "Marvels and Fun of Mesmerism." The propriety of bringing such "subjects" before the New York Academy of Sciences, may well be questioned, and so far from accepting their performances as genuine exhibitions of the phenomena of Hypnotism, we apprehend the closest scrutiny should be made to test the genuineness of their acts.

Professor Hitchcock admits that he and others observed what appeared to us as evidence of collusion between Dr. Beard and his subjects, but objects to our having pointed out these facts, without having first permitted Dr. Beard to give his explanation of them. This amounts to a request to suppress all criticism, except that controlled by the person criticised, which appears to us one of the least inviting methods of arriving at the truth.

The subject is one of undoubted interest, and as we do not wish to prejudice the question, we defer any detailed reply to Professor Hitchcock's letter until others have had an opportunity of expressing their views. Our columns will be open to any correspondent who can add to our knowledge of this subject, or who can give a rational explanation of the phenomenon of Hypnotism.

SCIENTIFIC SOCIETIES IN WASHINGTON.

THE ANTHROPOLOGICAL SOCIETY.

NEW OFFICERS ELECTED AND A CHANGE OF LOCATION AGREED UPON.

The Anthropological Society met at the Smithsonian Institution on the evening of January 18th, Major J. W. Powell, the president, in the chair. The following new members were elected: Dr. A. F. A. King, Dr. William Lee, and Mr. Ivan Petroff for active membership, and Mr. J. C. Tache and B. B. Redding for corresponding membership. It being the evening of the annual election, no papers were read. A motion to remove from the present location to the lecture-hall of the National Medical College of the Columbian University was introduced by a committee of the council, and adopted by the society.

The election of officers to serve during the ensuing year resulted as follows: President, Major J. W. Powell; vice-presidents, Colonel Garrick Mallery, Dr. George A. Otis, Professor O. T. Mason, Dr. H. C. Yarrow; corres-

ponding secretary, C. C. Royce; recording secretary, Lester F. Ward; treasurer, J. Howard Gore; curator, Dr. W. J. Hoffman; council, President J. C. Welling, Professor E. A. Fay, Dr. J. Meredith Toner, Mr. F. A. Seely, Mr. Miles Rock, Mr. H. L. Thomas.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

On the first of December last, another society was organized for the study of the Biological sciences which, after completing its organization, elected the following officers for the ensuing year: President, Theodore Gill; vice-presidents, C. V. Riley, J. W. Chickering, Henry Ulke, Lester F. Ward; secretaries, G. Browne Goode, Richard Rathburn; treasurer, Robert Ridgway; council, George Vasey, O. T. Mason, J. H. Comstock, and Drs. Schafer and A. F. A. King. Professor S. F. Baird was elected an honorary member. Dr. Frank H. Baker, Mr. H. H. Birney and Mr. C. W. Scudder were elected to active membership. Professor L. F. Ward read a paper entitled "The Flora Columbiana of 1830 and 1880," in which a comparison was made between the lists of plants recorded as growing in the District of Columbia in 1830 in Brereton's "Flora," and the lists as now known to the botanists of the District. Mr. Ulke spoke of the occurrence in the District of many species of beetles, before known only in Alaska and other remote localities. Professor Jordan read a paper on "The Salmon of the California Coast," which contained many new and important facts regarding their habits and economic value. The annual address will be delivered at the next meeting by Professor Theodore Gill. A paper was also read by Professor Tarleton H. Bean on "An Excursion to the Northern Coast of Alaska."

CHEMICAL SOCIETIES.

The January *Conversazione* of the American Chemical Society was held at the rooms of the Society on Monday evening, January 17. The Vice-President, Dr. Albert R. Leeds, of the Stevens Institute, exhibited a new modification of Dinitro-orcine and certain of its salts. These salts were originally prepared by Professor Leeds at his own laboratory in the course of his investigations of Hyponitric Anhydride in organic substances.

Specimens of Dibenzole and Diphenyle were also exhibited by the same gentleman. Several of the members took advantage of the occasion to visit the laboratory and see the recently patented electrical inventions of Dr. O. Lugo.

The next and regular meeting will take place on the first Monday of February, the 7th prox.

The Chemical Society of Paris announces that among the vice-presidents, according to the constitution, the president shall be chosen from the following gentlemen; M. M. Grimaux, Salet and Berthelot, and that the Council nominates M. M. Grimaux and Salet; therefore M. Berthelot will remain as vice-president during 1881, and in consequence of the regretted decease of M. Personne, M. Berthelot will be the only occupant of that office.

The German Chemical Society at their annual re-union increased the dues of the non-resident members from 15 to 20 marks. This action has been in contemplation for several years, and has now been definitely settled.

M. B.

THE French Association for the Advancement of Science is to hold its next meeting in the city of Algiers, on the 14th of April. The people and authorities of the city are making preparations to give the Association a fitting welcome, and liberal appropriations have been made by the Council for organizing the meeting, to entertain the members and their friends.

THE UNITY OF NATURE.

BY THE DUKE OF ARGYLL.

V.

ON THE TRUTHFULNESS OF HUMAN KNOWLEDGE CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

But another nightmare meets us here—another suggestion of hopeless doubt respecting the very possibility of knowledge touching questions such as these. Nay, it is the suggestion of a doubt even more discouraging—for it is a suggestion that these questions may probably be in themselves absurd—assuming the existence of relations among things which do not exist at all—relations indeed of which we have some experience in ourselves, but which have no counterpart in the system of Nature. The suggestion, in short, is not merely that the answer to these questions is inaccessible, but that there is no answer at all. The objection is a fundamental one, and is summed up in the epithet applied to all such inquiries —that they are anthropomorphic. They assume authorship in a personal sense, which is a purely human idea—they assume causation, which is another human idea—and they assume the use of means for the attainment of ends, which also is purely human. It is assumed by some persons as a thing in itself absurd that we should thus shape our conceptions of the ruling power in Nature, or of a Divine Being, upon the conscious knowledge we have of our own nature and attributes. Anthropomorphism is the phrase employed to condemn this method of conception—an opprobrious epithet, as it were, which is attached to every endeavor to bring the higher attributes of the human mind into any recognizable relation with the supreme agencies in Nature. The central idea of those who use it seems to be that there is nothing human there; and that when we think we see it there, we are like some foolish beast wondering at its own shadow. The proposition which is really involved when stated nakedly is this: that there is no Mind in Nature having any relation with, or similitude to, our own, and that all our fancied recognitions of intellectual operations like our own in the order of the Universe are delusive imaginations.

The denial of what is called "The Supernatural" is the same doctrine in another form. The connection may not be evident at first sight, but it arises from the fact that the human mind is really the type of the Supernatural. It would be well if this word were altogether banished from our vocabulary. It assumes that we know all that "Nature" contains, and that we can pronounce with certainty on what can and what cannot be found there. Or else it assumes that Nature is limited to purely physical agencies, and that our own mind is a power and agency wholly separate and distinct from these. There might indeed be no harm in this limitation of the word if it could be consistently adhered to in all the terms of any argument involving its use. We are all quite accustomed to think of Man as not belonging to Nature at all—as the one thing or Being which is contradistinguished from Nature. This is implied in the commonest use of language, as when we contrast the works of Man with the works of Nature. The same idea is almost unconsciously involved in language which is intended to be strictly philosophical, and in the most careful utterances of our most distinguished scientific men. Thus Professor Tyndall, in his Belfast address to the British Association, uses these words: "Our earliest historic ancestors fell back also upon experience, but with this difference, that the particular experiences which furnished the weft and woof of their theories were drawn, not from the study of Nature, but from what lay much closer to them—the observation of men." Here Man is especially contradistinguished from Nature; and accordingly we find in the next sentence that this idea is connected with the error of seeing our-

selves—that is, the Supernatural in Nature. "Their theories," the Professor goes on to say, "accordingly took an anthropomorphic form." Further on, in the same address, the same antithesis is still more distinctly expressed thus: "If Mr. Darwin rejects the notion of creative power acting after human fashion, it certainly is not because he is unacquainted with the numberless exquisite adaptations on which the notions of a supernatural artificer is founded." Here we see that the idea of "acting after human fashion," is treated as synonymous with the idea of a supernatural artificer; and the same identification may be observed running throughout the language which is commonly employed to condemn Anthropomorphism and the Supernatural.

The two propositions, therefore, which are really involved in the thorough-going denial of Anthropomorphism and the Supernatural are the following: 1st, that there is nothing above or outside of Nature as we see and know it; 2nd, that in the system of Nature, as thus seen and known, there is no mind having analogies with our own.

Surely these propositions have been refuted the moment the definition of them has been attained. We have only to observe, in the first place, the strange and anomalous position in which it places Man. As regards at least the higher faculties of his mind, he is allowed no place in Nature, and no fellowship with any other thing or any other Being outside of Nature. He is absolutely alone—out of all relation with the Universe around him, and under a complete delusion when he sees in any part of it any mental homologies with his own intelligence, or with his own will, or with his own affections. Does this absolute solitariness of position as regards the higher attributes of Man—does it sound reasonable, or possible, or consistent with some of the most fundamental conceptions of science? How, for example, does it accord with that great conception whose truth and sweep become every day more apparent—the Unity of Nature?

How can it be true that Man is so outside of that unity that the very notion of seeing anything like himself in it is the greatest of all philosophical heresies? Does not the very possibility of science consist in the possibility of reducing all natural phenomena to purely mental conceptions, which must be related to the intellect of Man when they are worked out and apprehended by it? And if, according to the latest theories, Man is himself a Product of Evolution; and is therefore, in every atom of his body and in every function of his mind, a part and a child of Nature, is it not in the highest degree illogical so to separate him from it as to condemn him for seeing in it some image of himself? If he is its product and its child, is it not certain that he is right when he sees and feels the indissoluble bonds of unity which unite him to the great system of things in which he lives?

This fundamental inconsistency in the Agnostic philosophy becomes all the more remarkable when we find that the very men who tell us we are not one with anything above us, are the same who insist that we are one with everything beneath us. Whatever there is in us or about us which is purely animal we may see everywhere; but whatever there is in us purely intellectual and moral, we delude ourselves if we think we see it anywhere. There are abundant homologies between our bodies and the bodies of the beasts, but there are no homologies between our minds and any Mind which lives or manifests itself in Nature. Our livers and our lungs, our vertebræ and our nervous systems, are identical in origin and in function with those of the living creatures round us; but there is nothing in Nature or above it which corresponds to our forethought, or design, or purpose—to our love of the good or our admiration of the beautiful—to our indignation with the wicked, or to our pity for the suffering and the fallen. I venture to think that no system of philosophy that has ever been taught on earth lies under such a weight of antecedent improbability; and this improbability increases in direct proportion to the success of science in tracing the Unity of Nature, and in showing step by step how its laws and their results can be brought more and more into direct relation with the Mind and intellect of Man.

Let us test this philosophy from another point of view, and see how far it is consistent with our advancing knowledge of those combinations of natural force by which the system of the physical Universe appears to be sustained.

We may often see in the writings of our great physical teachers of the present day reference made to a celebrated phrase of the old and abandoned school of Aristotelian physics—a phrase invented by that old school to express a familiar fact—that it is extremely difficult, if not absolutely impossible, to produce a perfect vacuum—that is to say, a space which shall be absolutely empty. The phase was this: "Nature abhors a vacuum." It is now continually held up as a perfect example and type of the habit of thought which vitiates all true physical reasoning. Now let us observe what this error is. As a forcible and picturesque way of expressing a physical truth—that the difficulty of producing a vacuum is extreme, that Nature sets, as it were, her face against her doing it—the phrase is a good one, and conveys an excellent idea of the general fact. Sir W. Grove says of it, that it is an "aphorism, which, though caviled at and ridiculed by the self-sufficiency of some modern philosophers, contains in a terse though somewhat metaphorical form the expression of a comprehensive truth." But there is this error in the phrase (if indeed it was or ever could be literally understood)—that it gives for the general fact a wrong cause, inasmuch as it ascribes to the material and inanimate forces of Nature, whose simple pressures are concerned in the result, certain dispositions that are known to us as affections of Mind alone. In short, it ascribes to the mere elementary forces of Matter—not to a living agency using these as tools, but to mere material force—the attributes of Mind.

Now it is well worthy of remark, that, so far as this error is concerned, the language of physical science is full of it—steeped in it; and that in this sense it is chargeable with a kind of anthropomorphism which is really open to the gravest objection. To see Mind in Nature, or, according as Nature may be defined, to see Mind outside of Nature, acknowledging it to be Mind, and treating it as such—this is one thing—and this is the true and legitimate anthropomorphism which some physicists denounce. But to see Mind in material forces alone, and to ascribe its attributes to them—this is equally anthropomorphism, but a form of it which is indeed open to all the objections they express. This, nevertheless, is the anthropomorphism which gives habitually its coloring to their thoughts and its spirit to their language.

Let me explain what I mean by some examples. I will take, first, the theory of development, or the derivative hypothesis, which, as applied to the history of animal life, is now accepted by a large number of scientific men, if not as certainly true, at least as an hypothesis which comes nearer than any other to the truth. Whether that theory be true or not, it is a theory saturated throughout with the ideas of utility and fitness, and of adaptation, as the governing principles and causes of the harmony of Nature. Its central conception is, that in the history of organic life changes have somehow always come about exactly in proportion as the need of them arose. But how is it that the laws of growth are so correlated with utility that they should in this manner work together? Why should varied and increasing utility operate in the requisite direction of varied and increasing developments? The connection is not one of logical necessity. Not only can we conceive it otherwise, but we know it is otherwise beyond certain bounds and limits. It is not an universal law that organic growths arise in proportion to all needs, or are strengthened by all exertion. It is a law prevailing

only within certain limits; and it is not possible to describe the facts concerning it without employing the language which is expressive of mental purpose.

Accordingly, Mr. Darwin himself does use this language perpetually, and to an extent far exceeding that in which it is used by almost any other natural philosopher. He does not use it with any theological purpose nor in connection with any metaphysical speculation. He uses it simply and naturally for no other reason than that he cannot help it. The correlation of natural forces, so adjusted as to work together for the production of use in the functions—for the enjoyments and for the beauty—of life, this is the central idea of his system; and it is an idea which cannot be worked out in detail without habitual use of the language which is molded on our own consciousness of the mental powers by which all our own adjustments are achieved. This is what, perhaps, the greatest observer that has ever lived cannot help observing in Nature; and so his language is thoroughly anthropomorphic. Seeing in the methods pursued in Nature a constant embodiment of his own intellectual conceptions, and a close analogy with the methods which his own mind recognizes as "contrivance," he rightly uses the forms of expression which convey the work of Mind. "Rightly," I say, provided the full scope and meaning of this language be not repudiated. I do not mean that naturalists should be always following up their language to theological conclusions, or that any fault should be found with them when they stop where the sphere of mere physical observation terminates. But those who seek to remodel philosophy upon the results of that observation cannot consistently borrow all the advantage of anthropomorphic language, and then denounce it when it carries them beyond the point at which they desire to stop. If in the words which we recognize as best describing the facts of Nature there be elements of meaning to which their whole force and descriptive power is due, then these elements of meaning must be admitted as essential to a just conception and to a true interpretation of what we see. The analogies which help us to understand the works of Nature are not, as it were, foreign material imported into the facts, but are part of these facts, and constitute the light which shines from them upon the intellect of Man. In exact proportion as we believe that intellect to be a product of Nature, and to be united to it by indissoluble ties of birth, of structure, and of function, in the same proportion may we be sure that its organs of vision are adjusted to the realities of the world, and that its innate perceptions of analogy and resemblance have a close relation to the truth. The theory of Development is not only consistent with teleological explanation, but it is founded on teleology, and on nothing else. It sees in everything the results of a system which is ever acting for the best, always producing something more perfect or more beautiful than before, and incessantly eliminating whatever is faulty or less perfectly adapted to every new condition. Professor Tyndall himself cannot describe this system without using the most intensely anthropomorphic language, "The continued effort of animated nature is to improve its conditions and raise itself to a loftier level."

Again I say, it is quite right to use this language, provided its ultimate reference to Mind be admitted and not repudiated. But if this language be persistently applied and philosophically defended as applicable to material force, otherwise than as the instrument and tool of Mind, then it is language involving far more than the absurdity of the old mediæval phrase that "Nature abhors a vacuum." It ceases to be a mere picturesque expression, and becomes a definite ascription to Matter of the highest attributes of Mind. If Nature cannot feel abhorrence, neither can it cherish aspirations. If it cannot hate, neither can it love, nor contrive, nor adjust, nor look to the future, nor think about "loftier levels" there.

Professor Tyndall in the same address has given us an interesting anecdote of a very celebrated man whom the world has lately lost. He tells us that he heard the great Swiss naturalist, Agassiz, express an almost sad surprise that the Darwinian theory should have been so extensively accepted by the best intellects of our time. And this surprise seems again in some measure to have surprised Professor Tyndall. Now it so happens that I have perhaps the means of explaining the real difficulty felt by Agassiz in accepting the modern theory of evolution. I had not seen that distinguished man for nearly five-and-thirty years. But he was one of those gifted beings who stamp an indelible impression on the memory; and in 1842 he had left an enthusiastic letter on my father's table at Inverary on finding it largely occupied by scientific works. Across that long interval of time I ventured lately to seek a renewal of acquaintance, and during the year which proved to be the last of his life, I asked him some questions on his own views on the history and origin of organic forms. In his reply Agassiz sums up in the following words his objection to the theory of Natural Selection as affording any satisfying explanation of the facts for which it professes to account:—"The truth is that Life has all the wealth of endowment of the most comprehensive mental manifestations, and none of the simplicity of physical phenomena."

Here we have the testimony of another among the very greatest of modern observers that wealth—immense and immeasureable wealth—of Mind is the one fact above all others observable in Nature, and especially in the adaptations of organic life. It was because he could see no adequate place or room reserved for this fact in the theory of development that Agassiz rejected it as not satisfying the conditions of the problem to be solved. Possibly this may be the fault of the forms in which it has been propounded, and of the strenuous endeavors of many of its supporters to shut out all interpretations of a higher kind. But of this we may be sure, that if men should indeed ultimately become convinced that species have been all born just as individuals are now all born, and that such has been the universal method of creation, this conviction will not only be found to be soluble, so to speak, in the old beliefs respecting a creative Mind, but it will be unintelligible and inconceivable without them, so that men in describing the history and aim and direction of evolution, will be compelled to use substantially the same language in which they have hitherto spoken of the history of creation.

Mr. Mivart has indeed remarked in a very able work,[1] that the teleological language used so freely by Mr. Darwin and others is purely metaphorical. But for what purpose are metaphors used? Is it not as a means of making plain to our own understandings the principles of things, and of tracing amid the varieties of phenomena the essential unities of Nature? In this sense all language is full of metaphor, being indeed composed of little else. That is to say, the whole structure and architecture of language consists of words which transfer and apply to one sphere of investigation ideas which have been derived from another, because there also the same ideas are seen to be expressed, only under some difference of form. Accordingly when naturalists, describing plants or animals, use metaphorically the language of contrivance to describe the adaptations of function, they must use it because they feel it to be a help in the understanding of the facts. When, for example, we are told that flowers are constructed in a peculiar manner "in order that" they may catch the probosces of moths or the beaks of bees, and that this adaptation again is necessary "in order that" these insects should carry the fertilizing pollen from flower to flower, nothing more may be immediately intended by the writer than that all this elaborate mechanism does as a matter of fact attain

[1] "Genesis of Species."

this end, and that it may fitly be described "as if" it had been arranged "in order that" these things might happen. But this use of language is none the less an acknowledgment of the truth that the facts of Nature are best brought home and explained to the understanding by stating them in terms of the relation which they obviously bear to the familiar operations of our own mind and spirit.

And this is the invariable result of all physical inquiry. In this sense Nature is essentialy anthropomorphic. Man sees his own mind reflected in it—his own, not in quantity but in quality—his own fundamental attributes of intellect, and, to a wonderful and mysterious degree, even his own methods of operation.

It is really curious and instructive to observe how even those who struggle hardest to avoid the language of anthropomorphism in the interpretations of Nature are compelled to make use of the analogies of our own mental operations as the only possible exponents of what we see. Let us look, for example, at the definition of Life given by Mr. Herbert Spencer. It is a very old endeavor to construct such definitions, and not a very profitable one: inasmuch as Life is only known to us as itself, and all attempts to reduce it to other conceptions are generally mere playing with empty words. But it is not without instruction to observe that Mr. Spencer's laborious analysis comes to this: "Life is the continuous adjustment of internal relations to external relations." Bare, abstract, and evasive of characteristic facts as this formula is, it does contain at least one definite idea as to how Life comes to be. Life is an "adjustment." This is a purely anthropomorphic conception, conveying the idea of that kind of co-ordination between different powers or elements which is the result of constructive purpose. I have already pointed out in a former chapter that all combinations are not adjustments. The whole force and meaning of the word consists in its reference to intentional arrangement. No combination can properly be called an adjustment if it be purely accidental. When, therefore, Life is represented as an adjustment, this is the mental image which is reproduced; and in so far as it does reproduce this idea, and does consciously express it, the formula has at least some intelligible meaning. If, indeed, it has any plausibility or approach to truth at all, this is the element in it from which this plausibillty is derived.

We may take another case. Mr. Matthew Arnold has invented a new phrase for that conception of a Divine Being which alone, he thinks, can be justified by such evidence as we possess. And what is that phrase? "The Eternal, not ourselves, which makes for righteousness." Surely whatever meaning there may in this artificial and cumbrous phrase is entirely derived from its anthropomorphism. An agency which "makes for" something —that something, too, being in the future, and being also in itself an abstract, moral, and intellectual conception—what can such an agency be conceived to be? "Making for" an object of any kind is a purely human image—an image, too, derived primarily not from the highest efforts of human Will, but from those which are represented in the exercises of the body, and the skill with which, in athletic contentions, some distant goal may be reached and won. Such is the attempt of a very eminent man to instruct us how we are to think of God without seeing in Him or in His word anything analogous to our own thought and work.

Nor is it wonderful that this attempt should fail, when we consider what it is an attempt to do—to establish an absolute separation between Man and Nature; to set up Man as something above Nature, and outside of it; and yet to affirm that there is no other Being, and no other Intelligence in a like position. And if anything can render this attempt more unreasonable, it must be the urther attempt to reach this result through science—science, the very possibility of which depends upon and consists in the possibility of reducing all natural phenomena within the terms of human thought, so that its highest generalizations are always the most abstract intellectual conceptions. Science is the systematic knowledge of relations. But that which perceives relations must be itself related. All explanations consist in nothing else than in establishing the relation which some order of external facts bears to some corresponding order of thought; and it follows from this truth, that the highest explanations of phenomena must always be those which establish such relations with the highest faculties of our nature. Professor Tyndall, in another part of his Belfast address, like many other writers of the present day, goes the length of saying that the great test of physical truth is what may be called its "representability,"—that is to say, the degree in which a given physical conception can, from the analogies of experience, be represented in thought. But if our power of picturing a physical fact distinctly be indeed an indication of a true physical analogy, how much more distinctly than any physical fact can we picture the characteristic workings of our own mental constitution. Yet these are the conceptions which, we are told, we are not to cherish, because they are anthropomorphic—or, in other words, because of the very fact that they are so familiar to us, and their mental representability is so complete.

Some, indeed, of our physical teachers, conscious of this necessary and involuntary anthropomorphism of human thought and speech, struggle hard to expel it by inventing phrases which shall as far as possible avoid it. But it is well worthy of observation that, in exact proportion as these phrases do avoid it, they become incompetent to describe fully the facts of science. For example, take those incipient changes in the substance of an egg by which the organs of the future animal are successively laid down—changes which have all reference to a purely purposive adaptation of that substance to the future discharge of separate and special functions. I have already referred[2] to the fact that these changes are now commonly described as "differentiations," an abstract expression which simply means the establishment of differences, without any reference to the peculiar nature of those differences, or their relations to each other and to the whole. But the inadequacy of this word to express the facts is surely obvious. The process of dissolution and decay are processes of differentiation as much as the process of growth and adaptation to living functions. Blood is differentiated just as much when, upon being spilt upon the ground, it separates into its inorganic elements, as when, circulating in the vessels, it bathes and feeds the various tissues of the living body. But these two operations are not only different, but absolutely opposite in kind, and there does not seem to be much light in that philosophy which insists on using the same formula of expression to describe them both. It is a phrase which empties the facts, as we can see and know them, of all that is special in our knowledge of them. It is possible, no doubt, by this and other similar artifices of language, so to deprive them—or at least to appear to deprive them—of their highest mental characters. More foolish than the fabled ostrich, we may try to shut our eyes against our own perceptions, or refuse to register them in our language—resorting, for the sake of evasion, to some juggleries of speech. "Potential existence" is another of those vague abstract conceptions which may be, and is, employed for a like purpose. It may be applied indiscriminately to a mere slumbering force, or to an unfulfilled intention, or to an undeveloped mental faculty, or to an elaborate preparation of foresight and design. If we desire to take refuge from the necessity of forming any

[2] "SCIENCE," Vol. I., p. 181.

distinct conceptions, such phrases are eminently convenient for the purpose, whilst under cover of them we may cheat ourselves into the belief that we have got hold of some definite idea, and perhaps even of an important truth.

All who are puzzled and perplexed by the prevalent teaching on these high matters should subject the language in which it is conveyed to a careful, systematic, and close analysis. It will be found to fall within one or another of these three classes:—First, there is the phraseology of those who, without any thought either of theological dogma or of philosophical speculation, are, above all things, observers, and who describe the facts they see in whatever language appears most fully and most naturally to convey what they see to others. The language of such men is what Mr. Darwin's language almost always is—eminently teleological and anthropomorphic. Next, there is the language of those who purposely shut out this element of thought, and condemn it as unscientific. The language of this class is full of the vague abstract phrases to which I have referred—"differentiation"—"molecular change"—"harmony with environment," and others of a like kind—phrases which, in exact proportion to their abstract character, are evasive, and fall short of describing what is really seen. Lastly we have the language of those who habitually ascribe to Matter the properties of Mind; using this language not metaphorically, like the old Aristotelians whom they despise, but literally—declaring that Mind, as we know it, must be considered as having been contained "potentially" in Matter; and was once nothing but a cosmic vapor or a fiery cloud. Well may Professor Tyndall call upon us "radically to change our notions of Matter," if this be a true view of it; for in this view it becomes equivalent to "Nature" in that largest and widest interpretation to which I referred at the close of the last chapter—viz., that in which Nature is understood as the "Sum of all Existence." But if this philosophy be true, let us at least cease to condemn, as the type of all absurdity, the old mediæval explanations of material phenomena, which ascribe to them affections of the mind. If Matter be so widened in meaning as to be the mother and source of Mind, it must surely be right and safe enough to see in it those dispositions and phenomena which are nothing but its product in ourselves.

The truth is, that this conception of Matter and of Nature, which is associated with vehement denunciations of anthropomorphism, is itself founded on nothing else but anthropomorphism pushed to its very farthest limit. It is entirely derived from and founded on the fact that mind, as we see it in ourselves, is in this world inseparably connected with a material organism, and on the further assumption that Mind is inconceivable or cannot be inferred except in the same connection. This would be a very unsafe conclusion, even if the connection between our bodies and our minds were of such a nature that we could not conceive the separation of the two. But so far is this from being the case, that, as Professor Tyndall most truly says, "it is a connection which we know only as an inexplicable fact, and we try to soar in a vacuum when we seek to comprehend it." The universal testimony of human speech—that sure record of the deepest metaphysical truths—prove that we cannot but think of the body and the mind as separate—of the mind as our proper selves, and of the body as indeed external to it. Let us never forget that Life, as we know it here below, is the antecedent or the cause of organization, and not its product; that the peculiar combinations of matter which are the homes and abodes of Life are prepared and shaped under the control and guidance of that mysterious power which we know as vitality; and that no discovery of science has ever been able to reduce it to a lower level, or to identify it with any purely material force. And, lastly, we must remember, that even if it be true that Life and Mind have some inseparable connection with the forces which are known to us as material, this would not make the supreme agencies in Nature, or Nature as a whole, less anthropomorphic, but greatly more; so that it would, if possible, be even more unreasonable than it is now to condemn man when he sees in Nature a Mind having real analogies with his own.

And now what is the result of this argument—what is its scope and bearing? Truly it is a very wide scope indeed—nothing less than this: that nothing in philosophy, in theology, in belief, can be reasonably rejected or condemned on the sole ground that it is anthropomorphic. That is to say, no adverse presumption can arise against any conception, or any idea, or any doctrine on the mere ground that it rests on the analogies of human thought. This is a position—purely negative and defensive though it be—from which we cannot be dislodged, and which holds under its destructive fire a thousand different avenues of attack.

But this is not all. Another result of the same argument is to establish a presumption the other way. All the analogies of human thought are in themselves analogies of Nature, and in proportion as they are built up or are perceived by Mind in its higher attributes and work, they are part and parcel of natural truth. Man—he whom the Greeks called Anthropos, because, as it has been supposed, he is the only Being whose look is upwards—Man is a part of Nature, and no artificial definitions can separate him from it. And yet in another sense it is true that Man is above Nature—outside of it; and in this aspect he is the very type and image of the "Supernatural." The instinct which sees this image in him is a true instinct, and the consequent desire of atheistic philosophy to banish anthropomorphism from our conceptions is dictated by an obvious logical necessity. But in this necessity the system is self-condemned. Every advance of science is a new testimony to the supremacy of Mind, and to the correspondence between the mind of Man and the mind which is supreme in Nature. Nor yet will it be possible, in the face of science, to revive that Nature-worship which breathes in so many of the old religions of mankind. For in exalting Mind, science is ever making plainer and plainer the inferior position of the purely physical aspects of Nature—the vague character of what we know as Matter and material force. Has not science, for example, even in these last few years, rendered forever impossible one of the oldest and most natural of the idolatries of the world? It has disclosed to us the physical constitution of the Sun—that great heavenly body which is one of the chief proximate causes of all that we see and enjoy on earth, and which has seemed most naturally the very image of the Godhead to millions of the human race. We now know the sun to be simply a very large globe of solid and of gaseous matter, in a state of fierce and flaming incandescence. No man can worship a ball of fire, however big; nor can he feel grateful to it, nor love it, nor adore it, even though its beams be to him the very light of life. Neither in it nor in the mere physical forces of which it is the centre, can we see anything approaching to the rank and dignity of even the humblest human heart. "What know we greater than the soul?" It is only when we come to think of the co-ordination and adjustment of these physical forces as part of the mechanism of the heavens—it is only, in short, when we recognize the mental—that is, the anthropomorphic—element, that the Universe becomes glorious and intelligible, as indeed a Cosmos; a system of order and beauty adapted to the various ends which we see actually attained, and to a thousand others which we can only guess. No philosophy can be true which allows that we see in Nature the most intimate relations with our intellectual conceptions of Space and Time and Force, but denies that we

can ever see any similar relation with our conceptions of purpose and design, or with those still higher conceptions which are embodied in our sense of justice and in our love of righteousness, and in our admiration of the "quality of mercy." These elements in the mind of Man are not less certain than others to have some correlative in the Mind which rules in Nature. Assuredly, in the supreme government of the Universe these are not less likely than other parts of our mental constitution to have some part of the natural system related to them—so related that the knowledge of it shall be at once their interpretation and fulfillment. Neither brute matter nor inanimate force can supply either the one or the other. If there be one truth more certain than another, one conclusion more securely founded than another, not on reason only, but on every other faculty of our nature, it is this—that there is nothing but mind that we can respect; nothing but heart that we can love; nothing but a perfect combination of the two that we can adore.

And yet it cannot be denied that among the many difficulties and the many mysteries by which we are surrounded, perhaps the greatest of all difficulties and the deepest of all mysteries concerns the limits within which we can, and beyond which we cannot, suppose that we bear the image of Him who is the source of life. It seems as if on either side our thoughts are in danger of doing some affront to the Majesty of heaven—on the one hand, if we suppose the Creator to have made us with an intense desire to know Him, but yet destitute of any faculties capable of forming even the faintest conception of His nature; on the other hand, if we suppose that creatures such as (only too well) we know ourselves to be, can image the High and the Holy One who inhabiteth Eternity. Both these aspects of the truth are vividly represented in the language of those who "at sundry times and in divers manners" have spoken most powerfully to the world upon Divine things. On the one hand we have such strong but simple images as those which represent the Almighty as "walking in the garden in the cool of the day," or as speaking to the Jewish lawgiver "face to face, as a man speaketh to his friend;" on the other hand we have the solemn and emphatic declaration of St. John that "no man hath seen God at any time." In the sublime poetry of Job we have at once the most touching and almost despairing complaints of the inaccessibility and inscrutability of God, and also the most absolute confidence in such a knowledge of His character as to support and justify unbounded trust. In the Psalms we have these words addressed to the wicked as conveying the most severe rebuke, "Thou thoughtest that I was altogether such an one as thyself."

And perhaps this word "altogether" indicates better than any other the true reconciliation of apparent contradictions. In the far higher light which Christianity claims to have thrown on the relations of Man to God, the same solution is in clearer terms presented to us. "Knowing in part and prophesying in part," "Seeing through a glass darkly," and many other forms of expression, imply at once the reality and yet partial character of the truths which on these high matters our faculties enable us to attain. And this idea is not only consistent, but is inseparably connected with that sense of limitation which we have already seen to be one of the most remarkable and significant facts connected with our mental constitution. There is not one of the higher powers of our mind in respect of which we do not feel that "we are tied and bound by the weight of our infirmities." Therefore we can have no difficulty in conceiving all our own powers exalted to an indefinite degree. And thus it is that although all goodness, and power, and knowledge, must, in respect to quality, be conceived of as we know them in ourselves, it does not follow that they can only be conceived of according to the measures which we ourselves supply.

These considerations show,—first, that the human mind is the highest created thing of which we have any knowledge, its conceptions of what is greatest in the highest degree must be founded on what it knows to be the greatest and highest in himself; and, secondly, that we have no difficulty in understanding how this image of the Highest, may, and must be, faint—without being at all unreal or untrue.

There are, moreover, as we have seen, some remarkable features connected with our consciousness of limitation pointing to the conclusion that we have faculties enabling us to recognize certain truths when they are presented to us, which we could never have discovered for ourselves. The sense of mystery which is sometimes so oppressive to us, and which is never more oppressive than when we try to fathom and understand some of the commonest questions affecting our own life and nature, suggests and confirms this representation of the facts. For this sense of oppression can only arise from some organs of mental vision watching for a light which they have been formed to see, but from which our own investigations cannot lift the veil. If that veil is to be lifted at all, the evidence is that it must be lifted for us. Physical science does not even tend to solve any one of the ultimate questions which it concerns us most to know, and which it interests us most to ask. It is according to the analogy and course of Nature that to these questions there should be some answering voice, and that it should tell us things such as we are able in some measure to understand. Nor ought it to be a thing incredible to us —or even difficult to believe—that the system disclosed should be in a sense anthropomorphic—that is to say, that it should bear some very near relation to our own forms of thought—to our own faculties of mind, and soul, and spirit. For all we do know, and all the processes of thought by which knowledge is acquired, involve and imply the truth that our mind is indeed made in some real sense in the image of the Creator, although intellectually its powers are very limited, and morally its condition is very low.

In this last element of consciousness, however—not the limitation of our intellectual powers, but the unworthiness of our moral character—we come upon a fact differing from any other which we have hitherto considered. It is not so easy to assign to it any consistent place in the unities of Nature. What it is and what it appears to indicate, must form the subject of another chapter.

PROGRESS OF BOTANICAL SCIENCE IN THE UNITED STATES.

By J. C. Arthur.

The sketch by Professor Bessey in the December *Naturalist* of the work in Botany done in this country during 1879 is very interesting, and offers an opportunity of comparing the present status of the Science in America with its progress elsewhere. The article shows which departments have been most cultivated, and indicates to some extent the thoroughness and value of the observations and researches. The principal activity was manifested in Descriptive and Systematic Botany, and that largely among Phanerogams and Ferns. Such examples as Mr. Watson's "Revision of North American Liliaceæ" and Dr. Gray's "Botanical Contributions" are of the highest scientific value. These are accompanied by others which are little, if at all, inferior. Large and elegant works like Eaton's "Ferns of North America," Meehan's "Native Flowers and Ferns of the United States," Goodale's "Wild Flowers of America,"

and Williamson's "Fern Etchings," are signs of the healthy growth of popular interest in the objects of the Science.

Among the lower orders of plants, systematic work has not been so vigorous. The literature is widely scattered, and of many of the groups is in a most dishearteningly chaotic state. The disentangling and critical arrangement of this matter is at present one of the most important services that could be rendered the student. The labor of consulting all the descriptions belonging to any one group is often very great, and is always accompanied with a doubt if complete success has been attained. Further perplexities are the unequal value of the material when found, and the difficulty of determining synonymy. Monographs of the groups are exceedingly desirable; but such exhaustive studies are not often made, and in lieu of them careful compilations, aided by as much investigation and verification as possible, are very useful. Professor Bessey's "Erysiphei," Mr. Peck's "United States Species of Lycoperdon," and Dr. Halsted's "American Species of Characeæ" are admirable examples of such contributions to the advancement of knowledge.

It is a law in the growth of a biological science that the objects with which it deals must be carefully identified and systematically described before much progress will be made in the recondite investigations of structure and development, and the relations to physical forces, or in the higher problems regarding the *rationale* of forms and processes. Every advancement in morphology and physiology, however, reacts upon classification and helps to establish it upon a more satisfactory basis. While systematic work is thus the very foundation of the science, it is only by following it up in the same zealous manner with anatomical and physiological researches that the science makes most substantial advancement.

It is manifestly the natural and wise thing for American botanists to collect herbaria and study floras till the species and their distribution are fairly known. For Phanerogams and Ferns this has been well accomplished, and approximately so for Mosses and Liverworts, but the Thalloyhytes (Algæ and Fungi) remain comparatively unknown. Not but what there is still room for excellent systematic work among Phanerogams, but that the stumps and stones and other obstacles in the field have been pretty fully cleared away and it is now a matter of plain cultivation, while the other departments of the science need earnest workers who are not afraid of difficulties, and are willing to clear up and cultivate single handed as large areas as possible.

In the article cited, the Professor feels called upon to apologize for the neglect of Anatomy and Physiology during 1879. He says:—"While we may regret that so much of the field has been so sadly neglected in our country, we should remember that, as a rule, our botanists are overloaded with other duties which render it often impossible for them to command the time for making the necessary investigation." Admitting that the plea partly accounts for the inactivity, it still does not seem to touch the chief cause of the difficulty. It is rather to be ascribed to a lack of enthusiasm for these subjects. They have not yet come into vogue with lovers of the science: the tidal wave of laboratory and experimental Botany is yet but slightly felt; the problems seem new and strange, and just where and how to attack them appears obscure and uncertain. The work already done in these fields has mainly related to the means and accompanying phenomena of the fertilization of flowers. Some excellent papers have been published, although not lengthy. Histology, Embryology, and Physiology proper, however, appear almost without followers, judging from the results communicated. At the present time, Germany is the centre of the most active researches relating to the latter subjects, and France is not far behind.

In order to keep informed of the latest discoveries and results in the botanical world, an acquaintance with the journals in which they are announced is imperative. It is a trite saying in matters of daily life, that if one wishes to be "posted" he must read the papers. This applies even more forcibly to botanists, because their usual isolation deprives them of most other means of obtaining botanical news.

Among the most important exclusively botanical journals are the following: *Botanisches Centralblatt*, abstracts of the latest writings, and a full index, for all departments of the science; *Botanische Zeitung*, anatomy and physiology chiefly; *Flora*, general botany; *Pringsheim's Jahrbücker.*, physiological botany; *Hedwigia*, cryptogams; *Annales des Sciences Naturelles Botanique*, general botany, but with a large share of anatomy and physiology; *Bulletin de la Société Botanique de France*, general botany; *Journal of Botany*, largely systematic; *Grevillea*, cryptogams; and the two home journals—*Bulletin of the Torrey Club*, largely systematic; and *Botanical Gazette*, general botany, but inclined towards physiology. The first two of the list are weeklies; *Flora* is issued in thirty-seven numbers, and the others are monthlies. Beside these there are a large number of periodicals which devote considerable space to botanical matters, such as the *Quarterly Journal of Microscopical Science, Hardwicke's Science Gossip, American Naturalist, American Monthly Microscopical Journal*, etc. If one were confined to two, probably the *Botanische Zeitung* and the *Bulletin de la Société Botanique*, would prove the most satisfactory, presuming that the home journals are also taken, as a matter of course. Mr. Douglas, of Richland, N. Y., proposes to issue a translation of the *Zeitung*, for less than the subscription price of the original (but without the plates, we suppose). This laudable undertaking should receive substantial encouragement from English speaking botanists.

Probably there is no better indication of the beginning of a new era for American botany, than the changes made in the recent text-books. Dr. Gray's "Botanical Text-book" is expanded into four volumes, treating of the Morphological Structure of Phanerogams, Histology and Physiology, Cryptogams, and the natural orders of Phanerogams, respectively. The second volume is to be written by Dr. Goodale, and the third by Dr. Farlow. The first volume of the series has already appeared.

THE DETECTION OF STARCH AND DEXTRIN.

By Spencer Umfreville Pickering, B. A., Oxon.

In conducting some experiments in which it was necessary to ascertain the presence or absence of starch in a liquid containing various amounts of dextrin, the few facts here described were brought to light, and may, perhaps, be of sufficient interest to warrant their publication.

When a solution of starch which has been colored blue by the addition of iodine is heated, it is found that the temperature at which the color disappears varies with the intensity which it possessed before heating. Thus, for instance, 100 c. c. of a rather dark iodine-starch solution on being heated gradually in a flask became perfectly colorless at 58° C., and, on being cooled, showed a slight reappearance of color at 49° C., whereas an opaque blue solution did not lose its color till heated to 99° C., and became visibly colored again when cooled to 63° C. Similarly variable results were obtained by experimenting on iodine-starch solutions in sealed tubes, the temperatures of reappearance being much more constant (generally about 50° C) than those of disappearance; this no doubt is due to the fact that, the stronger solutions having been heated to a higher temperature than

the weaker ones in order to effect the disappearance, a greater quantity of the iodine present in them would have been converted into hydriodic acid, and this would tend to an equalization of the amounts of iodine present on cooling in the various cases. Owing to this conversion of iodine into hydriodic acid, the solutions on cooling, as might be expected, are considerably lighter than they were before heating, and their intensity naturally depends to a great extent on the rapidity with which they have been cooled; even a very weak iodine-starch solution which has been heated may be made to re-assume its color if cooled very quickly.

The amount of starch which may be recognized by means of the iodine reaction varies, of course, with the bulk of liquid operated upon. Using about 200 c.c. the weakest solution which gives an easily discernable blue tint in a beaker contains about 0.0001 per cent. of starch, while if small quantities are examined in a test-tube this percentage must be doubled in order that the color may be rendered visible. The green color which is noticed when a large quantity of iodine is added to a weak solution of starch, appears to be due simply to the combination of the proper yellow color of the free iodine with the blue color of the iodine-starch.

When two weak solutions of iodine, to one of which some starch has been added, are exposed to the air in an uncovered beaker, the iodine in both cases disappears entirely in the course of a few days, but more slowly from the solution which contains the starch; hence the iodine which disappears (owing partially to its volatilization into the air and partially to its hydrogenation) seems to be retained to a certain extent by the presence of starch. The presence of iodine has a reciprocal action in the preservation of starch. A solution of starch, which, in a few days, is converted into dextrin, may be preserved unaltered for a long time—possibly for an indefinite time, if an excess of iodine is present in it.

When a sufficient quantity of iodine is added to a solution of dextrin, a deep brown color is produced; the colored compound which is here present is in a state of true solution, whereas in the case of starch it will, as is well known, settle entirely to the bottom of the liquid in deep blue flocks, leaving the supernatant solution quite colorless, and these flocks on agitation are disseminated again so as to form an apparent solution. The dextrin reaction with iodine is not nearly so delicate as that of starch; the weakest solution which gave any discernable color on being tested contained 0.005 per cent. of dextrin, and in this case the color could only be detected by using about 200 c.c. of the solution, and comparing the color with that of some iodine solution of the same strength as that to which the dextrin had been added.

With starch, the first drop of iodine which is added produces a permanent coloration. With dextrin, however, this is not the case; the color produced by the first drops disappears instantly and entirely. A considerable quantity must be added before a moderately permanent color is produced, and the reaction, owing to which the iodine disappears in this way, will continue for six or seven days. Whether the dextrin disappears or not at the same time has not been ascertained, although it seems most probable that it should do so.

When a solution of iodine-dextrin is heated, the color becomes lighter and gradually disappears, as in the case of iodine-starch, but the temperature at which this disappearance takes place is considerably lower. An opaque brown solution on being heated in a flask became colorless at about 81° C., and, on cooling, regained its color with considerable diminution in intensity) at 64° C. A solution of one-quarter the strength of the preceding one lost its color at 52°, and regained it on cooling at 34° C.; here also, as in the case of iodine-starch, we find that the colored principle does not become colorless at any particular temperature, but its disappearance is dependent on its original intensity.

The dextrin usually met with in commerce contains a considerable amount of starch, which, however, may be entirely converted into dextrin by prolonged heating at 140° to 160° C for several hours, after which it gives the pure brown reaction with iodine above mentioned.

When iodine is added in excess to a mixture of starch and dextrin, the colors produced are blue, violet, purple, claret, red-brown, or brown, according to the various proportions in which the two substances are present. When the iodine is added gradually to the mixed solutions the colors produced, both temporary and permanent, follow the same order as those above mentioned, the blue colors appearing first, and the red ones only on the addition of larger amounts of iodine. Conversely, when the colored solution is allowed to stand, the red tints disappear first, and the blue ones last. Obviously, therefore, the gradual addition of iodine affords an easy and delicate means of detecting starch in the presence of even a large amount of dextrin. Another way in which starch may be detected in similar cases, is to add an ample sufficiency of iodine to produce a permanent color, and then to heat the liquid; the brown iodine-dextrin is decomposed at a comparatively low temperature, while the blue iodine-starch remains till the heat is raised considerably higher, and again, on cooling, the blue tint reappears long before the brown or red tint does; even when there is not sufficient starch to yield satisfactory results by this method, it may often be detected by the liquid being of a more bluish tint after the heating than it was before it.

O. Knab (*Chem. Centr. Blatt*, 1872, 492) found that some dextrin which he had prepared by repeated (ten times) precipitation with alcohol gave the reaction of a mixture of dextrin and starch, and hence concluded that it still contained some of this latter substance. It appears superfluous, however, to raise an impure preparation to the dignity of a chemical compound by giving it a distinct name—dextrin-starch—as Knab does. On leaving a mixture of solutions of starch and dextrin for some days, Knab found that, whereas the addition of iodine had at first caused a deep blue coloration, after a time nothing but the red or brown color of iodine-dextrin was produced, and hence draws the somewhat startling conclusion that starch under the influence of dextrin is converted into dextrin. A simpler and more probable conclusion from these experiments would surely have been, that at the end of the few days during which his experiments lasted, the starch had suffered that spontaneous decomposition to which it is, as is well known, so prone, leaving in solution nothing which would give a coloration with iodine but the unaltered dextrin.

Dextrin and starch, it appears, give entirely different reactions with iodine; the former combines with the halogen to form a brown soluble substance, whereas the latter forms with it a deep blue insoluble body; and these two reactions are so distinct that presence of either of the reagents may be easily detected in a solution containing both of them.

The fact that the addition of iodine to dextrin produces only a transitory color at first, and that an excess of it is necessary to give a permanent tint, will, no doubt, explain the various discordant statements which exist as to whether any color is produced by the mixture of these two substances or not, and will probably render unnecessary the theory of there being two or three different dextrins, as proposed by Mulder and Griessmayer.

DETERMINATION OF THE FATAL DOSE OF CARBONIC OXIDE FOR VARIOUS ANIMALS.—Air containing 1-300th of its volume of carbonic oxide proved fatal to a dog when inhaled for fifty minutes. In another dog of the same size the fatal dose was 1-250th. A rabbit resisted various proportions up to 1-60th. A sparrow perished with 1-500th.—M. GREHANT.

MICROSCOPY.

The annual reception of the New York Microscopical Society will be held on Monday evening, February 14th, 1881, at the rooms of the New York Academy of Sciences, No. 12 West 31st street.

Microscopical preparations of great interest will be exhibited, and the Board of Managers extend a cordial invitation to all possessing microscopes to attend the meeting. We trust that those microscopists residing in the city, who are not members, will avail themselves of this opportunity to observe the many facilties this society offers for extending a knowledge of this branch of science. Microscopical Societies do not profess to teach, but students will find ample opportunities of having the best methods of preparation practically explained to them, and by associating with the members at the ordinary meetings, information on any point relating to microscopy can be readily obtained. The annual dues of this society amount to $5 a year. Cards of admission to the *soirée* can be obtained of Professor Hitchcock, 53 Maiden Lane, New York City.

ASTRONOMY.

DR. B. A. Gould, Director of the Cordoba Observatory, Argentine Republic, has been unanimously elected a corresponding member of the Paris Academy of Sciences in the section of Astronomy, to fill the place of the late Dr. Peters.

The Observatory of Dunecht, near Aberdeen, Scotland, has undertaken the important matter of informing the astronomical observers in the United Kingdom, by means of circulars through the mails, of such facts as must be immediately made known to be of use. It has already issued thirteen circulars, and promises to be of the greatest advantage to British Astronomers.

W. C. W.

REMARKABLE METEOR.

Whilst returning home on the evening of December 29, 1880, I observed a very brilliant and somewhat remarkable meteor. Having seen no observation of this meteor published, and as it may be of interest, I will give a description.

The night was just beginning to be dark enough for the principal stars to shine brightly, the sky being intensely clear, with a cold, cutting wind from the west, the thermometer being below zero. My attention was suddenly attracted by a brilliant light; looking hastily up, I observed the meteor. It was very white and brilliant, with a short train; there was no sensible disk. It started from near δ *Aquarii* and moved at a moderate speed, passing some four or five degrees south of Venus, and appearing fully twice as large as that planet. After passing Venus a short distance, it suddenly flared up as if an explosion had occurred. It immediately slackened its speed, and assuming the brilliancy of a dullish first magnitude star, it floated slowly down in a slanting direction toward the southwest horizon. I watched closely, expecting to see it sink behind the horizon. It sunk slower and slower until, at an elevation of not more than 2°, it disappeared suddenly.

From the moment of explosion until its disappearance it was the size of a dull yellowish first to second magnitude star. No explosion was heard. It was first seen at about R.A. 22h. 54m. south declination, about 15°, disappearing at about R.A. 19h. 44m. and 19° or 20° south declination. Its visible path was about 42°.

It remained visible for fully half a minute, the greater portion of the time being after the explosion. Time, 6 hours Nashville m. t. Did any other observer note this object?

E. E. BARNARD.

NASHVILLE, TENN., *January* 19, 1881.

JUPITER.

THE RAPIDLY MOVING WHITE SPOT.

The white spot, described by me in "SCIENCE" (No. 24), having continued permanent up to the last observation of Jupiter, led me to investigate its history. Tracing backward through my note-book, I find observations at intervals of the same spot, the first observation being on June 26, 1880.

On account of its rapid motion and frequent variation of form I had at each observation failed to recognize the identity of the objects seen.

The spot has invariably borne the same relative position to a long sinuous rift in the northern part of the equatorial band. In 1879 a similar spot was observed, bearing then the same relative position to a similar rift. It is probable that the object seen in 1879 is identical with the present white spot.

My observations this year show a decided variation in the rotation period of this object. Its varying velocity is doubtless due to changes in its form. My sketches show it to be at times scarcely noticeable as a pale, tolerably well defined spot. At other times it is shown as a long curved brilliant spot with its head "tucked" under towards the south, apparently plowing the dusky material of the equatorial belt before it, and a well-defined luminous train following in its wake. A sufficient number of observations have not yet been obtained to decide under what form it attains its greatest velocity. It is likely some sort of violent action takes place in the spot, under the influence of which it becomes very white, increases its motion throwing off a luminous train and cleaving the matter composing the great equatorial "river" like a vessel scudding before the gale. The action in the spot then gradually becomes quiescent, its motion slackens and it drifts along shorn of its train and scarcely recognizable; remaining thus until the forces in it are again at work, when it will once more pursue its rapid course in all the glory of a streaming train. But a lack of observations leaves its times of greatest motion in doubt, and it may be that the motion is greater when its appearance is less conspicuous.

On December 31 this object was seen as a pale, well-defined spot without any train. It was slightly following —by about two or three minutes—the meridian of the following end of the great red spot, having, since the middle of November, made a complete circuit of the planet, and was once more passing the red spot.

At the next observation, January 7, it had left the red spot a considerable distance behind, coming to the middle of the disk one hour before the red spot was central, having passed that object at about the time predicted in "SCIENCE" (No. 24.)

From the observations of June 26, 1880, and January 7, 1881, I get a rotation period of 9h. 50m. 47s.; in this case the transit on June 26 was estimated from a sketch The observations of Nov. 22 and December 2 give a period of 9h. 50m. 19s. Transits of November 22 and December 29, give a period of 9h. 50m. 14s. Transits of November 22 and January 7 give 9h. 50m. 5s. An estimated transit on August 17 and observed transit of January 7 give for its rotation 9h. 50m. 9s. It makes a complete circuit of Jupiter, compared with the red spot, once in 45.08 days. If at any time it is seen passing the red spot it will in forty-five days go completely around the planet and back to the red object again, which would indicate a daily velocity of 6170 miles, or 257 miles an hour.

E. E. BARNARD.

NASHVILLE, Tenn., *Jan.* 18.

DETECTION OF ALCOHOL IN ETHEREAL OILS.—A. Drechsler employs, as reagent, a solution of 1 part potassium bichromate in 10 parts nitric acid of sp. gr. 1.30. Alcohol, if present, is at once betrayed by the pungent odor of ethyl nitrite.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

RELIEF TO THE JEANNETTE.

To the Editor of "SCIENCE:"

In compliance with your request concerning my views of the present probable status of the *Jeannette*, and especially the subject of a relief party to be sent to her, I would state that not desiring to renew at length the reasons set forth in the New York *Herald*, of January 12th, I will confine myself mainly to the few but important motives which point to the necessity of such a step in so far as they concern the interest of science. The urgency of immediate succor has been so thoroughly dwelt upon by yourself and others interested, that it can receive but little addition at my hands; suffice to say that the greater majority of Arctic accidents to naval expeditions, which would demand assistance, are of a violent character, such as wreck, ice-pressure, besettal and abandonment, etc., and which show plainly that rescue here, like that in all other zones, must be immediate to be effective in such emergencies. Also the necessity of replenishing the weakened portion of DeLong's crew, should they have been unfortunate in securing a sufficient supply of fresh meat can not be too strongly presented, for such a circumstance might fatally compromise an otherwise successful termination of the voyage, and just at the critical period of the undertaking. In a scientific point of view the field entered by the *Jeannette*, and which would be entered by her relief ship (which should carry a full and complete scientific corps) is one of the most interesting character. Nearly all of the Arctic estuaries of the Atlantic, have been more or less covered by the scientist and their fields of geography, physical and otherwise, their geology and minerology, their fauna and flora and many other kindred and interesting sciences, form huge volumes in the many libraries accessible to the student of these various topics, but on the Pacific side the many branches of science there presented form a vast field of investigation and research almost yet untrodden. That Lieutenant De Long's expedition could circumscribe, even in outline, this great theatre of undeveloped scientific resources is clearly impossible, and there have been but few predecessors along his route to show anything of value to those most deeply interested.

Every civilized nation has taken a public pride in bringing to light all the scientific knowledge attainable, pertaining to its own domain and its adjacent waters, acknowledging defeat and chagrin where it has been left to those differing in blood and allegiance to accomplish. It is only the savage, the barbarian, and semi-civilized community, that can allow these peaceful invasions without patriotic mortification or national chagrin. The Pacific Polar Seas are adjacent to the colonies of our own country and those of Russia. The latter has no great seaports or readily available fitting points in her Pacific coast whence an expedition may sail. With us, on the contrary, our Occidental shores are studded with goodly sized cities, one of which for such a purpose is as perfect as any in the world. It is therefore the plain duty of America to harvest this field, at least, even if the grain must be sent abroad to be ground.

It has also been proposed to establish permanent stations in the Arctic for scientific purposes, all nations uniting, forming a grand international chain, whose united observations will settle many disputed, and probably bring forth and illustrate many new, theories in the science of these zones, especially in the domain of meteorology, where continuous observations are so essential. To cover the Alaskan coast would be the least that could be expected of us, and it is not at all doubtful but that the British American shores belonging more peculiarly to us, by reason of contiguity, than to Great Britain, by reason of colonial possession, would be partially assigned to us, at least in this scheme. The relief party sent to the *Jeanette* could found this little colony, herself make extended investigations, and subserve the purpose of humanity by rescuing or relieving an expedition of our own countrymen under our own flag.

In all cases of abandonment of vessels in Arctic waters, the scientific collections have necessarily been left, as nothing should burden the retreating crews, except absolute necessaries, in a race for life where every ounce of weight is of vital importance, and these collections are almost as good as lost when only feebly represented by their descriptions and imperfect sketches. Such has been the fate of so many collections, rendering the voyage, in a scientific sense, almost *nil*, so that the rescue of an expedition, with such facilities of research, should meet the hearty encouragement of every scientist of America.

F. SCHWATKA.

GOVERNOR'S ISLAND, NEW YORK HARBOR,
January 29, 1881.

HYPNOTISM.

To the Editor of "SCIENCE:"

I doubt not that many of the readers of "SCIENCE" who attended the recent lecture of Dr. Beard, before the New York Academy of Sciences, will be surprised to read the article which you have published on page 13, Vol. II. It is not my purpose in this letter to defend the position of Dr. Beard in this matter, for if he deems it necessary I have no doubt he will give a satisfactory explanation of the few minor points which have given rise to your suspicions as to the genuineness of the phenomena. The circumstance of the person who was rendered deaf, and who was roused from his trance in the surprising manner which you describe, likewise aroused some questions in my own mind, as did also one or two other experiments; but instead of selecting these as a basis for adverse criticism, it has seemed to me more in accord with scientific methods to first inquire what explanation of them Dr. Beard himself can give.

The question before the general scientific world is not whether we can pick out single points for criticism, but whether the phenomena, as a whole, are genuine. The study of trance is not one with which most of us can claim familiarity, and although it is one which, more than almost any other, demands very special training to enable a person to profitably investigate the phenomena, we seldom find a person, even among scientific men, who has not his own ideas or theories or explanations about it. For this reason, Dr. Beard's careful study of the subject probably will not be fully appreciated during this, in some respects, conservative generation. Physical phenomena may be tested and abstruse hypotheses framed to explain them, and the world will accept the explanation; but in matters of trance, the clearest demonstrations cannot shake deep-seated beliefs, or convince unreasoning skeptics.

What has been the attitude of scientific men in the past toward this subject? It has been one of disbelief and nothing more. It is true that many of the phenomena (not all of them) have been known for many years. Your statement, however, that "nearly all our present knowledge of the subject dates from Braid's book" was directly contradicted by Dr. Beard in his lecture. Your assertion is only true of the phenomena. Dr. Beard's object was not to give an amusing exhibition of the phenomena of trance before a scientific body, but to explain them; the experiments being merely illustrative of the subject. I have still another criticism to make. You have assumed that "two of the subjects were evidently trained performers, if not professional actors." Admitting this

mere supposition, to be true, what possible bearing can it have upon the result? Why should not professional actors be as good subjects as any other persons? This objection seems to me about on an equality with some others which I have heard, *e.g.*, that all the subjects were trained to perform to suit the occasion. Your assertion that "the subjects of Dr. Beard are selected from the nervous classes of our population," is in direct contradiction to the doctor's declaration. In no sense can I regard your criticism as quite fair. Moreover, you have neglected to mention two of the most convincing demonstrations of the reality of the phenomena,—I refer to the extraction of two teeth from one subject, and the application of actual cautery to another. The opinion seems to be very common that the phenomena of mesmeric trance cannot be genuine unless all persons can be brought under its influence. A very little reflection will show that this is an erroneous opinion. There is much more that might be said upon the subject, but my purpose is only to correct the erroneous impressions which I am sure your article will give to many readers. I hope the columns of "SCIENCE" will be held open for a free discussion of these phenomena. R. HITCHCOCK.

To the Editor of "SCIENCE:"

In Dr. Spitzka's suggestive "Notes on the Anatomy of the Encephalon, etc.," in "SCIENCE," No. 29, occurs the following passage:

"Now, the third ventricle, as shown by Hadlich and Wilder, extends over the entire thalami."

I regret to be obliged to make a correction. The passage contains two distinct statements: the one, that the third ventrical extends over the entire thalami, and the other that such was shown to be the case by Hadlich and myself.

Since upon this point—as upon all others presented in the article—no exact references are given, I will not speak now of Hadlich's views; but no such statement has ever been made by me, and I am at a loss to understand how Dr. Spitzka can have gained that impression. On the contrary, my paper "On the Foramina of Monro in the Domestic Cat," read at the Boston meeting of the A. A. A. S., but not yet published, included an expression of my belief that, in the cat, the dorsal limit of the third ventricle on each side corresponds with the *Habena*, (the so-called "peduncle of the pineal body,") along which the *Endyma* (the lining membrane of the ventricles), is reflected from the mesial surface of the thalamus toward the opposite side. Hence, only the mesial aspect of each thalamus is "in the third ventricle," the remaining and much larger part of the surface being wholly extra-ventricular. BURT G. WILDER.

ITHACA, N. Y., *January* 26, 1881.

BOOKS RECEIVED.

BULLETIN No. 3 of the Illinois State Laboratory of Natural History at Normal, Ill., is a pamphlet of 160 pages, containing papers by the Director of the Laboratory, Prof. S. A. Forbes, on the following subjects: On some Interactions of Organisms; The Food of Fishes; Acanthopteri; On the Food of Young Fishes; The Food of Birds; Notes on Insectivorous Coleoptera. Likewise a brief but significant paper—Notes upon the Food of Predaceous Beetles, by Mr. F. M. Webster, who has independently come to the same conclusion as Prof. Forbes that the Carabidæ, in place of being exclusively insectivorous as is generally supposed, can, and in fact do, derive considerable sustenance from grains, grasses, and other vegetable substances.

The instructiveness and practical as well as scientific value of the researches which form the basis of these papers may be inferred from their titles, and from Prof. Forbes' well known accuracy and enthusiasm. But they are also very interesting and entertaining reading, and will thus be more apt to reach the minds of many who would otherwise fail to profit by the stores of information they contain. It would be well for other states to make the slight provision required for carrying on similar investigations into the food habits of the Birds, Fishes and Insects found within their limits.

B. G. W.

CHEMICAL NOTES.

DETECTION OF IODINE IN BORMINE AND METALLIC BROMIDES.—A few drops of the bromine in question are placed in a small porcelain capsule, 30 c.c. of a solution of potassium chlorate, saturated in the cold, are added, and the liquid is boiled till colorless. The solution is then poured into a test-tube, allowed to cool, mixed with a few drops of a solution of morphine sulphate and a little chloroform. If the chloroform takes a violet color, iodine is present in the sample. The morphine solution is prepared by dissolving 0.5 grm. morphine in an excess of dilute sulphuric acid, and diluting to 50 c.c. In examining potassium bromide the solution is mixed with 2 or 3 drops of pure bromine water, and a few c.c. of a cold saturated solution of potassium chlorate, and further treated as above. —A. JORISSEN.

DETERMINATION OF SULPHUR IN IRON PYRITES.—On oxidizing pyrites with nitric acid and precipitating the sulphuric acid from the ferriferous solution, slightly acidified with hydrochloric acid, there is always obtained a barium sulphate, contaminated with iron, and still the results were too low. The following process is, therefore, adopted: 1 grm. pyrites was mixed in a large covered crucible with 8 grms. of a mixture of equal parts potassium chlorate, sodium carbonate, and sodium chloride. The crucible is heated at first gently so as to dry the contents, which are afterwards melted at a high temperature. The mass when cold is treated with boiling water, and the solution together with the deposit is introduced into a measuring-flask of 200 c.c. filled up, filtered, and the sulphuric acid is determined in aliquot parts, say 50 c.c. The insoluble residue does not retain any sulphuric acid. In this manner the use of nitric acid is evaded. The decomposition of the potassium chlorate is complete.—BERNHARD DENTECON.

CONTRIBUTION TO ELECTROLYSIS.—L. Schucht describes the electrolytic determination of uranium, thallium, indium, vanadium, palladium, molybdenum, selenium, and tellurium. For qualitative analysis he uses a strong test-glass, 10 to 12 c.m. high, and 1.5 c.m. wide, fitted with a cork coated with paraffin. Two platinum wires, 1½ m.m. in thickness, pass through the cork down to the bottom, and are connected above the cork with the polar wires of the battery by means of small binding screws. This decomposition tube may be held in a wooden clamp. After the current has passed through the solution to be analyzed for ten to fifteen minutes, the stopper with the wires is drawn out, without interrupting the current, and the deposited metal is determined by its color, lustre, solubility in acids, &c. The manner of decomposition and the slight or strong evolution of gas is noticed. The solution is completely precipitated, rendered alkaline, and again electrolysed, after the wires have been cleansed. Copper is recognised by its color, mercury by the precipitated globules, nickel and cobalt by their lustre and sparing solubility in acids, zinc and cadmium by their color and solubility in potassa. The formation of peroxides is characteristic for lead, silver, bismuth, thallium, manganese. Bismuthic acid is gradually formed, whilst the peroxides of lead, silver, and thallium are deposited at the beginning of the precipitation. Silver peroxide dissolves in ammonia with liberation of nitrogen. The decomposition of the alkalies and alkaline earths is best effected in a U-tube. The hydroxides of the latter are separated in a voluminous form; those of calcium and magnesium in white crusts. The hydroxides of barium, strontium, and the alkalies dissolved on the negative wire, *Berg-und Hütten Zeitung*, 39, 121.

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ON MATTER AS A FORM OF ENERGY.

In the vortex-ring theory of matter as propounded by Sir William Thomson, the characteristic differences between the elements is supposed to be due to complications in the rings themselves, as they may be knotted in innumerable ways. Several such forms are drawn in the memoir, and one such is stamped upon the cover of "The Unseen Universe," by Tait and Stewart.

This vortex-ring theory assumes that matter is a *form of energy*, not interchangable with the other variable forms, such as heat, electricity, etc., for the simple reason that its *form* renders it impossible, but if the elements be forms of energy, the law of energy may possibly be traced in them. Now, the energy of a given mass of matter varies as the square of its velocity, but the *properties* of the mass vary with the form of the energy, that is to say, the physical properties of a heated body are not identical with those of the same body when it is cool, but possesses the same amount of energy in free path motion. The physical properties of atoms and molecules vary with atomic and molecular velocities; for example, whether a piece of iron or steel is magnetic or not depends upon its temperature, that is, its rate of molecular vibration. It is not, therefore, *a priori* improbable that such differences as exist between the ultimate atoms constituting what we call *mass*, may be due to relative velocities of rotation of the vortex-ring. Atomic weights represent numerically these constant differences, and one might expect to find in any one of these atomic weights the two factors that constitute energy, namely a mass (or its equivalent) and a velocity; so we might write $\frac{m\,v^2}{2}$ = atomic weight. Applying this to a specific case, suppose $\frac{m\,v^2}{2} = 75$ = atomic weight of Arsenic; by inspection it is seen that $m = 6$ and $v = 5$. If $m = 6$ and $v = 2$, then $\frac{6 \times 2^2}{2} = 12$ = At. Wt. Carbon. Let a table now be constructed $m = 6$ and v with values 2, 3, 4, and so on, and there results a series of numbers N either exactly the same as the atomic weights of some of the elements or a very close approximation to such numbers. The elements have their symbols under E with their atomic weights as given under At. Wt. for comparison.

$\frac{mv^2}{2}$ = Energy=Atomic Weight.

	N.	E.	At.Wt.		N.	E.	At. Wt.
$m=6$				$m=9$	18	?	----
$\frac{6\times 2^2}{2}=$	12	C.	12		40.5	Ca.?	40
$\frac{6\times 3^2}{2}=$	27	Al.	27		72	?	m
	48	Ti.	48		112.5	Cd.	111.6
	75	As.	75		162	?	----
	108	Ag.	108		220	?	----
	147	Di.	147	$m=11$	22	?	----
	192	?	---		49.5	?	----
$m=7$	14	N.	14		88	Sr.	87.2
	31.5	P.?	31		137.5	Ce.	137
	56	Fe.	56			Ba.	136.8
	87.5	Sr.	87.2		183	W.	184
	126	I.	127	$m=12$	24	Mg.	24
	171.5	Er.?	170.6		54	Mn.	54
	224	?	---		96	Mo.	95.8
$m=8$	16	O.	16		125	?	----
	36	Cl.?	35.5	$m=13$	26	?	----
	64	Cu.	63.3		58.5	Ni.	58.6
	100	?	m			Co.	58.6
	144	?	m		104	Ru.	103.5
	196	Au.	196			Rh.	104.2
		Pt.	196.7		162.5	?	----
		In.	196.7		234	Th.	233.9
		Os.	198.6				

By changing the value of m to 7, 8, 9, etc., a new series of numbers is obtained and the process is carried until the resulting number is higher than any known atomic weight, namely, that of Thallium 233.9. Where the number obtained is not that of any known atomic weight an interrogation point is placed. In several cases the resulting number is the same as the ones given by Mendelejeff as those of probable elements yet to be discovered; for example, in table $m = 9$. 72 is such a number and is marked m in the line of atomic weights.

Now, here is a series of forty numbers calculated serially, and thirty-three of them are either the exact atomic weights of elements or vary less than one unit from them, and it does not seem probable that so large a proportion could be the result of chance, for the numbers range from 12 to 234. Moreover, by carrying the process still further many more of the atomic weights are obtained. Thus, with $m = 13$ we have Co. Ni. Ru. Rh. and Th.

$m = 14$, Si. Cu. Cd. and one of Mendelejeff's hypothetical ones.

$m = 15$ only Antimony, 120.

$m = 16$ S. Te. Hg.

$m = 17$ Se. Ce.

$m = 20$ Ca. Zr.

It must be remembered that with this large value for m, only three or four calculations are possible without obtaining numbers quite beyond any known atomic weights; for instance, when $m = 20$, only three calculations can be made, two of which are atomic weights.

With 66 serial computations, 49 elements are determined; 74 per cent. and more than that if Mendelejeff's hypothetical elements may be counted.

If there be any underlying truth in this theory of calculation, then the conception of the elements will be much simplified, for it will dispense at once with complexity in the atom, and substitute a common form for all, differing arithmetically from each other in size and velocity. The only conception I have of the term m corresponding to *mass*, is a relative *volume* of ether in rotation with certain velocity.

TUFTS COLLEGE, MASS. A. E. DOLBEAR.

RECENT ADDITIONS TO THE E. M. MUSEUM AT PRINCETON COLLEGE.

HENRY F. OSBORN, S. D.

The E. M. Museum of Geology at Princeton has recently purchased Messrs. Ward & Howell's well-known collection of fossil animals and plants. Under the partial supervision of these gentlemen the collection has been unpacked and hastily arranged in the cases, and as it has never been fully displayed before, it now appears to very great advantage and possesses peculiar interest. The east wing of the museum already contains the collections made by the Princeton western parties during the summers of 1877 and '78. These include several hundred specimens of fossil insects preserved in the delicate Miocene shales of Florissant, Colorado, and leaves from the same neighborhood. The former have already passed into the hands of Dr. Scudder for identification. Still more valuable is a large collection of fossil leaves from Strata closely overlying the Lower Eocene Lignitic Beds, near Black Butte, Wy. Terr. These have been studied by Dr. Lesquereux; he pronounces them of great novelty as contributing largely to our knowledge of the extent of the Eocene Flora, and they will form the subject of a special memoir to be published by the museum.

Among the western Vertebrate collections are nearly complete skeletons of various members of the Dinocerata family, parts of which have been figured and described in bulletins from the museum. These, together with numerous specimens of *Palæosyops* and allied genera, from the now classical beds of the Henry's Fork and Bitter Creek country, Wy. Terr., together with a great variety of carnivorous, rodent, lemurine and perhaps insectivorous forms, many of which are undescribed, give an admirable idea of the fauna inhabiting the Lower Eocene. In addition to these are many complete turtles and remains of lizards, snakes and birds. Representing the Miocene is a collection from Colorado including widely different forms. Prof. Cope, who has kindly glanced over the whole collection, pencil in hand, pronounces several of these forms new to science.

The Ward collection is, however, of much greater value to the general student, as it includes representative specimens from almost every age and country—from the disputed *Eozoon canadense* of the Laurentian to the Post Pliocene cave bear and Irish elk. It is the result of seventeen years of intelligent travel, purchase and selections. Mr. Ward's theory being to perfect the collection by constantly substituting the best obtainable examples of each type, not aiming at a complete series for each age, but giving a synoptic view from the dawn of life upwards. In this he has succeeded, we have little doubt, far beyond his own expectations at the outset, and although his catalogues have made this collection familiar to many palæontologists in this country, it well deserves a brief description here.

The Silurian corals, crinoids and trilobites fill the first cases. The latter are very fine. Among them is the outline of an *Asaphus gigas* indicating an animal over 12 inches long. On large stone slabs are other Crustacea, *Eurypterus* and *Pterygotus*. These are the earliest of a series represented in the Jurassic by a fine collection from the Solenhofen Beds and throughout by numerous Trilobites. The Solenhofen crustacea include, among others, *Peneus, Glyphea, Eryon, Limulus, Æger,* and a very perfect *Megachirus,* while from the English chalk are some fine fossil crabs, *Enoploclytea, Hoploparia,* etc.

The remains of Devonian Ganoids are very numerous; *Osteolepis, Cheirolepis, Ptericthys, Cephalaspis* and other genera characteristic of the middle and lower Devonian. Most interesting, however, is a fine block containing a number of *Holoptychii* from the old red sandstone, which specimen comes direct from Hugh Millers's collection. From the Lias beds of Lyme Regis are well preserved specimens of *Dapedius, Lepidotus, Eugnathus* and others varying in length from one to three feet. There are fish remains from each epoch. The Solenhofen beds have furnished a very beautiful group, including *Cakuras, Lepidotus, Leptolepis, Aspidorhynchus* and others, imbedded in a clear yellow shale.

There are fine examples of *Lepidodendron* and *Sigillaria* from the English, Prussian and American coal measures; also, many ferns. Among these are perfect remains of *Sphenopteris* and *Pecopteris* from the Scottish coal measures, with a full series from Mazon Creek, Illinois. The fossil flora throughout is numerous, with good collections from the German, Italian and French Tertiary deposits.

From the Jurassic are eleven entire Saurians marked for their exceptional beauty, rather than great size. An *Icthyosaur,* over 11 feet in length, is the largest of a number of skeletons of this genus, and is finely preserved. One complete skeleton and several parts give a very correct idea of *Plesiosaurus.* A head of *Mistriosaurus* complete, rare in this country. From the Wurtemburg Lias is a large *Teleosaur* with the ventral scales in position. There is also a humerus of *Pliosaurus.* Besides these are many fragments; the ossified Sclerotic of Icthyosaurus and parts of the neck, pelvic and shoulder girdles affording a complete study. Probably belonging to the saurians, too, are the so-called bird tracks from the Triassic sandstone of the Connecticut River Valley, including tracks assigned to *Brontozoum, Anisopus* and other genera. Also of the five-toed *Cheirotherium,* supposed to mark the steps of *Labyrinthodon.*

The Echinoderms can be studied almost without interruption. In the earlier crinoid series are *Periechocrinus* and *Pentacrinus* from the older strata. The latter are represented beautifully and in profusion from the Lyme Regis locality, England. Among later forms are *Apiocrinus* and Eucrinus Lilliiformis, a rare specimen from the Brunswick Muschelkalk. In the Echinoid series are perfect specimens of *Periaster, Holaster* and *Hemiaster,* in addition to many others. Beautiful specimens of *Asterias* and *Astropecten* and *Ophiorderma* from the English Lias represent in part the Star Fishes.

The Cephalopods are a great feature of the collection,

beginning with *Endoceras, Gyroceras, Phragmoceras* and others characteristic of the Silurian merging into the more elaborate and coiled *Goniatites, Nautilus* and *Orthoceratites* of the Carboniferous, and into these forms and the *Ammonites* in the Cretaceous. The latter appear in great elegance and profusion from the Lias. In this and the two succeeding ages in which this family reached its maximum the Ammonite and Nautilus group are represented by a number of genera. The series closes in the multiplicity of Cretaceous forms *Ancyloceras, Crioceras, Scaphites, Hamites, Toxoceras* and many others. A heavy slab covered with Trigonia is noticeable among the Lamellibranchs. But a mere enumeration of these series and other Invertebrates that have not been mentioned gives but an inadequate impression of their value as a typical collection, which rests so largely, not upon their number but upon their exceptional perfection and completeness.

From New Zealand are the recent struthious birds, the collection containing many incomplete skeletons of *Meinornis,Dinornis* and *Palapteryx*, and completing the series are three fine Moas, one of them standing 8 feet high. There are important remains of *Halitherium, Titanotherium* and *Rhinoceros*, the latter from the Black Hills. From the Pleistocene shell marl underlying the peat beds near Limerick is a tall Irish elk, *Megaceros Hibernicus*, quite rare in this country. A cave bear from the south of France is one of the most perfect specimens that has been found. It is mounted complete, the ribs and a few vertebræ alone having been restored. These, with a large mastodon from Hudson, N. Y., a skull of Bos Primigenius, and many scattered Mammalian remains give an admirable idea of the Post Pliocene fauna of Europe and America.

The east wing of the museum is almost entirely filled by the collection. It contains no plaster, but the originals of over 130 of Ward's series of casts. It reflects the greatest credit upon the intelligence and energy of its collector. It will come into immediate service in connection with a lately instituted course of lectures upon Palæontology, and give new impetus to the general interest in Biology at Princeton.

THE CLASSIFICATION OF SCIENCE.

Rev. Samuel Fleming, LL. D., Ph. D.

II.

PRINCIPLES OF CLASSIFICATION.

Science may be properly classified with respect to either the order and facts of nature, or the laws of thought and methods of obtaining the knowledge of facts. In respect to the first basis, the classification may proceed upon the twofold method of arranging the order and laws of phenomena, separately considered, or of considering these in their immediate connection. And while either special method involves the complex process of nature, which is the province of philosophy in the discovery of laws,—the object of classification is to set forth the order of facts and laws which have already been discovered. It is a statement of their connections as brought within the scope of observation, as they stand in their completeness of order, while many facts may still remain unknown. Processes are continually going on in the physical realm, as exhibited in the heavens and in the earth. It is hence not a statement of historical development of each particular science, nor of the body of sciences. It is not an arrangement according to the chronological order of discovery of the facts. It is not a curriculum or course of study for discipline and acquisition. Such a course is arranged with reference to a harmonious development of mind, and requires the prosecution of diverse studies pursued simultaneously. Yet a proper classification proceeds upon the method of arranging or grouping the subordinate sciences according to both the order of philosophic inquiry, and of the subordination of facts and principles to the divisions and uses of science from the lower to the higher, and from phenomena to laws and applications.

Further, any scheme of clasification, founded upon material existences and relations irrespective of the immaterial entities which give qualities and motion to the material, must be radically defective. The fact of an order of succession in respect to the modification of the primary Force which inheres in matter, is too obvious to need more than a statement of the fact. Thus, in organic existence, the all-related force of Gravity is *general*, being applied to all bodies, whatever their constituents or mode of combination, while modified forms of this principle are limited to specializations. As at every step in the gradation of material existences, the order of nature is from the inorganic to the organic, so these terms involve the general and the special, and the addition proceeds from the lower and more general forms of force to the higher, more limited and special. Thus, also, in organic being we find Life as a common or general substance or entity, forming the basis of the general division of science denominated Biology. The lowest specialized form of life pertains to Botany,—the science of organic unconscious vegetal life, including many classes; the next higher pertains to Zoology, which is the science of that form of organic life, which has consciousness and animation, including many classes, and subordinate orders, kinds and species. The highest in gradation of being pertains to Anthropology, the science of the form of organic life which is conscious and rational, limited to mankind. In every higher order a new capacity has been added. It has been a "life unto life."

This natural order of classification from generals to specials, and from the lower to the higher, may be illustrated by the following diagrams, commencing with the lower, or gravitation, as in reading the scheme of classification accompanying this paper:

Life, { Man=organization + sensation + rational mind.
Animal=organization + consciousness and sensation.
Plant=organization.

Force, { Special : Chemical affinity.
Special : Cohesion.
General : Gravitation.

The fundamental distinctions of this classification are those which pertain to the body of sciences included in the scheme given. They are first, *Ontology*, the science of being, or the material or immaterial substances, qualities and attributes of universal being. This properly includes not only the general divisions given, but those which relate to the superior orders of being not given, viz.: Angeology, Christology and Theology. A classification of all Science, therefore, embraces these subjects. Ontology includes three general divisions: Cosmology, Biology and Anthropology. These are arranged in their natural order, as based upon the succession of immaterial or spiritual entities united with their respective material forms. Such order is essentially *serial*: in other words, there is a gradation of existences, as just noticed, and as indicated by the branch and group-descriptive terms given in the body of the scheme, as *Physico-dynamic*, etc.

Each general division includes its subordinate divisions or departments. Cosmology, the science of inorganic nature, includes three departments: Physical, Mechanical and Chemical Philosophy. The general term, Dynamology, formed upon the Greek etymon *dunam*, is used to designate the science of the immaterial principle, Force, as Biology designates the science of the vital principle, or Life. Biology and Anthropology include the several branches or departments as given. Individuals of a group are allied by some mode, principle or law distinguishing them from others in special respects.

The progress of science within the past few decades, and the very wide applications rendering divisions of scientific research and use indispensable, has made it necessary more and more to distinguish the several subordinate branches of a general division with reference to special relations and purposes of science. What has been denominated physical science in the reeent past is found to include too extensive a field of culture and use, and to require too vast an amount of scientific labor in research, analysis and application, both for individual gratification and for the demands of science. Then "Natural Philosophy" monopolized the whole field. Now Chemical Philosophy has taken the rank of a distinct department, and has extended its domain in every direction wherever it could find a field of research. It has even been obliged to review its own analyses, and to criticise its own results, by further experiment upon its own elements, to determine whether they are themselves compounds. And the analyses have yielded important fruits. Recently four new elements—cæsium, rubidium, thallium and iridium--have been detected by the new and wonderful method of the Spectrum Analysis, a notice of which will be given farther onward.

But Mechanical philosophy has an equal claim to distinction as a special department. Its aims and uses are practical—the relations and applications of matter and motion to mechanical effects; and in this age of inventive genius and of vastly extended applications of mechanical force to the demands of utility, give increasing importance to this department of science. The distinguishing triumphs of the past few years have resulted from the conservation of those forces and agencies which appear phenomenally in their general relations in physical nature, but are now specialized in this department for the higher uses of human society. Thus the form of force which has operated naturally as heat in all the previous history of matter, has become a science in mechanical philosophy, manipulated and controlled by scientific art, and takes the name of Thermotics, a science of vast extent and application. Hydrology has become specialized in Hydro-dynamics, Aerology in Pneumatics, Electricity in Electro-magnetism, etc. The subdivision of Physico-dynamic science into three departments—Physics, Mechanics and Chemics—seems to be demanded by the vastly extended range and special applications of these, as well as by the legitimate distinction recognized between *phenomena* and *laws*.

Cosmogony is treated as a branch of Astronomy. It is obvious this is its place, from the fact that Stellar Astronomy grows out of it, and includes its forming masses and nebular states. This contemplates a prior state, and the processes of the formation of special masses from the original mass of nebulous matter. The advancement from nebulous masses to globes in the various stages of condensation gives Stellar Astronomy. The sun is one of the stars, and is specialized as the center of the system to which our planet belongs, and hence Solar Astronomy is a consequent, and its place above Stellar Astronomy is appropriate. Again; our earth, far back in the periods of world-formations, was in its cosmogenic stage, forming part of the great nebulous cosmos; hence the term *geogony*, the science of the genesis of the earth, is grouped with *cosmogony*. But while the greater part of the earth's interior is still in its gaseous state, the facts pertaining to its crust create a new sub-group, as Geology, Mineralogy and Seismology.

Biology is divided into two general departments, while it includes three sub-sciences, viz.: Botany or Phytology, Zoology and Anthroposophy,—the latter being the science of the human physiological constitution. The radical distinction between animals and man pertains chiefly to the immaterial nature—the latter possessing rational and moral capacities, and also an order of physical nature not possessed by animals; yet a real distinction obtains physiologically, and indeed a vastly greater difference than between any of the different orders of animals. This distinction is stated in the classification. Physiology, which pertains to man's physical nature, is the sub-science of Biology, termed Anthroposophy, while comparative physiology, and morphology, belong respectively to Zoology and Phytology—the former relating to beings having sentient but irrational life, and the latter to insentient or unconscious life.

If this method of division, in which Biology and Anthropology share in the inclusion of a special subject appears to be anomalous, it is legitimate; for while both include those sciences which are grouped as belonging to physiological nature, Anthropology includes also the higher order of psychical nature, in essential connection with our mental, rational and moral nature,—entities and attributes of an imperishable subsistence, but whose functions and development for temporal existence depend upon the physiological connection. Biology is the general science of organic being having *Life;* Botany is the special science of organic being having *vegetal life;* Zoology is the special science of organic being having *sentient* life; Anthroposophy is the special science of organic being having *rational* life—the latter term having been chosen to express the distinction maintained above. If it is held by any readers of this paper that animals possess a psychical nature, as well as man, be it so. At least a nervo-ethereal nature may be predicated of beings having sensation and the power of voluntary motion; and such a substratum or basis of the physical as well as the sentient nature of animals, as corresponds with man's psychical nature, may exist, perhaps must. If so, it is reasonable to presume it must be of an order as much lower than man's psychical nature, as the mental or sentient constitution of animals is lower than man's. But if such psychical nature does exist, the fact can be known only by rational induction, for the beast has no capacity for language to verify the assumption.

INCOMPLETE, SUBORDINATE AND CONDITIONING SCIENCES.

Few of the physical sciences, especially, can be completly developed by themselves. Physics, Mechanics and Chemics are more or less mutually related, either as conditioned or conditioning. Astronomy has necessarily required for observation of its facts some of the principles and laws of physical optics, while scientific art has been called to construct appropriate instruments for observation, as the telescope and spectroscope. And the laws of planetary and stellar motion must necessarily be known before the science of astronomy can be fully acquired. But classification cannot await the discovery of all the facts of science, but must proceed with the materials at hand, when radical distinctions have been determined.

Geogony treats of general phenomena, the unformed, but forming and mingling elements, and conditions of meteorology by furnishing the materials involved in the latter science, in its special sphere.

Meteorology cannot be completed as a science by the study of the atmosphere alone, but in connection with the facts which reveal themselves by the action of atmospheric electricity. Thermotics, the science of heat, is but partially developed by the study of the ethereal radiations giving the physical phenomena of heat, but finds its completion in the experiments and application of mechanics, of hydrology and pneumatics.

Paleontology, being allied with mineralogy in respect to the general process of stratification, by furnishing materials which enter into it, properly belongs where it is assigned; yet these materials, constituted in part of fossils, cannot be completed without employing the facts which are brought forward in vital organisms. Hence paleontology is given as a conditioning science, contributing to botany and zoology, inasmuch as the ancient organisms, while many of them contain extinct types, are made a study in connection with living organisms;

and thus the apparent anomaly of the same branch of science being grouped both with physics and biology, is explained by the fact that paleontology, in its mere physical relations, deals with substances irrespective of relations to organisms, while fossilogy belongs to both. So, as already noticed, anthroposophy belongs both to biology and anthropology.

Light and sound are grouped together because produced by vibratory motion, yet not affiliated, because the media of vibration differ, the former being ether and the latter air. The analogy between light and sound is illustrated by firing a cannon at a distance from the observer; first the flash of light is seen at the moment of the explosion of the powder, transmitted at the rate of about 184,000 miles per second, the sound being heard some moments after the flash is seen, transmitted at the rate of about 1100 feet per second. Neither the luminous body nor the sonorous body throws off any *substance*, but only gives an impulse in wave-form causing vibrations of different kinds of substance,—ethereal vibrations exciting the optic nerve causing the sensation of seeing, and aerial vibrations exciting the auditory nerves causing the sensation of hearing. But while acoustics (or photology) is grouped with physical optics, in respect to the cause of their production, both musical sounds and colors are grouped as belonging to esthetics high in the series of science. In these respects both phonology and photology are subordinate sciences.

Actinism, produced by vibration of ether, like light, but exceeding in rate those which produce the highest color, *i. e.*, exceeding 800 billions of miles per second, is affiliated with electricity, light and heat, and bears relations to two diverse and widely separated sciences—photography and phytology. Its action is both chemical and vital, operating on the sensitive silver in photography (which more properly may be termed actinography), and also constitutes the vital agency necessary to excite germination in plants. This latter result has been attributed to the violet ray revealed by the spectrum, but this may be owing to the fact that the higher, inconceivably rapid vibrations of ether producing the actinic rays are not appreciated, and the effects in germination have been associated with the highest rays of light brought within the scope of vision. Actinism is hence grouped generally with sound, and specially with heat, light and electricity, but is subordinate to botany. There are reasons for the theory that electricity is concerned in normal vital action—not only vegetal, but animal.

Nature has anticipated both the mechanic and the fine arts. Far down in the depths of mineralogy are found gems of rarest beauty—the esthetics of Architecture. Up in the field of meteorology the clouds are tinted by the sunbeams with a perfection of beauty surpassing the possibilities of the esthetic art of Painting. "The music of the spheres" have for centuries enchanted the votaries of astronomical science, and still challenges the admiration of all observers contemplating the perfection of that grand choral movement which excels the harmony of a Handel or Beethoven—anticipating the rhythm both of Poetry and Music. Mineralogy, meteorology and astronomy belong to physical science, but they have furnished elements of the esthetic forms which reason appropriates in the sphere and achievements of the Fine Arts.

THE ROTATORY POWER OF COMMERCIAL GLUCOSE.*

A METHOD OF DETERMINING THE PERCENTAGE OF REDUCING MATTER BY THE POLARISCOPE.

By H. W. Wiley, Lafayette, Ind.

In the "trade" the name "grape sugar" is applied only to the solid product obtained from starch.

The name "glucose" is given to the thick syrup obtained from the starch, and which is used in immense quantities in this country for table use and other purposes.

Before being sent into the market it is usually mixed with a little cane sugar syrup to give it color rather than flavor, since the glucose itself is quite or nearly colorless. My polariscope is the *holb-schotten* variety, and is used with the sodium monochromatic light. The sugar scale is graduated to give 100 divisions, with a tube 200 m.m. long filled with sugar solution of 26.048 grammes in 100 c.c.

The angular rotation produced is 34°.7, which shows a specific rotatory power of 66°.6 for pure cane sugar.

In all my examinations I took 10 grammes of glucose in 100 c.c., and used tubes of observation 200 m.m. in length.

The average specific gravity of the various glucoses I examined was 1.412, and the number may be taken as a standard.

In order to conform to the following formulæ the specific gravity should not vary greatly from this number.

I have found from a large number of observations that the average reading on the sugar scale for 10 grammes of glucose in 100 c.c. is about 50 divisions. When the reading approached 53 divisions I found that the glucose contained nearly 53 per cent. of reducing matter, as determined by Fehling's solution. When the reading fell below 53 the percentage of reducing matter was above 53 and *vice versa*. I therefore made a large number of observations to determine, if possible, any relation between the polariscopic reading and the percentage of reducing matter.

I found as a result that the difference between the polariscopic reading and 53 multiplied by 1.25 gave a product which, added to or subtracted from 53, would give the percentage of reducing matter required. When we consider the difficulty of hitting the exact point in using the copper solution, the differences exhibited in the following table will not seem so important. See following page.

From a study of the following table we may write the following formulæ:

Let g = percentage of reducing substance, and a = reading of polariscope.

We may have three cases:

1st. $a = 53$.
2d. $a > 53$.
3d. $a < 53$.

For case 1st, $g = 53$ per cent.
Case 2d, $g = 53 - (a - 53)\ 1.25$ per cent.
Case 3d, $g = 53 + (53 - a)\ 1.25$ per cent.

ILLUSTRATIONS.

No. 14, following table.

$a = 40$.
$g = 53 + (53 - 40)\ 1.25 = 69.25$ per cent.

No. 16, following table.

$a = 63.80$.
$g = 53 - (63.80 - 53)\ 1.25 = 39.50$ per cent.

In seven of the seventeen cases given the percentage of reducing matter calculated from the polariscope exceeds that given by the copper solution and by a mean amount of .539 per cent. In ten of them it falls short, and by an average of .938.

In many examinations made subsequent to the above the mean deviation has been even less.

Hence I can say that the method indicated will give results which in the mean differ by less than the half of one per cent. from the reduction tests. I regard my calculations from the polariscope equally as reliable as those made with the copper solution.

* Read before the A. A. A. S., Boston, 1880.

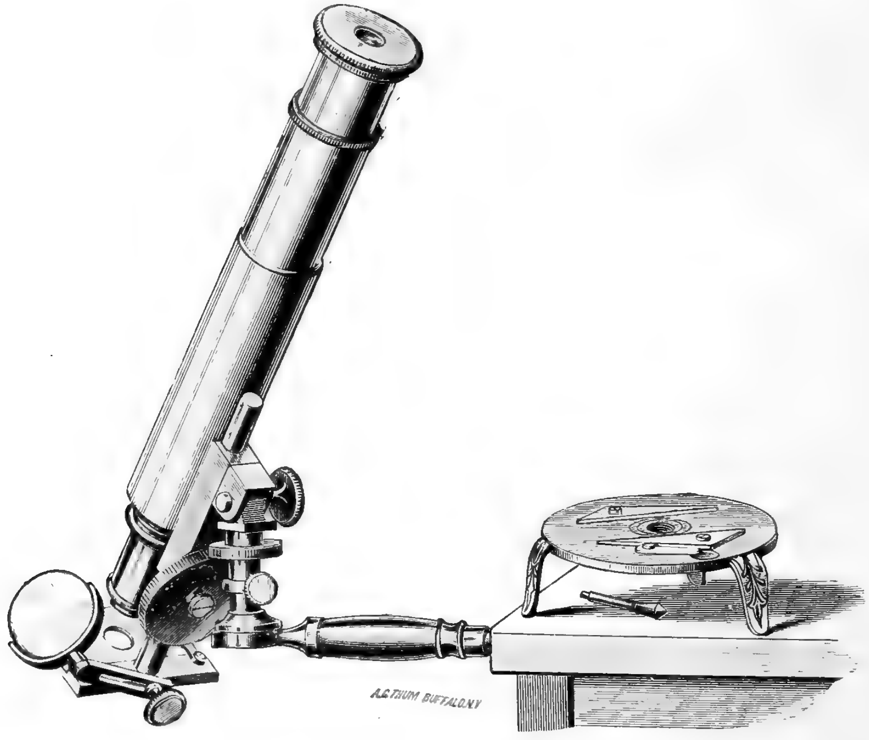
TABLE.

No.	Reading of Poloriscope.	Percentage of Reducing Matter by Copper Solution.	Percentage of Reducing Matter by Polariscope.	+ Differences.	–Diffe-ences.
1	52.65	53.20	53.44	.23	....
2	46.07	61.73	61.66	....	.07
3	52.65	52.36	53.43	....	1.07
4	43.05	62.50	64.90	2.40	....
5	48.04	59.35	58.75	....	.60
6	47.70	61.40	59.63	....	1.77
7	49.80	58.80	57.00	....	1.80
8	48.45	58.55	58.56	.01	....
9	50.26	55.60	56.45	.85	....
10	51.50	53.50	54.88	1.30	....
11	50.57	56.49	56.04	....	.45
12	51.74	56.18	54.58	....	1.60
13	40.83	69.93	68.21	....	1.72
14	40.00	69.30	69.25	....	.05
15	50.53	56.34	56.09	....	.27
16	63.80	39.22	39.50	.28	....
17	51.73	54.05	54.37	.32	....

NEW PORTABLE MICROSCOPE.

We present with this number two illustrations showing a new form of portable microscope stand, designed by Mr. E. H. Griffith, and called by him the "*Griffith Club Microscope*," the chief merit of which appears to be its portability, and adaptability to certain positions, which are impossible with the ordinary instruments.

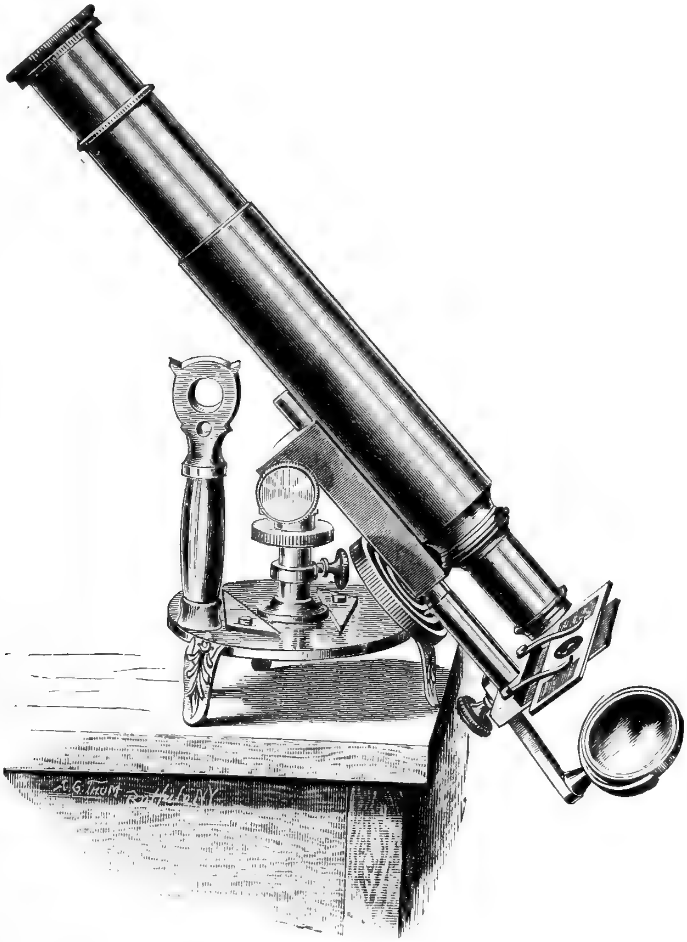
To those familiar with the use of the microscope an examination of the illustrations will suffice to arrive at a clear understanding of what Mr. Griffith has produced. It will be seen that much originality has been displayed, and that novelty of construction is a leading feature. The greatest innovation is the use of an ordinary self-centering turn-table for mounting, as a stand for the instrument; if, however, the turn-table is required for use, the microscope can be closed and used as a stand for the turn-table. The fine adjustment is also an original device of Mr. Griffith, and will be noticed as a large milled-edged screw in the cut. On the inner surface of this cir-



GRIFFITH'S PORTABLE MICROSCOPE. (Fig. 1.)

cular plate is a spiral groove into which works a pin controlling the stage. Mr. Griffith states that with this appliance, a very perfect focal adjustment can be obtained.

Illustration No. 1 shows the instrument attached to a table by a screw support, the mirror placed in position above the stage. As an adjunct to a dissecting table the Griffith microscope, thus used, would be found most useful, occupying no surface space. In excursions it could by the same means be attached to the side of a tree or to a fence. No arrangements have been as yet completed for the manufacture of this instrument, but it is believed they will shortly be made by a firm who will undertake to produce them at a reasonable cost, as Mr. Griffith has aimed to construct a serviceable portable instrument at a moderate price.



GRIFFITH'S PORTABLE MICROSCOPE. (Fig. 2.)

ON CHICKEN CHOLERA: STUDY OF THE CONDITIONS OF NON-RECIDIVATION AND OF SOME OTHER CHARACTERISTICS OF THIS DISEASE.*

BY M. L. PASTEUR.

I.

In the communication which I had the honor of presenting to the Academy in the month of February last, I announced, among other results, that chicken cholera originates in a microscopical parasite; that there is an attenuated virus of this disease, and that one or more inoculations of this attenuated virus may preserve chickens from death when inoculated with the virus of maximum virulence. On account of the striking similarity that these two forms of virus present with the effects of variola and vaccine in man, it becomes interesting to ascertain not only if the immunity from the more aggravated form of virus is absolute, for the regions of the body which have undergone the preventative inoculation, but also if this immunity exists in the system, no matter what portion of the animal may have been inoculated, and what may have been the manner of introducing the virus.†

To explain with brevity the results which I have to communicate, I may be allowed to use the word *vaccinate*, to express the act of inoculating a chicken with the attenuated virus. This being admitted, I may state, as the result of many experiments, that the effects of vaccination are very variable. Some chickens are little affected by the most virulent virus after one inoculation of the attenuated virus; others require two such inoculations, and even three. In every case, the preventive inoculation does some good, be-

* Translated from the *Comptes Rendus de l'Academie de Sciences*, of April 26th, 1880, page 952, by P. Casamajor. The translation of the first paper of this series appeared in the *Chemical News*, vol. xli., page 4 (July 2nd, 1880).

† From all I have seen and read of vaccine in man, and from my experiments on chicken cholera, I infer that *vaccine* rarely acts as a complete preventative. There are cases cited of vaccinated persons who have had the variola, and there are even cases of persons who have had it, afterwards, as much as three times.

cause it acts in a certain measure. Vaccination, then, may be of several degrees; but we may always succeed in completely vaccinating a chicken, which means that we can bring it to such a condition that it becomes incapable of being affected by the most virulent virus.

To make this matter clear, I will now give the results of experiments:—I take eighty new chickens (I call *new* those which never suffered before with chicken cholera). Twenty of these I inoculate with the most virulent virus, and they all die. Of the sixty that remain, I take another lot of twenty, and I inoculate them with that quantity of the most attenuated virus which the point of the needle will take up*—and not one dies. Are they then vaccinated for the aggravated form of virus? Some are and some are not, for if I afterwards inoculate these twenty chickens with the most virulent virus, six or eight of them will not die, although they may be ill, while in the first case every inoculated chicken died. I take again from the remaining chickens another lot of twenty, and these are vaccinated with the attenuated virus exactly as the preceding lot, and, a week afterwards, they are again vaccinated in the same manner. Are they now safe from the virulent virus? We now inoculate these twenty chickens with this virulent virus, and, instead of there being six or eight which do not die, there are twelve or fifteen. Finally, I take the twenty remaining chickens, and vaccinate them successively three or four times. If now I come to inoculate them with the most virulent virus, not one will die. In this case, chickens are brought to the condition of animals which are incapable of suffering from chicken cholera.

As to the cause of non-recidivation, I find it impossible to resist the idea that the microscopic germ, which causes the disease, finds in the body of the animal conditions suitable to its development, and that to satisfy the necessities of its life, the germ alters certain substances, or destroys them, which comes to the same thing, whether it assimilates them, or whether it consumes them with oxygen borrowed from the blood.

When complete immunity has been reached, the most virulent germ may be inoculated into any of the muscles without producing any effect. This means that the cultivation of the germ has become impossible in these muscles. They no longer contain food for the germ.

It is impossible to convey the impression that one receives from observing such phenomena. Here are twenty chickens which never had this disease. I inoculate them in their pectoral muscles or, still better, in the muscle of the thigh, so as to observe with greater ease the effect of the innoculation. The next day all the chickens are lying down; they are very lame and seem overcome by sleep. The inoculated muscle becomes of enormous size, and is profusely filled with the parasites. From time to time, a chicken dies, and, at the end of forty-eight hours they are all dead. We may take also twenty chickens, previously vaccinated several times, and inoculate them at the same time as the others, with the same virus, in equal quantities. The next day and the next, they are all alive and in good health; they eat and cackle as usual; the cocks crow; the inoculated muscles present nothing abnormal. There is not even a sign to show where the skin was punctured. This healthy condition remains permanent.

We may now inquire whether the impossibility of cultivating the parasite is not limited to the muscles which have been inoculated. This may be answered by introducing the deadly virus in the blood vessels and in the digestive organs. I have taken ten chickens, never before inoculated, and ten others inoculated several times with the mild virus. I have then injected the worst form of virus in the jugular vein of all these chickens. The first ten have died rapidly; many of them within twenty-four hours. The ten vaccinated chickens, on the contrary, have only been slightly ill from the incision of the skin and of the jugular vein, and were soon in good health. This shows that the blood of these ten chickens was itself *vaccinated*, which means that previous cultivation had deprived it of the materials fit for further developments of the germ.

As to the introduction of the parasite in the digestive organs, I have imitated the epidemics which depopulate poultry yards, by introducing the parasite in the food of the chickens. On the 11th of March I brought together twelve chickens, bought at the market that very morning, and twelve others, previously vaccinated several times. Every day I gave to these twenty-four chickens a meal of the diseased muscles of chickens, who had died from chicken cholera. Through the combs of the twelve chickens which had not been vaccinated I passed a platinum wire, so as to distinguish them from the other twelve. On the next day the unvaccinated chickens began to sicken and die. On the 26th of march the experiment terminated. Seven of the chickens that had not been vaccinated have died, and a *post mortem* examination reveals the fact that the disease was introduced in the system, either through the first portion of the alimentary canal, or, more frequently, through the bowels, which were highly inflamed, and sometimes ulcerated, in a manner which recalls the lesions of typhoid fever.* The five other unvaccinated chickens are more or less ill, one seriously so. As to the twelve vaccinated chickens, not one has died, and to-day † they are all alive, and in good health. We may now sum up the results as follows:

It is the life of a parasite, in the interior of the body, which causes the disease known as chicken cholera and which causes death by this disease. When the cultivation of this parasite cannot take place in the body of a chicken, the disease does not show itself. The chicken is then in the constitutional condition of animals which chicken cholera cannot attack. Animals in this condition may be said to be born vaccinnated for this disease, because the fœtal evolution has not placed in their bodies the proper food of the parasite, or because substances, which could serve as such food, have disappeared while they were yet young. We must not wonder that there are constitutions more or less apt to receive inoculations of certain kinds of virus, for, as was announced in my first note, the broth of beer-yeast is entirely incapable of supporting the life of the parasite of chicken cholera, while it is well adapted to the cultivation of a multitude of microscopical germs, notably of the bacteridia of carbuncular disease.

The explanation to which we are led by the facts already mentioned, of the different degrees of constitutional resistance of some animals, as well as of the immunity which chickens acquire by preventive inoculations, must seem a natural one, if we take into consideration that every cultivation modifies the medium in which it takes place. In the case of ordinary plants, the soil is modified, in the case of parasites, the animals and plants on which they live are also modified. The same thing happens with the liquids in which they live, in the case of ferments and other microscopical germs. The modifications which take place have this character in common, that new cultivations of the same species in these media soon became difficult or impossible. If chicken-broth is used for cultivating the germ of *chicken cholera*, and if, after three or four days, the liquid is filtered, to separate all the germs, and furthermore, if after this fresh quantities of the germs are placed in the filtered liquid, it will

* There are degrees of attenuation as well as of virulence. I will give explanations in a future communication.

* The blood is full of parasites, and the interior organs are frequently covered with pus and false membranes, particularly next to the intestinal pockets, through which the germ seems to have penetrated.

† April 26th.

be found incapable of producing the feeblest development. Perfectly limpid at first, the liquid remains indefinitely limpid.

We are led to believe that the cultivation of the attenuated virus in a chicken places its body in the same state as that of the liquid which can no longer sustain the life of the germ of disease. We may extend the comparison still further, for, if we filter the broth on the second day of the cultivation, instead of on the fourth, the filtered liquid will still permit the cultivation of the germ, but less readily than at first. This may enable us to understand that the cultivation of the attenuated germ in the body of a chicken may not have removed all the food for the germ. The remainder may allow a fresh cultivation of a feebler kind. This is the same as a first *vaccination*. Subsequent inoculations will remove progressively all the materials for the cultivation of the parasite. Through the action of the circulation, a time will come when any new cultivation on the animal will remain unproductive. Then the disease cannot recidivate, and the subject becomes perfectly vaccinated.

It may seem astonishing that the first cultivation could have stopped before all the food of the germ has been destroyed; but we must not forget that the germ is aerobian,† and that, in the body of an animal, it does not find the same conditions as in an artificial medium of cultivation, in which there are no obstacles to its propagation. In the body, on the contrary, it finds opposition from the cells of the organs, which are also aerobian, and are continually absorbing oxygen.

We might also account for the fact of non-recidivation by admitting that the life of the germ, instead of destroying certain substances in the body of an animal, on the contrary, adds other substances which act as an obstacle to its further development. The history of the life of these inferior beings, of all beings in fact, authorizes this supposition. The excretions due to vital functions often prevent vital functions of the same nature. In some fermentations, antiseptic products are formed while fermentation is going on, and even by the action of ferments, and these products put an end to further action, even if there are still substances left capable of undergoing fermentation. In the cultivation of our germ, there might, in the same way, be substances formed whose presence might explain non-recidivation and vaccination.

Our artificial cultivation of the parasite will enable us to examine this hypothesis. If we prepare an artificial cultivation of the germ of chicken cholera, we may evaporate the liquid *in vacuo* while cold, then bring it back to its original volume by the addition of chicken broth. If the extract contains a poison which destroys the germ, and if the presence of this poison is the cause of its non-development, the cultivation of the germ cannot take place in this liquid. On the contrary, the development does take place without difficulty. We cannot then believe that, during the life of the parasite, there are substances produced which prevent its further development. This is a corroboration of the opinion which we have expressed on the cause of non-recidivation in certain virulent diseases.

DENSITY OF LIQUID OXYGEN.—J. Offret has revised Pictet's calculation of the density of liquified oxygen and considers the method inadmissable. His own calculation gives 0.840.

EXPLOSIVE ANTIMONY.—A solution of crystalline antimony chloride and hydrochloric acid at 1.12 sp. gr. was prepared so as to stand at 38° B. On electrolysis with the Lechlanché element there was obtained in twenty to twenty-four hours a most explosive deposit.—E. MASCARENAS Y HERNANDEZ.

† Pasteur divides germs and other microscopic organisms into *aerobians* (requiring air to live) and *anaerobiæ* (which do not require air).—*Translator.*

ASTRONOMY.

THE Roman Academy of Sciences has awarded half of the King Hubert Prize to Dr. Wilhelm Temple, Director of the Acetri Observatory at Florence, for his observations on Nebulæ.

THE second Part of Vol. II. of papers relating to the Transit of Venus has recently been published by the Paris Academy of Sciences. It contains, among other things, the last of the Memoirs relating to the expedition to the island of St. Paul, the Meteorology by Dr. Rochefort, and the Geological Researches made at Aden, Reunion, St. Paul, Amsterdam and Seychelles, by M. Vélain. The first Part of Vol. III., which is to contain a report of the work done at Campbell Island, is in preparation.

THE "Reports of the Total Solar Eclipses of July 29, 1878, and January 11, 1880," forming Appendix III, to the "Washington Observations for 1876," has just been distributed from the Naval Observatory.

OWING to an error in the telegraphic dispatch, the discoverer of Comet *f*, 1880, was called *Pennule*. It should have been *Dr. C. F. Pechüle*, of Copenhagen. The comet seems to have two tails, one pointed towards the sun, and the other pointed about N. 15° *f*.

ASTRONOMICAL MEMORANDA: — (Approximately computed for Washington, D. C., Monday, February 7, 1881.)

Sidereal time of Mean Noon, $21^h\ 11^m\ 49^s$.

Equation of time, $14^m\ 25^s$.

Mean noon *preceding* apparent noon.

The *Moon* crosses the meridian at about 8.30 P. M. Full moon occurs on the 13th, and the last quarter on the 21st of the month:—New moon on the 29th.

Mercury is still evening star, following the sun by nearly an hour. He reaches his closest position to the sun on the 21st, and "greatest elongation" on the 22nd.

Venus is still the most conspicuous object in the evening sky. She increases her apparent distance from the sun until Feb. $20^d\ 7^h$, when she reaches "greatest elongation" East, an angular distance of 46° 34′.

Mars crosses the meridian at about 10 o'clock in the morning. He is nearly 23° south of the equator.

Jupiter and Saturn form with Venus an unusually good opportunity for the amateur astronomer to make use of his telescope in the early part of the evening. Jupiter and Venus will be in conjunction on the 21st.

Uranus is on the meridian about two hours after mid-night, and *Neptune* about half-past five in the afternoon. Uranus is in conjunction with the moon on Feb. 15th.

The *Comptes Rendus* for Jan. 3, 1881, contains a paper by M. Rouget upon a method for use at sea, and for travelers, explorers and others, for determining latitude and sidereal time, dispensing with the measurement of angles.

Two stars are observed having at a given moment, the same altitude: such observations are combined in pairs, and by merely noting the time which has elapsed between the two observations, a simple interpolation in tables prepared for the purpose will give the sidereal time and the latitude of the place of observation. Formulæ are given for the case mentioned above, and also for deducing the latitude and sidereal time from stars having the same azimuths, or azimuths differing by 180°. A succeeding paper by the same author extends the formulæ to the determination of longitudes, by employing observations of the moon.

W. C. W.

ON THE FIRST COMET OF 1861 AND THE METEORS OF APRIL 20.

By Professor Daniel Kirkwood.

M. Arago was the first to call attention to the frequent appearance of shooting stars in unusual numbers about the 20th of April, and to suggest the theory[1] that they are derived from a ring which intersects the earth's orbit. We are indebted, however, to the late Edward C. Herrick, of New Haven, for the collection of the principal facts by which the suggestion of Arago was fully sustained.

I.

THE GREAT METEORIC SHOWER OF APRIL 20, 1803.

More than thirty-six years after the event the old newspaper accounts of this wonderful display were sought out by Mr. Herrick and rescued from oblivion.[2] The following description of the phenomena as seen at Richmond, Va., is taken from the Virginia *Gazette*, of April 23, 1803.

"*Shooting Stars.*—This electrical phenomenon was observed on Wednesday morning last, at Richmond and its vicinity, in a manner that alarmed many, and astonished every person that beheld it. From one until three in the morning, those starry meteors seemed to fall from every point in the heavens, in such numbers as to resemble a shower of sky rockets. The inhabitants happened at the same hour to be called from their houses by the fire-bell, which was rung on account of a fire that broke out in one of the rooms of the Armory, but which was speedily extinguished. Every one, therefore, had an opportunity of witnessing a scene of nature, which never before was displayed in this part of the globe, and which probably will never appear again. Several of these shooting meteors were accompanied with a train of fire, that illuminated the sky for a considerable distance. One, in particular, appeared to fall from the zenith, of the apparent size of a ball of eighteen inches diameter, that lighted for several seconds the whole hemisphere. During the continuance of this remarkable phenomenon, a hissing noise in the air was plainly heard, and several reports, resembling the discharge of a pistol. Had not the city bell been ringing, these reports would probably have seemed louder. The sky was remarkably clear and serene, and the visible fixed stars numerous the whole night. We are anxious to know at what distance from Richmond this phenomenon has extended. It is hoped that persons who have remarked it in other places will not neglect to inform the public of the particulars; as such information may add in a great degree to the knowledge of meteorology.

Since writing the above, we have been informed that several of the largest of these shooting meteors were observed to descend almost to the ground before they exploded. Indeed, many of those which we saw, appeared to approach within a few yards of the house tops, and then suddenly to vanish. Some persons, we are told, were so alarmed that they imagined the fire in the Armory was occasioned by one of these meteors, and in place of repairing to extinguish the earthly flames, they busied themselves in contriving to protect the roofs of their houses from the fire of heaven."

The display was also witnessed at Raleigh, N. C.; Wilmington, Del.; Schoharie County, N. Y.; Portsmouth, N. H.; and at several points in Massachusetts. The descriptions of the shower as seen at these respective localities declare that, "the heavens seemed to be all on fire from the abundance of lucid meteors;" that they were "too numerous to be counted;" and that "part of the time the light was so great that a pin might be picked up on the ground." The shower, in short, would seem to have been one of the most extensive and brilliant on record, and hence to have been derived from a meteoric cluster of extraordinary density.

According to the catalogues of Biot and Quetelet[3] a great meteoric shower was seen in China on the 16th of March, B. C. 687. This date corresponds with the 20th of April in the nineteenth century. The display was therefore a shower of Lyraids. The interval between this extraordinary apparition and that of 1803 was 2490 years which may be regarded as a multiple of the true period.

The year 558 of our era,[4] midway between those brilliant displays, was the date of another great meteoric shower. The month and day are not given, but we may assume with reasonable probability that it was the great April display. Mr. Herrick found several other showers derived from the same stream. They seem, however, to have been of inferior brilliancy. They will be considered hereafter.

II.

THE FIRST COMET OF 1861.

The first comet of 1861 was discovered by Mr. Thatcher on the 4th of April. It was visible to the naked eye, and had a tail three degrees long. Its elements, calculated by Dr. Oppolzer, of Vienna, are as follows:

ELEMENTS OF THE FIRST COMET OF 1861.

Perihelion Passage	1861, June 3.
Longitude of Perihelion	243° 22′.
Longitude of Ascending Node	29° 55′.
Inclination	79° 45′.
Eccentricity	0.98345.
Semi-axis Major	55.67
Period	415.4 years.
Perihelion Distance	0.9207.
Aphelion Distance	110.425.
Motion	Direct.

Professor George Forbes has shown[5] that the comets of 1444, 1032 and 616 were former apparitions of this comet; the mean of the three periods being 415 years. The dates of ancient perihelion passages would therefore have been about A. D. 201, B. C. 214, and B. C. 629. In 1867, soon after the discovery, by Schiaparelli and others, of the connection between the comets of 1862 and 1866 with the August and November meteors, the probability of a similar relation between the first comet of 1861 and the meteors of April 20th was pointed out by Drs. Weiss and Galle.[6] The orbit of the comet nearly intersects that of the earth in longitude 210°, the point passed by the earth at the epoch of the April meteoric shower. An approximate equality of the periods of the comet and the meteoric stream was thus rendered highly probable.

The facts here collated constitute several very striking coincidences.

1. Dr. Oppolzer's period of the comet, derived solely from observations, is 415 years.
2. The mean period from 616 to 1861 was 415 years.
3. The interval between the great meteoric showers of B. C. 687 and A. D. 1803 is equal to 6 periods of 415 years.
4. The shower of A. D. 558 was midway between B. C. 687 and A. D. 1803.
5. The comet and the meteoric swarm seem to have equal periods.

It is by no means surprising that all returns of the meteoric group have not been recorded. The observations were restricted to the eastern continent; or, as

[1] In 1836.

[2] See Herrick's article in the Am. Journ. of Sci. for July, 1839, p. 358.

[3] Quetelet's Physique du Globe, p. 290.

[4] Quetelet's Catalogue.

[5] In a paper read before the Royal Society of Edinburgh, Feb. 16, 1880.

[6] Astr. Nach., Nos. 1632, 1635, and 1710.

Herrick has remarked, "some of them have doubtless been concealed by clouds, and others witnessed only by barbarians."

But between the great display of B. C. 687 and A. D. 1803, Professor Newton gives the following list of showers at or near the epoch of April 20,[7] viz.: B. C, 15, A. D. 582, 1093, 1094,[8] 1095, 1096, 1122 and 1123. The appearance of 582 ought probably to be rejected. It was two days from the epoch, and the record as quoted by Quetelet may have no reference to shooting stars.[9] The three remaining returns, B. C. 15, A, D. 1093-1096, and 1122-3, indicate a period of about 27 years. Now it is obvious that, at every close approach of meteors to the earth, many must be thrown into new orbits, all of which will pass through the point at which the perturbation occurred. It seems probable, therefore, that at some remote epoch a considerable cluster of this meteoric stream was thrown by perturbation into a new orbit corresponding to a period of 27 years. The change may have been produced by the earth during the passage of the meteoric swarm.

The facts which we have considered apparently indicate that the first comet of 1861, and the April meteors, formed a system in space before entering the solar domain; the latter moving in advance of the former at a distance comparable to the diameter of Neptune's orbit. By planetary perturbation the orbits were transformed into ellipses. If, as supposed by Professor Forbes, the disturbing body was an ultra-Neptunian planet in the vicinity of the present aphelion of the comet's orbit, said planet would probably describe less than 20° of its circuit during the interval between the nearest approach of the two bodies. But in aphelion the comet 1861 I, is too remote from the plane of the ecliptic to be sensibly disturbed by a planet moving in that plane. It seems more probable that the comet, as well as the meteoric group, owes the transformation of its orbit to one of the known major planets. Its radius vector when at its ascending node is about 10. In other words, its orbit approaches very near that of Saturn in longitude 30°. Now, it is remarkable that the interval between the perihelion passages of the meteors and the comet is almost exactly equal to two periods of Saturn. The meteors and Saturn were in the same longitude and in close proximity about B. C. 683, and the comet approached very near the planet at the same point about B. C. 625. The orbits may have been transformed into ellipses by Saturn's influence at these respective epochs. It may be worthy of remark that 11 times the period of the comet are equal to 155 times that of Saturn.

CHEMICAL NOTES.

ON BALLO'S SUPPOSED ADIPIC ACID OBTAINED FROM CAMPHOR.—On oxidation with chromic acid camphor does not yield adipic acid, but the same oxidation-products as with nitric acid. Chromic acid, however, converts the camphoric acid first formed completely into members poorer in carbon.—J. KACHLER.

ON THE REMARKABLE REDUCING PROPERTIES OF POTASSIUM FERROUS OXALATE, AND ON SOME OF THE REACTIONS THUS PRODUCED.—Ferrous oxalate is very permanent on exposure to the air, both in a wet and a dry state, and possesses very feeble reducing properties. The solution of ferrous oxalate in potassium oxalate, as well as the solid double salt, takes up oxygen greedily, and passes into potassium ferric oxalate. Its affinity for oxygen is equal to that of an alkaline ferrous hydrate, or of ammoniacal cuprous chloride, or of pyrogallic acid in an alkaline solution. The double oxalate exerts its reducing powers, not merely in alkaline, but in neutral, and even acid solutions. The solution quickly reduces platinum chloride and silver nitrate to metal. Silver chloride, bromide, and iodide are reduced completely, but more slowly. Copper acetate is reduced very slowly to cuprous oxide, and even to metal With the aid of heat mercuric chloride is reduced to metal. Recently precipitated Prussian blue is reduced to white ferro-cyanide of potassium. Indigo blue is reduced to white indigo, and solutions of sulphindigotic acid are rapidly decolorised.—J. M. EDER.

ON THE ACIDS $C_8H_{14}O_4$ FORMED FROM BUTYRIC ACID. Besides a volatile oily acid, probably identical with isocrotonic acid, there are formed by the reaction of suberic and bromobutyric acid, two acids agreeing in composition with suberic acid, but distinctly different from each other, and from the two isomeric acids produced by a corresponding reaction with brom-isobutyric acid. There exist, therefore, five isomeric suberic acids.—CARL HELL AND O. MULHAUSER.

A NEW SYNTHESIS OF PHOSPHENYL SULPHO-CHLORIDE.—Twenty parts phosphenyl-chloride are placed in a small flask with a reflux condenser, and five parts sulphur-chloride are slowly added by means of a dropping-funnel. After the reaction is over, the flask is set in a freezing mixture of Glauber's salt and hydrochloric acid. Pale yellow crystals of phosphenyl-tetra-chloride are formed, from which the liquid is separated by decantation, then shaken with water, dried and rectified. The yield is almost quantitative.—H. KOEHLER.

MORE PARTICULAR OBSERVATIONS ON THE ACTION OF POTASSIUM CARBONATE UPON ISOBUTYL-ALDEHYDE.—F. Urech places about 3 grms. pure isobutyl-aldehyde in a narrow test tube graduated in half millimetres. With a lens it is possible to read accurately quarter millimetres. After 3 decigrms. of finely-powdered recently-ignited potassium carbonate have been added, the tube is closed, set in a horizontal position, and the level is read off every five minutes for forty-eight hours. The liquid will be found to have sunk from 21.50 to 14.50 degrees.

AT a meeting of the Société Industrielle of Mulhouse, it was stated that tin sulphocyanide, formed by the double decomposition of calcium sulphocyanide and tin oxalate, is found very useful in calico printing.

For printing cotton with the azo-colors, Dr. Allrich proposes to dissolve 100 grms. of the color in five times its weight of water; then to make up a solution of sodium stannate or aluminate at 15° B., to every litre of which are added 20 grms. alizarin oil. Of this mixture 150 grms. are incorporated with the color, which is then thickened with starch and printed. After printing the pieces are steeped for an hour in lead or barium acetate or barium chloride at 5° to 10° B., and washed in cold water.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

In reference to the correction of one of my statements made in your issue of the 29th inst. by Dr. Burt G. Wilder, I would say that I accept the criticism in all its bearings. The view which Dr. Wilder expresses regarding the upper wall of the third ventricle being constituted by the *ependyma* stretched across between the *habenulæ* of the pineal gland, was once entertained by myself (in accordance with the orthodox view of embryologists since the time of Rathke), and was the one which Dr. Wilder may perhaps recollect I expressed to him in conversation last year. I return to that view again. My abandonment of it was due to the confounding of two distinct questions, *i. e.*, the question of the true inner boundary of the floor at the lateral ventricle and the true upper and outer boundary of the third. The view I should have credited to Wilder and Hadlich, is that the *lateral ventricle does not* extend over the thalamus. My misapprehension of Wilder's statement is based on the fact that it rested on a verbal communication. That I mentioned it at all was

[7] Am. Jour. of Science, July, 1863.

[8] "At this period, so many stars fell from heaven that they could not be counted. In France the inhabitants were amazed to see one o thenmsfi great size fall to the earth, and they poured water on the spot, we hish their exceeding astonishment smoke issued from the ground with aoot f--ing noise."—Herrick's Catalogue. This record is of great interest as indr cating the fall of an aerolite during the shower of meteors.

[9] "A Soissons, on voit le ciel en feu. Une pluie de sang tombe sur Paris."

due to my unwillingness to adopt even unpublished views of others without at least giving formal credit. If my good intentions have in this instance had rather the effect of doing an injustice, that was not a matter of intent, but of error, and one I may claim an excuse for under the circumstances above explained.

Dr. Wilder refers to the absence of literary references in my short paper. This is in accordance with the fact that the paper was a provisional communication of results purely my own, and that quotations of the views of others were limited to such relating to collateral details or of works like those of Luys and Meynert, which are so much household words among cerebral anatomists that I thought a mention of their exact titles a useless affectation.

On reviewing the matter, I find that the relations are in every respect as Dr. Wilder gives them, viz.: 1st. That the lateral and third ventricles only communicate through the Foramen of Monro. 2d. That the medial aspect of the Thalamus only is in the third ventricle. 3d. That no part of the Thalamusis in the lateral ventricles. 4th. That the dorsal aspect of the Thalamus is neither in the third nor in the lateral, nor in any other ventricle.

My description of the third ventricle as a T shaped slit, requires this modification, that the vertical branch alone is ventricular, the horizontal being neither a part of the lateral nor of the third ventricle, but a *fissure* extending between the thalamus and the cerebral hemispheres.

While thus embryologically and morphologically the thalamus must be looked on as a distinct cerebral segment, yet in the higher mammalia (as I claimed some years ago) it has become practically a subsidiary ganglionic depot of the cerebral hemispheres, and thus assumes a position not without its analogies to that of the *Corpus striatum;* a view in full accord with that of Meynert.

In conclusion I would offer a formal apology for the erroneous interpretation I made of Dr. Wilder's views, and state that the communication I made was merely provisional, and intended to be followed up by more full essays on the different subjects mentioned therein, in which the necessary literary references will not be wanting.

I also trust that Dr. Wilder will, in his publication, enlighten us on the anterior termination of the "extraventricular" slit which extends over the thalamus and under the Fornix. I am unable to determine this question from my sections, which are chiefly made in the transverse direction, and in which the ependymal roof of the third ventricle was either absent or removed by the knife. This feature was in part responsible for my misapprehension of the shape of the third ventricle. Respectfully,

NEW YORK, February 3d, 1881. E. C. SPITZKA.

RECURRING PERIODS IN THE WEATHER.

To the Editor of "SCIENCE:"

Last year I published in the New York *Tribune* a table for the month of February showing the dates of rain and snow in that month for twenty years. I also pointed out that there seemed to be two well marked storm periods, about the 3rd and 20th of that month, when a storm might be expected. Since then I have made some corrections in the dates as then published and have extended my investigation to all the rest of the months. The result will be found in the table below; the table gives the dates of rain or snow as observed at Newburgh, N. Y. In all cases the blanks indicate that I had no observations for those dates. I would be very much obliged to any of the readers of "SCIENCE" who may have observations for these missing dates if they would publish them in "SCIENCE." For example, did it rain about 3rd of August 1862, May 1877, August 1873, August 1874, August 1876 and 1877, or about September 3, 1869? The missing dates from the 20th I would also be very much pleased to have supplied. An examination of the table will show that there seems to be two well marked storm periods in each month, and that a storm may be expected about the 3rd and 20th of every month. The crucial test for any theory concerning the weather is to see how accurately it predicts for any one place. It will readily be comprehended that the larger the area we predict for, greater is the probability of our predictions coming true, for if it does not storm at one place it may at another. Had I felt justified in using observations made in the vicinity of Newburgh, the table would have agreed much closer with theory, but I have only used the dates on which storms occurred at Newburgh. Besides, when the storm was very light near the 3rd or 20th, I have in some cases omitted it and recorded the date of the next storm. Thus, for example, it snowed at Newburgh December 21, 1880, but it did not last long, and I have recorded the heavier storm of December 25th; and in September, 1864, it rained before the 30th, but as it was very light, I omitted it and recorded the first rain of any amount after the 20th. However, my object will be accomplished if this article should direct the attention of observers to the importance of paying more attention to the recurring periods in the weather, on which Vennor has founded his predictions which have proved so true in many instances.

JAMES H. GARDINER.

NEWBURGH, N. Y.

YEAR.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
1860	3 26	3 18	1 20	1 21	3 19	1 20	3 21	4 21	8 21	2 14	6 18	4 20
1861	3 20	3 20	3 19	3 24	1 25	2 19	6 20	8 22	3 18	5 19	2 21	1 23
1862	5 20	3 19	3 20	2 20	2 19	3 19	5 20	.. 22	1 17	4 19	6 19	3 22
1863	2 21	1 20	3 25	4 23	4 17	2 21	2 20	8 22	12 19	3 19	3 19	3 22
1864	1 19	1 24	2 20	2 25	3 21	1 20	2 25	3 21	4 30	2 28	4 21	3 21
1865	4 19	5 26	3 21	6 20	1 18	9 19	1 21	3 21	2 18	4 19	4 21	10 19
1866	12 20	2 19	4 20	7 22	2 20	3 22	4 20	2 21	4 20	14 22	11 22	7 20
1867	1 20	3 20	3 21	5 20	6 19	2 18	4 20	3 20	5 19	5 21	11 20	4 19
1868	1 21	9 19	2 21	2 20	2 21	2 21	3 24	3 20	3 20	1 21	2 18	4 20
1869	4 18	3 18	2 19	1 20	3 19	1 20	3 26	4 21	.. 25	3 28	5 20	5 20
1870	5 17	5 20	4 21	4 21	7 24	7 ..	3 28	4 25	3 17	11 20	3 19	4 21
1871	2 20	8 18	3 21	4 20	3 17	3 20	4 22	5 ..	3 19	2 18	1 22	5 19
1872	3 19	3 25	4 23	1 21	4 19	4 25	4 18	6 21	5 19	3 20	3 20	3 20
1873	2 20	3 21	2 20	3 21	3 21	6 23	1 19	.. ..	.. 19	4 20	7 18	2 18
1874	2 21	3 20	4 22	4 19	3 21	3 20	2 ..	.. ..	.. 19	2 21	9 20	1 20
1875	2 18	3 20	3 20	3 18	2 19	7 18	4 18	3 19	10 19	1 16	2 19	5 21
1876	2 19	3 21	1 20	3 20	4 20	3 20	2 20	.. 17	1 21	5 21	6 20	2 21
1877	2 22	2 16	2 21	2 20	.. 21	5 21	1 17	.. ..	6 28	4 21	2 24	5 21
1878	2 20	1 21	3 18	2 ..	1 20	4 22	4 22	1 20	3 21	6 19	6 20	2 21
1879	2 21	5 20	4 21	3 19	5 19	3 22	4 22	4 19	3 22	9 19	2 19	4 19
1880	4 20	3 21	2 20	3 20	2 22	2 20	3 20	3 21	7 20	4 22	5 20	5 25
1881	4 21											

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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It appears to be a fact not generally known in the United States that a prize is annually offered by His Majesty, the King of the Belgians, amounting to the sum of twenty-five thousand francs, for the encouragement of intellectual effort.

The intentions of the King were made known by a decree dated the 14th of December, 1874, inviting the authors of all nations to compete, and placing the settlement of the award in the hands of a jury appointed by His Majesty, composed of seven members, three of whom must be Belgians, and four foreigners of different nationalities.

The prize for the year 1881 will be awarded "to the best work on the means of improving ports established on low and sandy coasts like those of Belgium."

The original time for sending in these essays, which may be either printed or in manuscript, was the 1st of January, now last past, but we are authorized in stating that the time has been extended to the 31st day of March, 1881.

Foreigners desiring to compete for this prize are required to send their works to the Minister of the Interior at Brussels; but Mr. John Eaton, U. S. Commissioner of Education, advises competitors in the United States to forward their articles through the Department of State at Washington.

We are informed that the manuscript work obtaining the prize must be published in the course of the year following that in which the prize shall have been awarded, but in what manner the publication shall be made is not stated in the document placed in our hands.

Engineers and scientific men who would avail themselves of this opportunity must act promptly, and we would advise such to apply directly to Mr. John Eaton, of the Bureau of Education, in regard to any further information required for facilitating their work.

CHIMPANZEES IN NEW YORK.

THE last of the Chimpanzees at the New York Aquarium died on the 2d of February, of a throat affection. It was a remarkably well developed specimen. Its principle dimensions were, height (when standing) from heel to vertex 33 inches, distance from coccyx to vertex 20½ inches. Length of foot 6½ inches. Length of hand exactly the same. Its weight was twenty-four pounds. The brain was obtained by Dr. Edward C. Spitzka, making the third brain of this species in his possession. New York has been comparatively rich in anthropoids during the past three years. At one time there were five Chimpanzees and one Orang-Outang on exhibition together. The former lived about nine months. Altogether there have been at different times nine Chimpanzees at the Aquarium. Of the first pair, "Nip" and "Tuck," the former died of a tubercular meningitis, the latter passed successfully through an attack of Enteritis and later of Diphtheria, to die at Coney Island. A comparatively large animal standing over 3½ feet high, died of neoplasm in the lung. A female of depraved propensities such as have not yet been noted in anthropoids (devouring her own excrement), and a little two year old, one of the finest and most active anthropoids yet kept in captivity, died of catarrhal affections contracted at the seaside Aquarium, whither supposed business interest had directed they should go. Two well-developed animals, aged over two years, were sold to the Philadelphia Zoological Gardens.

A single survivor remained at the Aquarium. This animal had been in excellent health for a year and grown considerably during that period. About two years ago a new specimen arrived which had been brought from Africa, after a very stormy voyage, in a sailing vessel; it looked shrivelled and shrunken, weighed nine pounds, and was not expected to live. Those who saw it remarked that it bore the same relation to the other that a starved inmate of a baby farm does to a healthy, well-nourished child. But after a year it had outstripped its comrade in growth, and altogether gained fifteen pounds weight in the two years of its life of captivity. There must be considerable disparity between individual anthropoid apes in a state of nature, and this observation seems to confirm it,

TRICHINÆ IN PORK.

Dr. Ed. W. Germer, Health officer, of Erie, Pa., sends to us a portion of trichinous pork, as a sample of meat which infected a family of seven persons with trichinosis. The pig in question was raised with another, both being fed with the same food and reared under the same conditions. The pigs were killed at the same time, and an examination by Dr. Germer showed that one of these pigs was infected with trichinæ while the other was free from the parasite.

The owner of the diseased pig, his wife and two children were all taken sick simultaneously, and were treated for typhoid fever. Later three persons visited the house and were all seized with the same symptoms. The attending physician attributed the trouble to a well which supplied the family water. The mystery was solved by Dr. Germer who made the discovery of trichinous pork, and under his treatment the patients recovered. Dr. Germer suggests the possibility that many cases of trichinosis occur which are treated for other diseases, and trusts the time is not distant when young physicians will purchase a microscope before buying a gold watch or a gold-headed cane. We have examined the sample of trichinous pork, and confirm Dr. Germer's report; stripping a portion of the sarcolemma from the muscle we found seven trichinæ in the field of the microscope, using a ¼th objective. The trichinæ were in a free condition without cysts, and very transparent; for this reason they could be seen only by making very thin sections,

and would probably have been passed over by one making a careless examination. The Medical *Presse* of Vienna reports 80 cases at one town and 40 at another city, and the more recent fatal cases on board the British School ship *Cornwallis* would appear to suggest the importance of an official examination of all pork to be used for food.

PHILOSOPHICAL SOCIETY OF WASHINGTON.

At the one hundred and ninety-second meeting of the Philosophical Society, of Washington, a very interesting communication was read by Prof. J. W. Chickering, entitled, "Notes on Roan Mountain, North Carolina."

The Appalachian chain with its undulating line of 1300 miles from the promontory of Gaspé on the Gulf of St. Lawrence to Georgia and Alabama, beginning as a series of folds of moderate height, increases in complexity and altitude from north to south, attaining its greatest elevation in the Black Range of North Carolina. Following it from Gaspé to the Hudson we find the single chain of the Green Mountains reaching its extreme height in Mt. Mansfield, 4430 feet; the outlying cluster of the White Mountains with Mt. Washington, 6288 feet, and others exceeding 5000 feet; Mt. Katahdin, in Maine, about 5200 feet; the Adirondacks, with Mt. Marcy, 5379 and the Catskills considerably lower. From the Hudson to New River, Va., a distance of 450 miles, it gradually gains both in width and altitude. It consists of many parallel ranges with fertile valleys between, of which the great Valley of Virginia is the largest and best known, and all in reality a part of that Piedmont region. In Pennsylvania the summits vary from 800 to 2500 feet. Towards the south the chains become more numerous and indented, and in Virginia the Peaks of Otter reach 4000 feet. The extreme eastern range is called the Blue Ridge, the extreme western the Cumberland Mountains, or more properly *plateaus*, while the high range or ranges between is in general called the Alleghenies.

From the New River southward, the system becomes more complex. The main chain hitherto called the Blue Ridge is deflected to the west, and for 250-300 miles in a circuitous chain under the names of Iron, Stone, Bald, Great Smoky and Unaska Mountains joins the boundary between North Carolina and Tennessee, rising frequently to a height exceeding 6000 feet. The more easterly range retaining the name of Blue Ridge, having its southern terminus in Caesar's Head in South Carolina, turns abruptly to the northwest and reaches even loftier altitudes, Mitchells Peak being accredited with 6717 feet. In North Carolina these two ranges are more than 50 miles apart, are partially connected by transverse ranges, and for more than 100 miles constitute a great central plateau like that of Colorado on a small scale.

The eastern chain or Blue Ridge is still the watershed, and on the Atlantic slope gives rise to the Roanoke, Catawba, Broad, Saluda and Savannah rivers. On the other side, this area of mountains and plateaus is separated by transverse chains into many deep basins. At the bottom of each runs one of those mountain streams, the New Watanga, Nolechucky, French, Broad and others. These are compelled to cut their way to join the Tennessee through gaps, gorges and defiles in the heart of this great chain, giving us some of the most picturesque scenery to be found on the continent.

In the midst of this region with all three ranges in sight stands Roan Mountain (a Laurentian mass), the State line crossing it at an altitude of 6391 feet. I desire to call attention to some of the peculiarities of the region as contrasted with the northern Appalachians.

Standing upon the summit of Roan we look into seven different States, and command a horizon of 30 to 80 miles. On the north and west the eye catches the Cumberland Range on the horizon, and in the interval the great Cumberland plateau, and and many other ranges, but all as level as if designed for railroad embankments—sometimes not a peak to be seen in 40 miles of crest. On the south is a wilderness of mountains. Guyot gives fifty to sixty with altitudes exceeding 6000 feet, and yet the highest is only 6717, and perhaps forty of them between 6000 and 6500, and hundreds of others 5000 +. The valleys rarely go below 3000 feet. The railroad after leaving Lynchburgh in a few miles reaches 1000 feet, and from that point for nearly 300 miles rarely goes below 1500 feet, and at one point reaches 2550. The true Piedmont region, extending through to Virginia, North and South Carolina, Georgia, Alabama and Tennessee, at an elevation of 1500 to 2500 feet, offers as attractive a region for health and comfort as can be found on the globe.

Uniformity of temperature. During nine weeks the mercury indicated once 75°, seven times 70°+, once 45 , three times 50°—, the general daily variation being between 55° and 65°. The spring a few rods from the hotel, has a temperature of 45°. Equally remarkable was the uniformity of atmospheric pressure, the highest barometer being 24 19, and the lowest 23.87. No wind had a velocity greater than 20 miles an hour, and seldom reached ten miles. The last time I was at Mt. Washington, in August, the mercury was 36° and the wind 40 miles.

Fertility of summit. Instead of the upper 1000 feet being, as in most of the higher northern peaks, a pile of barren rocks with lichens their only vegetation, the summit of Roan and many other peaks is a smooth grassy slope of the most vivid green, dotted with clumps of *Alnus viridis, Rhododendron Catawbiense*, the soil one or two feet deep and black. How this amount of humus was accumulated, and what cause destroyed the forests which its existence seems to indicate as formerly existing are questions not easily answered. The valleys are very fertile and adapted to almost any crop.

At an elevation of 3000 to 4000 feet occurs a belt of the most magnificent forest trees I have ever seen. Hundreds of chestnuts, sugar maples, lindens, tulip trees, yellow beeches, and buck-eyes were seen from four to seven feet in diameter, and rising 70 to 80 feet without a limb. One chestnut measured 24 feet in circumference, and one black cherry 19 feet. Thorn bushes were as large as apple trees, and with dwarf buck-eyes and yellow beeches looked like old orchards of vast extent in the higher levels.

Flora. Ascending the mountain, the vegetation takes on a northern aspect. Hemlocks abound till near the summit, where they are replaced by Abies Fraseri, the characteristic spruce of these summits. Anemone nemorosa, Oxalis acetosella, Rubrus ordoratus, Aster acuminatus, Habenaria orbiculata, Ribes lacustris and prostratum, Veratrum viride, Lycopodium lucidulum and similar species, remind one of the woods of Maine and New Hampshire. The peculiar flora of the upper 1000 feet greatly resembles in habit those of the White Mountains, but very few are of the same species. Paronychia argyrocoma, Alnus viridis, and a species of Lycopodium are almost the only plants occurring to me as common to the two localities. Anemone Grœnlandensis is replaced by A. glabra; Solidago thyrsoidea by S. glomerata. The species peculiar to these mountains in general are hardly sub-alpine, and thus continuous with similar species further north but rather apparent instances of local variation, many species being confined to very narrow localities. The same is true of the molluscs. On Mt. Washington, a few rods will sometimes give the same plant in bud, flower and fruit, as a north or south exposure, a precipice, or a snowdrift, may retard or accelerate growth. But on these southern mountains no such difference obtains any more than in the valleys below.

On this communication Professor J. W. Powell remarked that the uniformity of altitude of the peaks is a a feature resulting from the fact that the mass out of which they have been carved by erosion possesses a plateau structure. The elevation of that region was dis-

tributed in its effects with an approach to uniformity over a wide extent of country, and was unaccompanied by those sharp flexings or the protrusions of abrupt granitic cores which are encountered in some portions of the Appalachians and other mountain regions. The individual masses and ranges in the Cumberland region are the work of erosion acting upon a broad platform, excavating wide valleys and narrow gorges, leaving the peaks and ridges as cameos and mere remnants of the general degradation of the entire region. Professor Powell exemplified the process by citing the Uinta Mountains as a broad platform similarly carved by an enormous erosion.

Mr. Lester F. Ward then read a communication entitled, "Field and Closet Notes on the Flora of the District of Columbia." Mr. Ward's paper was more comprehensive than its title indicated. He read extracts from a local monograph which he has been preparing on the Flora of the District of Columbia. The work has been done by Mr. Ward in his usual energetic, thorough, and philosophical manner, and presents many points of interest. It will be published in full by the Society.

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

The Society met in the lecture room of the National Medical College on Tuesday evening, February 1, Major J. W. Powell in the chair. By the provisions of the Constitution the retiring President is required to deliver his annual address at the meeting succeeding that held for the election of officers, and to review therein the work of the Society during the past year. As before mentioned, the reasons for the publication of elaborate proceedings, existing in the case of other societies, do not obtain here. The President, therefore, in connection with his address, had prepared a pamphlet of 100 pages, in which were embodied abstracts of every paper read during the two years of the Society's existence, together with a brief history of its formation, the two annual addresses, the constitution, and the list of officers and members. The whole constitutes a very important contribution to knowledge.

Major Powell thus presented a classification of the papers and discussed the several subjects treated in their order, namely: Archæology, ethnography, linguistics, biology, philosophy, technology, sociology, and mythology. As the address will appear in full as a part of the pamphlet, it is not necessary to present an abstract.

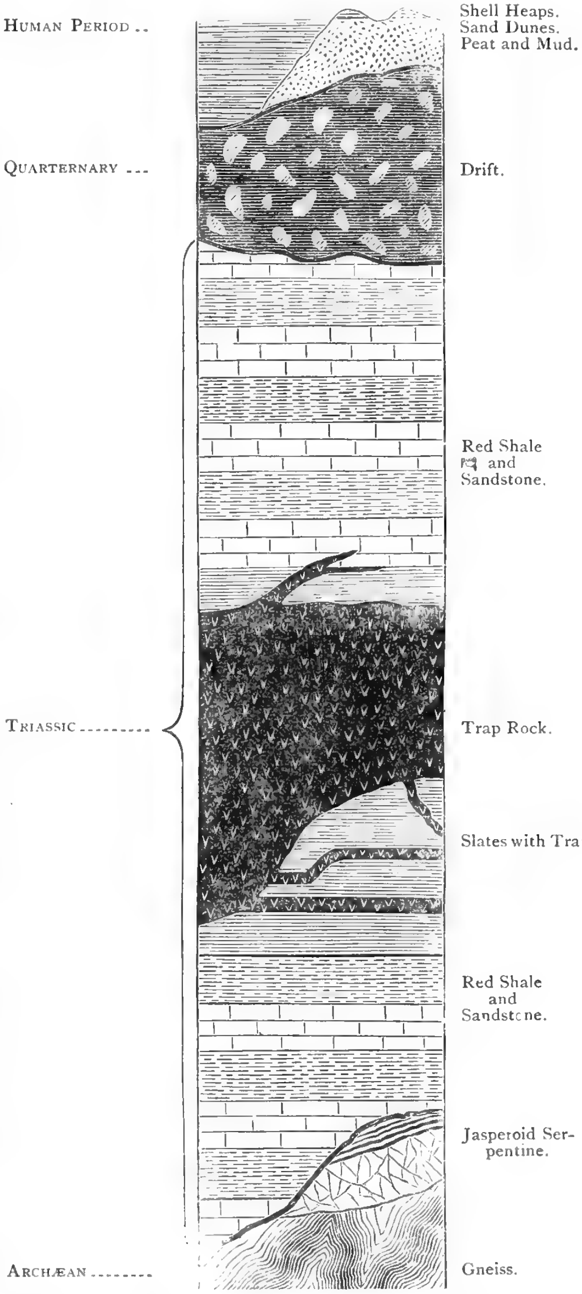
Determination of Gold and Silver in Alloys, after Quartation with Cadmium.—Two portions of the alloy, each of 0.25 grms., are weighed off and placed with the cadmium in small porcelain vessels. A piece of potassium cyanide is melted in a porcelain capsule over the flame, and the metal thrown in. The melting together takes place readily, and is complete in a few minutes. By changing with two or three porcelain capsules, and having a vessel with warm water at hand, in which the melted portion is dissolved when sufficiently cool, twenty to thirty meltings can be executed in an hour. The two metallic granules are now thrown together into a small long-necked flask, in which is nitric acid of sp. gr. 1.30; a piece of wood charcoal is introduced to prevent bumping—which would rupture the globules—and heat is gently applied. The first solution lasts rather long, according to the proportion of gold; *e.g.*, an hour in case of fine gold. The solution is poured off, the boiling repeated with nitric acid of sp. gr. 1.3 for ten minutes, the liquid again poured off, the globules rinsed with hot water, boiled for five minutes with water, which is poured off, and the flask filled with water is inverted into a porous earthern crucible, dried, ignited strongly, proceeding as in cupellation. In most cases the globules can be weighed separately. Silver is determined in the solution of titration with ammonium sulphocyanide according to Volhard's method.—Fr. Kraus.

A SKETCH OF THE GEOLOGY OF HUDSON COUNTY, N. J.*

By Israel C. Russell.

An outline of the geology of Hudson County, N. J., is delineated in the accompanying generalized section.

Fig. 1—Generalized Section of the Rocks of Hudson County, N. J.



At the base of the series is crystalline gneiss of Archæan age, which is exposed in a few reefs along the shore of the Hudson in Jersey City. These rocks are composed mainly of quartz, feldspar and mica, and form highly crystalline gneiss, mica schist, hornblende schist, etc., and are not to be distinguished from the rocks of

* Taken from a paper published in the Annals of the N. J. Academy of Sciences, Vol. II., No. 2, pp. 27-80.

the same formation exposed so abundantly on Manhattan Island.

Associated with the crystalline Archæan rocks that to a limited extent border Hudson County on the east, are beds of quartzite and serpentine, exposed in the bluff known as Castle Point at Hoboken. This premontory is about thirty acres in area, and is limited on the east by bold bluffs of serpentine. The rock here exhibits considerable variety, being sometimes yellowish and dull in appearance, and so earthy as to crumble between the fingers; again it is compact, dark green in color, and furnishes an ornamental, although inferior, building stone. This rock is a silicate of magnesia containing chrome-iron in scattered grains, and furnishes also the minerals *marmolite, brucite, nemalite* and *magnesite.*

The quartzite or jasperoid rock, occurring on the southern slope of the serpentine, in the neighborhood of the Stevens Institute, has, together with the serpentine, been referred to the Archæan series, but as the exposures are now obliterated little can be said concerning it.

TRIASSIC ROCKS.

In Hudson county we have a portion of the eastern border of the Triassic formation which forms a band thirty miles broad across the State. In general with the Triassic formation in New Jersey and the Connecticut Valley, the rocks are here felspathic sandstones, slates and shales traversed by sheets and dikes of trap. The sedimentary rocks occupy nearly the whole area of the county and dip uniformly to the northwest at an angle of about 15°. The sandstone is largely composed of granules or fragments of felspar, cemented by oxide of iron to which the reddish or brownish color of the rock is due; this is the stone so largely used for architectual purposes in New York and the neighboring cities. Traversing these inclined beds of sedimentary rocks, and in a general way conformable with them, are sheets of intrusive trap, which now owing to unequal erosion, form the most prominent features in the topography of the county. This statement holds good, also, for the entire Triassic area in New Jersey, and with more or less accuracy for this formation in general along the Atlantic slope. The main trap ridge in Hudson County, composing the highland known in different portions of its course as Bergen Hill, Jersey City Heights, and the Heights of Weehawken, is continued northward with increasing height, and forms the bold picturesque shore of the Hudson as far northward as Haverstraw. The outcropping edge of the trap, especially in Hudson County, has been abraded by glacial action so as to form an irregular, badly drained, plane surface. Although in a general way following the bedding of the associated slates and sandstone, the trap sheet is really unconformable to them and breaks across their bedding in various places. From both the upper and lower surfaces of the main trap sheet smaller sheets and dikes of molten rock have been intruded among the stratified beds. Examples of these branches from the principal mass may be seen at the base of the cliffs along the west bank of the Hudson, from Hoboken northward. Secondary sheets originating from the upper surface also appear on the western border of Bergen Hill, where they have been accented by erosion. The intrusive nature of the trap sheets and dikes is shown by their crystalline structure, their unconformity to the inclosing stratified beds, and by the metamorphism produced in the strata with which they have come in contact. A section exposed in the cliffs bordering the Hudson a few miles north of Hoboken, is given in the following figure, and illustrates especially the abrupt manner in which the New Jersey Triassic area is cut off along its eastern border.

In the diagram D represents the sheet of drift that covers the eroded surface of the hill, and S the slates that unconformably underlie the trap into which a small

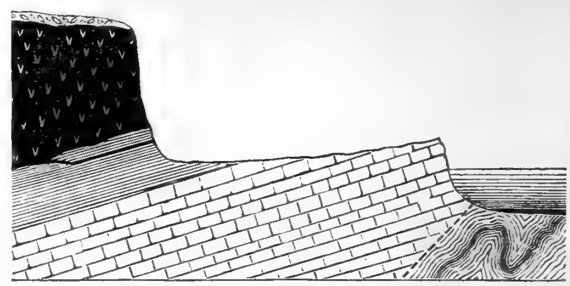


FIG. 2—SECTION AT DOG'S POINT, WEEHAWKEN.

secondary sheet of the crystalline rock has been intruded. Beneath the slates are beds of light colored felspathic sandstone ending in a cliff at the water's edge; the whole series has the usual dip of 15° N. W.

The irregular line formed by the eastern boundary of the trap is caused, at least in two instances, by sheets of trap that leave the main mass at an angle and stand out in ridges tangent to the principal line of cliffs; examples of this feature, which is difficult of description withou illustrations, may be seen at Kings Point, Wehawken and Fairmont Hill, Jersey City.

POST-TRIASSIC HISTORY.

No records are found in Hudson County, of the Jurassic cretaceous or Tertiary periods during all these geological ages; the area under discussion must have been a land surface exposed to subaerial denudation. The same destructive agencies were at work, too, with accelerated energy during the Quaternary period. The result is that we have but a decimal portion of the original Triassic formation remaining.

During the Quaternary, northern New Jersey, in common with a great area in the northeastern part of the continent, was buried beneath glaciers of great thickness. In Hudson County the ice-sheet moved from north-west to south-east, ploughing out in its journey the soft Triassic shales and sandstones, and grinding off the projecting ridges of trap with a force so irresistable that a mountain-ridge like the Palisade range could not deflect it from its course. When the climate ameliorated and the glaciers retreated northward, the hard crystalline trap was left with a polished surface that glitters in the sun light, and is crossed by deeply engraved lines that faithfully record the direction from which the glaciers came. In places the rock rises into smoothly rounded hillocks, forming typical *roches moutonnées.* The load that the glaciers carried was spread over valleys and uplands, forming a continuous sheet of glacial drift, which now composes the immediate surface of a large portion of the county. This glacial drift is generally a tenacious clayey deposit, at times fifteen or twenty feet thick, of a reddish color, derived from the debris of Triassic shalas and sandstones that enter largly into its composition. Scattered through it are boulders of trap, sandstone, slate, etc., that have been transported but a short distance, and others of gneiss and conglomerate, the parent ledges of which are thirty or forty miles to the northwestward. Hudson county also furnishes examples of modified drift, consisting of irregular layers of sand, gravel and small boulders, all well rounded and plainly assorted and deposited through the agency of running water.

More recent than the glacial deposits are the sand dunes that skirt the base of the upland on all sides. Again more recent than the hills of aeolian sand are the many deposits of peat and mud still in process of formation along nearly the whole water-front of the county.

The bed-rock in Hudson county is, in most places, excepting on the uplands, deeply covered by Quarternary and recent deposits. The topography of the rocky floor of the county and of the neighboring portions of New

York, would not only be of great interest to the geologist but of direct economic importance to all interested in shipping, harbor improvements, reclamation of land, etc. The records of deep wells and soundings in the salt marshes that have a bearing on this subject are tabulated in the paper published by the Academy. On the Newark Meadows and in Newark Bay the rock bottom is from two to three hundred feet below the present surface. East of Bergen Hill soundings show a depth to rock nearly as great. The following list taken from the tables mentioned above, give some of the soundings on the borders of the deeply eroded channels of the Hudson, East and Harlem rivers :

Hudson River, foot of 23d st., 250 feet from the east building line of the river street	175 ft. to rock.
Hudson River, foot of Bethune st., 20 ft. W. of bulkhead line	176 ft. rock not reached
Hudson River, pier 60 (old No.), 20 feet W. of bulkhead line	175 ft. to rock
East River, N. Y. Tower of Brooklyn Bridge	107.4 ft. to bed rock
East River, Brooklyn Tower of Brooklyn Bridge	88 " "
East River, pier 41, N. Y , 200 ft. from the building line of South st.	91 " "
East River, pier 18, 200 ft. from the building line of South st.	60 " "
Harlem River at High Bridge, centre of river	70 ft. rock not reached
Harlem River, Madison av. Bridge, centre of river	75 " " "
As shown on the Coast Survey Charts of New York harbor, the water in the Hudson off Castle Point is	50–65 ft. deep
In East River, W. of Blackwell's Island	107 " "
In East River, at Hell Gate	121 " "
" " near Ward's Island	170 " "
In New York Harbor	60–80 " "
In the Narrows	60–116 " "
In the Kill Von Kull	25–54 " "
In Arthur's Kill	20–35 " "

These measurements, none of which give the maximum depth of the old channels, clearly prove that the drainage system about New York was at no very distant time several hundred feet below the present water surface. It might be shown with equal certainty that we are living many thousands of feet below what would have been the surface of the county had there been no erosion.

THE SOULS OF PLANTS AND ANIMALS.

By the Rev. Dr. Thomas Hill.

The only things concerning which we can arrive at absolute certainty are space, time and spirit. Their existence and some of their attributes are announced in every act of self-consciousness. Their existence and attributes are not matters of inference, but of direct sight. Matter, on the other hand, can substantiate its existence only by inference from these primal truths of space, time and spirit. All natural sciences are matters of mere deduction from the data furnished by mathematics and mental philosophy. All the business of life, (our manufactures, commerce, history), relating primarily to material things, rests in the same way, ultimately on truths of space, time and spirit ; that is on mathematics and philosophy. The conclusions at which we arrive in the historical and natural sciences are therefore more or less probable ; and the probability may reach a degree that is practically indistinguishable from certainty. I am practically as sure that this sheet of paper would burn if I held it in the gas jet, as I am that two straight lines cannot enclose a space. Nevertheless the first truth is a matter of contingency and probability, the second of absolute knowledge. These truths of absolute certainty, of direct intuition, concerning space, time and self-conscious mind, are not contingent ; they remain true, though heaven and earth pass away, and the perception of them is that which puts the stamp of immortality on the human mind.

But in addition to these fields of direct sight, the three fields of truth outside the conscious mind, are of the highest value. In the first place the certainty of the existence of other minds, is as near absolute certainty as it is possible for a truth of inference to be. That there are other men about me, and there is an Infinite Mind above us all, are truths which are practically as certain as the axioms of geometry. In the second place my fellow men are acting and have been acting, thinking, writing, painting, composing, legislating, warring and making peace, manufacturing and inventing for thousand of years ; and the study of their history is the richest and most fruitful method of developing my own powers, and learning to know myself. In the third place the field of space and time in which their history is cast is full of this wondrous matter, which gives them their opportunities, their means, their tools ; without it mental or moral life is inconceivable ; consciousness itself is awakened to activity only through contact with matter ; space and time are visible only through motion as a phenomenon of matter.

Here then is a great object of study, worthy of man's thought. Socrates was fearful lest Plato should spend too much time on questions relating to the measurement of matter ; Dr. Johnson in the Rambler carried Socrates's implied censure much farther than the old philosopher himself would have done. Swift in his voyage to Laputa satirizes the students of physical science ; the newspapers of our own day indulge occasionally in laughing at the technicalities of the scientific man ; even men as wise as the Autocrat of the Breakfast Table utter occasional words of disparagement in speaking of scientific pursuits. But Plato's geometry has done as much for the intellectual and purely spiritual development of our race as Socrates's morality ; and the physical philosophers of Europe, during the past three centuries, have, despite their own frequent ignorance of spiritual things, been of immense advantage to spiritual philosophy.

The relations of space are the earliest object of our scientific research. The first really intellectual ideas in a child's mind are those of geometric form. Hence all sciences that flow directly from geometrical relations are likely to be earliest developed. Mechanics preceded chemistry, and the classification of plants and animals by their outward forms preceded the knowledge of physiology, animal or vegetable.

Let us look then a moment at the geometrical study of material things, and see what it involves. Material forms suggest to the child the consideration of shape. He early learns to abstract form from the outward things and compare likeness in form only. He is but a few months old when the smallest drawing of a man, a dog, or cat, is recognized at sight. In a few years he takes the further step of looking by reason beyond the picture of imagination, and seeing the unimaginable realities in space itself. He conceives, for example, a sphere. But that portion of space which lies in a given sphere, surrounding a given point, has no properties by which it is distinguished from other parts of space. This is the Leibnitzian argument by which some modern writers would disprove the existence of space ; that its parts are indistinguishable and therefore coincident. But the geometer answers : No ! by an act of mind I seize upon any point of space and hold it as the centre of any sphere I wish to consider. When he has thus seized upon and considered a portion of space, bounded and separated from the surrounding space, by an act of his pure intel-

lection, he can communicate his thoughts to his fellow-men in either of two ways; first, scientifically by the medium of conventional symbols or language; and second, artistically, by a model or a drawing. This second method reaches a larger audience. What I write concerning a sphere, for example, can be understood only by those who have learned the language in which I write. But if I illustrate my propositions by good drawings or good models, the thought will be grasped by persons unlearned in mathematics—and persons of all nations, whether they understand English or not.

A form or model is therefore the clearest and most complete statement of a mathematical truth. Nor can a person give a more convincing proof of his understanding a truth than his ready and accurate drawing of an original diagram illustrating it. When we see material particles, the fine particles of crayon, for example, on the blackboard, obeying geometrical law, we recognize at once the expression of geometric thought. Law is a mental reduction of particulars to mental order. A geometric locus is a space in which each point is mentally referable to a single proposition; that is, a space in which the position of every point can be mentally grasped and defined by giving properly the position of one of them. When therefore we see numerous particles of matter conforming to a geometric locus, we are forced to believe that that mass of matter was moulded, directly or indirectly, by a mind which comprehended the law of the locus; and in the moulding of it, enounced the condition which defines the position of its points. The enunciation of a thought can come only from a mind comprehending that thought; and the formation of a geometric figure is the clearest enunciation of a geometric thought.

In the physical forces, therefore, which govern inorganic matter, is revealed the existence of a guiding intellect, since the forces are constantly producing perfect geometrical forms or leading to harmonious arithmetical relations. If we are forced to believe that gravity and chemical differences and chemical affinities inhere in matter, we must still, from the geometrical and algebraical powers exhibited by these forces, believe that they were bestowed upon matter by an intelligent power, who foresaw and comprehended their effects. The study of the natural sciences might otherwise well be given over to the reproach of Dr. Johnson in the Rambler, and Swift in the voyage to Laputa. Without a faith that law lies hidden in the material world, all the efforts of scientific explorers would be paralyzed; and without faith that that law is the choice of infinite wisdom, and adapted perfectly to fulfil the purposes of infinite love, the success of the scientific explorer in discovering it would be robbed of its highest and peculiar value.

In fact, the physical forces governing inorganic matter, acting under definite laws, and tending towards a state of stable equilibrium, nevertheless show in the intellectual, the geometrical and algebraical nature of those laws, a spiritual origin; they show that, however independent of will we may now conceive them to be, they nevertheless are the embodiment of thought; and we know of no way of embodying thought without a volition.

But if the Creator has thus stored in crystallizable matter, in a manner transcending all our thoughts, forces which carry out His geometrical and algebraical conceptions, much more marvellous and beautiful are the modes in which he has imparted to the souls of plants and animals (if I may thus extend the use of the word soul) the power of carrying into execution more complicated geometrical and algebraic plans.

For in every organized being, plant or animal, there is a guiding principle which we may call, if we please, a soul, which causes the forces of matter no longer to act under laws tending towards stable equilibrium, but under a variety of laws, different in each species, tending not towards stable but towards unstable equilibrium. This guiding principle has in itself no forces. The most careful investigators of the phenomena of organic growth fail to find any evidence of vital *force*, although there is abundant evidence which must convince the most careless observer that there is in organized growth a vital *guidance* of force peculiar to each species of plant or animal, which I cannot conceive as inherent like the physical forces in matter and which I therefore, must attribute either directly to the Deity, or to an animal or vegetable psyche, empowered by him to carry out these higher geometric forms, just as in each species of matter he has implanted the ability to carry out the simpler geometrical forms of crystals.

There are those whose philosophy differs from mine, and who hold to the opinion that the vital guiding principle in organic growth, and even the rational soul of men and animals are inherent in matter, in the same manner as the forces which they guide are inherent in matter. According to this philosophy the vital principle and the rational principle, inherent in matter, are usually latent and are brought into operation only under peculiar combinations of circumstances.

Now even should we grant the soundness of this view, I should still find it necessary, for the explanation of the logical series of vegetable and animal forms, to suppose that this universally diffused vital principle originally sprang from an intelligent self-conscious being who comprehends the laws of geometry perfectly; and who has expressed certain of his geometric thoughts through the psyche or soul of plants and animals, whether we suppose that psyche diffused but latent through all matter, or confined to the organisms in which it is patent.

The nature of this psyche, of course, transcends our knowledge. We recognize it only through its operations; and consciousness aids us in our attempts to understand it only so far as to show that its effects are intellectually identical in geometric form with the product of our geometric imagination. But we cannot suppose the psyche of the plant or animal conscious of all the thoughts which it develops, since we, whose psychical development is evidently vastly higher than that of any other terrestrial beings, are not conscious of the geometric law of our own bodies, which our souls unconsciously fulfill; not only during the period of our growth, but daily as we supply by nutrition the daily waste of the frame.

The fact that the plant or animal may, without the exertion of any mechanical force, guide the forces of electricity, and light and heat and chemical affinity, to the building of peculiar forms imprinted on its own soul, may receive a coarse illustration from the operations of the steam-engine in which, by delicacy and accuracy of workmanship, the direction given to the power can be changed by a force infinitesimal in comparison to the force exerted by the engine. The chemical forces which govern the organogens in their compounds are always the same, but the results vary for each individual plant or animal, and the law which those results indicate is different for each species. The forces, building out of these few simple elements so great a variety of forms, are tremendous in their energies, and their existence is forced upon the attention of the naturalist; but the vital force, if such a thing exist, which guides these tremendous powers and determines what result they shall bring to pass, has eluded the sight of the most careful and accurate observers. The nicer the investigation into the phenomena of organic form, the more wonderful do the results appear. The persistence of type, for example, through successive generations for many thousands of years, and the very evident transmission of all physical and psychical characteristics from the male parent to the offspring, how utterly inexplicable upon any gross material theory, when we reflect that from the male parent no

matter passes into the offspring, excepting the fluid contents of a microscopic cell strained through its walls and through the walls of the ovule or ovum! Through this infinitesimal quantity of fluid, filtered through this infinitely close double filter, there passes, in some way, a law of form, and a law of mental and physical idiosyncrasy which is stamped upon the whole terrestrial being of the offspring, bending all the untold energies of gravity and of chemical and electrical attraction, to its own particular whim. Through the whole terrestrial life of the offspring do I say?—yes, and I may include in that word offspring sometimes the whole progeny for a thousand generations. The conscious part of the soul is still less known. Its presence is one of the characteristics of animal as distinguished from vegetable life, and the investigation of its comparative development in different tribes of animals is the most valuable part of the field of natural history.

The soul of the plant is presumed to be unconscious. The phenomena of motion in the sensitive plants, and in the efforts of all plants to throw their leaves to the light and their roots to the richest spots in the soil, are supposed to be as unconscious as the contraction and dilatation of the pupil of the human eye. In that dilatation and contraction there is action adapted instantly to circumstances, and so long as the eye is healthy, with unerring promptitude and accuracy. An observer of the human animal might quote this as evidence of the wisdom of man. But on further thought he might see in the very fact that the action is unerring, evidence that it did not depend on the conscious volition of a finite being. And we, men, know that it is a movement of which we are absolutely unconscious.

In like manner it is presumed by the majority of investigators, that all the movements of plants are made with absolute unconsciousness on the part of the plant, and that plants have in short no consciousness whatever, either of their own existence, or of the existence of a world about them. Now it does not follow that on this account the plant is to be studied in its physical relations alone. It has psychical relations also, of the greatest interest to a true enthusiast in botany. The gardener and the botanist constantly speak of the feelings and tastes of plants, and of their moral qualities—indeed some plants have been named from moral nature, as, for example, Rumex patientia, and Carduus benedictus, the patient dock and the blessed thistle—while others have moral epithets that have become as familiar as their names, as, for example, the modest violet and the flaunting poppy.

The geographical distribution of plants will, I think, be found to depend upon something which eludes our study of the external conditions; something besides that physical struggle for life which the English naturalists see in every part of the animal and vegetable kingdom, as though the poor in those kingdoms were oppressed by unjust and impolitic laws impeding the distribution of land, as they are in the kingdom of Great Britain. There is also what may be called a choice in the plants, not implying by that language any consciousness in the plant, but simply affirming that its flourishing here or perishing there, depends in a great measure upon an idiosyncrasy of constitution, making it sensitive to physical changes that can be measured neither by the thermometer, barometer, hygrometer, electrometer nor chemist's balance. The mayflower of New England, called elsewhere trailing arbutus, will adapt itself, when it chooses, to clay or sand, to deep shaded woods or to sunniest hillsides; and I never saw it so flourishing as once in a peat meadow over which it was slily creeping from a sand bank on its edge. But take the plant up with never so much care, and with never so large a sod of unbroken earth about its root, and transplant it where you will, and it is a hundred to one that it dies in a twelvemonth of a broken heart, pining for its old home. Some of these freaks have been explained by the discovery that some plants are semi-parasitic, stealing from the roots of others a part of their food, and therefore incapable of living except in the presence of their patrons,—but many remain yet unexplained.

It sesms to me, however, very plain that the souls of plants, that which makes the difference between a plant living in the forest, and the specimen in the herbarium, that which guides the forces of nature to the building of the plant, and which turns its leaves to the light, is worthy of study in all its relations. It is a depository of divine thought, deposited there ultimately for our instruction as one of its final causes and therefore worthy of the most careful attention.

The intellectual and moral development of animals is also doubtless governed by a plan. The difference between the dull oyster and intelligent, affectionate dog, is as much the result of a plan, or thought, of the Creator, as is the difference of their forms. The horse and the ox are as admirably adapted to domestication by their mental as by their bodily gifts. All the instincts of all animals are adapted to their organization and to the nature of the world and of other animals among which they are placed.

Even should we suppose that the mental power of animals is the result of their organization, that is to say, even if we should suppose that mental power is latent in matter, and simply rendered active by organization, we should be compelled, upon a thorough study of the mental development of animals, to admit that their souls can be classified upon a logical plan, just as their forms can be; and we should be forced to admit, that this latent soul in matter is capable not only of organizing matter according to a logically developed series of forms, but of eliminating out of its own totality separate minds in a progressive series logically connected.

The very great importance of this study of comparative psychology, of becoming acquainted with the mental and moral characters of animals, is obvious. Many attempts to found a science of comparative psychology have been undertaken. But the field is vast, and the progress of the survey slow. At the Baltimore meeting of the American Association for the advancement of Science, Dr. Weinland proposed a method for this new science, ingenious and sound, but by no means exhaustive. He lays down nine fundamental principles; first, that the distinguishing mark of an animal is its consciousness of an outer world; secondly, that this consciousness of an outer world is the fundamental principle of the soul of animals; thirdly, that the consciousness of self results from and is proportioned to the consciousness of the outer world; fourthly, the degree of psychical development can be judged from the degree of development of the consciousness of an outer world; fifthly, this may be judged from the development of the organs of that consciousness; sixthly, these organs are of three kinds, those receptive of sensations, reflective organs and the organs of voluntary motion; seventhly, we may depend in comparative psychology mainly upon a study of the organs of voluntary motion; eighthly, these motions may be divided into two classes, those which refer only to the animal himself, and those by which he would hold communication with other animals; ninthly, man stands at the head of all animals, since his voluntary motions are not only more numerous and perfect than those of other animals, but because through machinery he increases vastly the number of his organs, runs upon the locomotive, talks through the printing press and telegram, and shows us what is most distant through the telescope and stereoscope.

But it is impossible for us to understand any of the phenomena of consciousness save through an appeal to our own consciousness. The mere investigation of the

organs of an animal and its movements can give no true knowledge of the soul of an animal to one who is incapable of analyzing carefully the phases of his own consciousness; nor would the student who is the most thorough master of the analysis of his own thought and feeling, be able to understand the souls of animals, did not the human spirit contain in itself the germ of every power of every terrestial creature. The disposition to attribute to others and to animals the feelings which we should have, were we in their circumstances, although it may mislead the student both of human and of animal life, is nevertheless an essential to successful study. It is impossible for us to understand beings either higher or lower in the scale than ourselves, except as they in some degree resemble us. Our knowledge of ourselves must keep equal pace with our knowledge of other beings; else we have no knowledge of either.

To recapitulate: In the study of organized beings we find three principal departments, their anatomy, their physiology, and their psychology. Their anatomy deals with their forms, and with the forms of their parts; and these forms furnish in general complete data for their classification. Physiology treats of the peculiarly modified chemical action by which food is assimilated and made part of the living structure, and by which the various secretions are formed. And were not this a much higher and more difficult inquiry than the study of the forms, we might doubtless classify all plants and animals by the chemical likenesses and differences of their tissues and secretions. At present these characteristics are used in classification only as confirmations of the accuracy of the results obtained by form. Psychology deals with the souls of organized beings, with those principles that guide the chemical and mechanical forces in matter to the formation of the organism. The classification of organized beings by their forms is, in fact, in one sense, a classification by their souls by the psychical principles which are empowered to create the forms. But these unconscious souls have other functions than the creation of forms; they have besides this intellectual work, a sort of moral quality by which they select peculiar food and form peculiar products, and by which also they are aquatic or terrestrial, tropical, tender, hardy, arctic or alpine, &c. Then in animals we have, either in the same soul or in a second one, consciousness added to life, the powers of thought and feeling, desire and volition, and of knowing that they think, feel, desire and will, and these powers culminate on earth in the human race.

Matter is a storehouse of forces; in each atom slumber or rage the forces of attraction and repulsion, and also the moral qualities of chemical difference and identity. These forces, whether chemical or mechanical, act according to fixed laws, and tend towards a state of rest and of stable equilibrium. And they are all so correlated that each of them can be referred as forces, to one common unit, and shown to be capable of lifting such a weight so many feet a second.

But organized beings push always into motion, and their tissues and secretions are usually such that, in air of the same temperature and moisture as that in which they grew, they will rapidly decay the moment that life is gone. They are perhaps in chemical equilibrium; but it can hardly be called stable,—at least it is not stable enough to resist the very heat and atmospheric influences under which it was built. Yet there is no trace of any force in the organism thus compelling the forces of inorganic matter to act in this peculiar way, so different from their behavior when the organizing life is wanting.

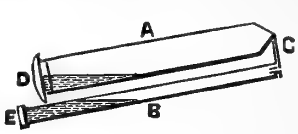
The intellectual power of the unconscious soul is not a force that can be compared with gravity, it cannot be measured by that unit; it does not act by attraction and repulsion, but simply guides (we know not how) the forces which do thus act—it rules them by moral or intellectual, not by corporeal power. The souls of plants and animals have a certain lordship over the earth, and the earth obeys their rule to a certain extent. This lordship is exercised in part involuntarily and unconsciously, that is in the phenomena of nutrition and growth; and in part consciously, in the phenomena of voluntary thought and motion and action. And had we sufficient knowledge of the habits of animals, we could doubtless classify them according to their voluntary life.

But in classifying organized beings, we do not find ourselves imposing law upon the series of species, but discover it already impressed upon them. Not only does the soul of the single organism develop thought, but in the whole gradations of the universe, from the chemical atoms up to the highest orders of mammalia, we find the development of more extensive thoughts; as though the whole universe had a soul; developing it as the soul of the violet develops its forms and color and odor. Now does this soul of the universe act consciously or unconsciously? Shall we take the vegetative power, or the conscious mind, as the type of the Deity? In endeavoring to find a symbol for the Highest in the universe, shall we look for the light of analogy into what is highest in ourselves, the conscious soul? or into what we have in common with the seaweed, the organizing power of life?

To me the answer is evident, that the highest of which we are conscious is the best symbol by which to speak of the Highest who is above our consciousness. Looking thus at the Divine Being as the Lord, who has consciously expressed His thoughts in the material world, that world becomes glorified and glows with heavenly splendor. Natural science becomes the study of the autograph works of an Infinite Author; and natural history—which is the highest of the series of physical sciences, and links them to the sciences that deal with the human mind and the works of man—becomes the means of communion with the highest geometrical, algebraical and chemical thought, which the Father of men has as yet revealed to us; and also becomes through the study of the instincts and reason of animals the fittest of all natural preparations for a study of ourselves, and of our own relations to the All wise and All good.

SIR W. THOMSON'S NEW DEPTH GAUGE.

Sir William Thomson has very recently patented another depth gauge which, though it depends upon capillary action, does not require the co-operation of chemical change. In fact, it operates by capillary action alone. The accompanying figure will illustrate the principle of this new device. Here A and B are two



glass tubes of different diameters united by a capillary tube C. The narrower tube, B, is closed at the end by a plug E, which can be removed at will: and the wider tube A is covered by a sheet of cotton cloth. This cloth acts as a porous septum which, when wetted, is permeable by water but impervious to air. For according to a law of hydrostatics, a film of water in a hole resists a difference of air pressure on its two sides, equal to the hydrostatic pressure due to a column of water in a capillary tube of the same diameter as the narrowest part of the hole. Thus it is that damp linen is impervious to air, and wet sails resist the wind much better than dry ones, as every sailor knows.

When this arrangement is lowered into the sea, water forces its way into the tube E, and the quantity forced into it during the descent becomes an indication of the depth when the relative capacities of the tubes are properly adjusted. In raising tne apparatus the water

in the wide tube is gradually expelled by the air, and the wet cloth secures that all of it will be driven out before any air gets in. The water contained in the narrower tube remains to indicate the depth by a suitable scale engraved on the glass, and then is let out by withdrawing the terminal plug.

For actual use the wide inlet tube is made of brass and the narrower tube of glass. Three sets of these tubes are combined into one instrument, and in each set there is a special ratio between the capacities of the inlet and retaining tubes, in order that the set in question may answer for certain depths. Flying soundings are usually taken in depths ranging up to 130 fathoms, and the three sets are designed to indicate depths, say, from 12 to 28 fathoms, from 28 to 60 fathoms, from 60 to 130 fathoms. They are fitted into a brass protecting cylinder, open at one end to the water, and slotted out in the sides to allow the engraved scales on the gauge tubes to be seen from the outside. The whole is then enclosed in a galvanized iron guard-case drilled with small holes to allow the sea-water to enter, and being attached to the sinker is lowered into the sea. The apparatus is manufactured by Mr. Whight, of Glasgow, for Sir William Thomson, and it has already been adopted on H. M. S *Valorous*, and the Russian imperial yacht *Livadia*.

While upon this subject we may also draw attention to the "nipper" lead of Mr. Lucas, engineer to the Telegraph Construction and Maintenance Company. The old plan of ascertaining the nature of the sea bottom, by bringing up a specimen of it in a tube, let into the bottom of the sinker and armed with tallow, is open to several objections. For instance, the specimen is apt to get washed out in rising to the surface, and when it is safely brought on board it is usually so smeared with tallow as to be objectionable. The nipper lead of Mr. Lucas, on the other hand, retains what it catches and renders it up in a pure state well fitted for preservation. The bottom of the lead or sinker in question is provided with two hollow claws or spoons, not unlike the mandibles of a crab. These are hinged to the sinker, and open out against the resistance of a stout spiral spring which is contained in the body of the sinker. When fully opened out they are kept apart by a locking device, consisting of two crossbars which meet end to end and fit into each other. The points of the open claws, however, in striking upon the bottom, spring this lock, and the claws snap together with great force, nipping up a specimen of the bottom at the same time, and from their hollow shape this specimen is retained. So effective is the nipper lead that the claws will nip a sheet of paper off a table, and they have been found to raise a specimen of the bottom from 2,000 fathoms.—*Engineering*.

BOTANICAL NOTES.

Every young naturalist needs to be on his guard against deception which is a frequent cause of serious mistakes.

Many strange species and unheard of peculiarities are sometimes discovered by the over zealous and credulous. Most imitations of natural objects are so bungling as to be readily detected, but occasionally something turns up which is such a surprise, that the fact is noted before its improbability is made evident.

The large springs of the limestone districts of Pennsylvania are exceedingly clear, cool and transparent. The principal plants living in them are species of Characeæ, and Veronica Americana, whose large, lettuce-like leaves have a very striking appearance when seen through the sparkling water. While visiting a spring one day in mid-summer, I was surprised to see some strange looking plants which appeared to be Marata cotula. Mentally noting this peculiar position for such a common and well-known weed of dry ground, I caught sight of something still stranger—a garden aster; another step and a zinnia and dahlia came to view. Indeed there was quite a garden "á immersion."

The small boys of the neighborhood had acquired the art of deftly binding flowering branches to small stones which held the plants to the bottom, while the strong upward flow of the water kept them neatly upright and life-like.

The search for plants upon vacant city lots, rubbish piles, and the like, always reveals a greater number and variety of species than one would suppose.

As several of these "local" floras have been published lately, I give one which interested me a good deal at the time of noting it. In Kingsford's Oswego Starch factory, large quantities of lime are used in the manufacture of corn starch. The refuse lime is a pasty mass still having to a considerable degree the caustic properties of fresh lime. Large quantities of it accumulate about the factory, and are hauled off to get it out of the way. Several hundred loads were once deposited in the middle of a pasture, in a loose pile varying from three to six feet in thickness. Cattle tramped over it carrying more or less mud upon their hoofs, and their droppings collected to a considerable extent upon it. In time plants began to get a foothold there, and one mild day in winter, about three or four years afterward, I visited it, and was surprised to find the following well established: Cirsium, 2 sp., Rumex, Poa, Phleum, Plantago, Graphalium, Verbena, Trifolium, 2 sp., Solidago, Marata, Chenopodium, Polygonum.

The white clover was especially luxuriant, and covered patches of several square feet with a perfect turf.

W. A. B.

A popular work on Algæ, by Rev. A. B. Hervey, to be illustrated with colored plates, is announced.

Professor Alphonso Wood, widely known as the author of a Class-book of Botany and other botanical text-books, died at his residence at West Farms, New York, on the 4th inst.

Trimen's *Journal of Botany*, despite its long standing and being without a rival in its chosen field, is obliged to make a call for a more liberal support in both subscriptions and contributions. This does not speak well for the enthusiasm of English systematists.

The second volume of *The Botany of California* has made its appearance. It includes the remainder of the Phanerogams not treated in Vol. I., the Pteridophytes, and the Mosses, and brings this eminent work to a successful close.

A new manual of the mosses of the United States will be published during the present year. The authors, Leo Lesquereux and Thomas P. James, are the most able and distinguished bryologists of America. The edition will not be large, and for the present the price is fixed by the publishers at $3.00. Such a manual has been needed for a long time.

In *The American Naturalist* for January, Professor Bessey calls attention to the Fly Fungi belonging to the genus *Entomophthora*. They have been but little studied. The most common species (*E. muscæ, Fres.*) infests the house fly. Dead flies are common in autumn covered with a white powder which fastens them to the walls and other objects of the room. Upon examination the bodies are found to be filled with the mycelium of the asexual stage of the fungus, the white powder being the conidial spores. This asexual form is described in many books under the name *Empusa*. The sexual stage developes entirely within the host, filling it with a mass of oöspores and hyphæ. The genus *Tarichium* is founded on this sexual condition of the plant. The two genera Empusa and Tarichium not being antonomous are re-

placed by the genus Entomophthora; but it is proposed by Giard, who has investigated the subject recently, to retain these names to designate the asexual and sexual stages respectively. These plants belong to the interesting order *Saprolegniaceæ*. Other species of the same order are abundant on dead and living fish, cray-fish, etc. They have sometimes proved very destructive to the young fish in hatcheries. The species of the order are not well known, although examples are easily obtained.

J. C. A.

MICROSCOPY.

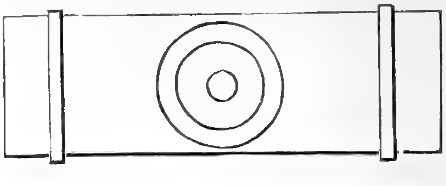
Mr. Julien Derby recently read before the Quekett Microscopical Club a paper describing various special "dodges," which may be employed by microscopists to facilitate their researches.

I. When allowing all but adepts in the use of microscope to peep through my high power glasses, I have often felt a certain degree of uneasiness, not to say of alarm, regarding the fate of valuable test-slides, or still more valuable objectives. Many others here present have no doubt experienced the same discomfort which I find an easy matter to attenuate to a considerable extent, by focussing from the eyepiece instead of from the coarse or the slow motion. All that is needed for this is a rack and pinion to the eyepiece of considerable length. An inch or two up or down corresponds here to a fraction of a turn of the fine adjustment of the microscope, so that very little danger exists of any sudden contact with the covering glass. As soon as an indistinct view of the object is obtained through the ordinary coarse adjustment of the microscope body, the focus is brought to exactness by means of the coarse motion of the eyepiece without much difficulty. For demonstrations or exhibitions in public, microscopes could thus be made without the ordinary fine motion.

II. When mapping with micro-spectroscope, the difficulty of measuring exactly the position of fine lines or absorption bands is often great, even when using the admirable micrometers invented by Mr. Browning and Mr. Sorby. I find that in most practical cases the micro-spectrum can be thrown upon a sheet of white paper by means of an ordinary camera lucida placed over the eyepiece of the spectroscope. Strong light by means of a condenser has to be thrown through the liquid under examination. By means of an ivory rule, finely divided, and brought back to a known line, say D, all other lines or bands may be directly measured off on the rule, and, if desired, the exact results in millionths of a millimetre may then be computed by any of the known interpolation formulæ, such as are given in Suffolk's useful little book.

III. The arrangement of small microscopic objects, such as diatomis, foraminiferæ, etc., on slides in regular lines, circles or patterns, can be much facilitated in the following way: "Draw with a pen and ink cross lines, or circles, or any other figure required on the surface of the plain mirror of the microscope; then focus down until the image of these lines is seen on the upper surface of the top lens of the condenser. By means of a mechanical finger, or of a steady hand with a rest, no difficulty will now be experienced in placing the objects in perfectly regular order.

IV. I now obtain excellent condensed monochromatic light by means of a bull's eye of unusual external shape, the internal portion of which, however, is filled with glycerine or oil of cloves colored to suit. This bull's eye has a plane back and a concavo-convex front, and the liquid is introduced through a hole in the flat side, closed by a small ground stopper. This apparatus is furnished with universal motions, and has a rack and pinion foot. It was made for me by Mr. J. Browning. When using blue light, produced by ammonia sulphate solutions, I have resolved, by means of this monochromatic bull's-eye amphipleura, with objectives in my possession, which will hardly show *Pl urosigma angulatum* under ordinary condenser illumination.



V. Some time ago, Mr. J. E. Ingpen, on my behalf, made a communication to the Club in regard to a growing regard to a growing slide I had devised for some special researches I was following at the time. Some difficulty seems to have been found in the making of these slides, so that it is with pleasure I now offer a still more simple contrivance for obtaining the same results. Here is the receipt: Take an ordinary glass slip with a circular hole, say, half an inch or more in diameter in the middle; lay this slip on an ordinary glass slide, not perforated. Then grease the top of the upper or perforated slide just a little way around the circular hole, and join the two slips of glass by means of two rubber rings (see Fig.). The object is then placed on a thin cover-glass, somewhat larger than the hole in the slide: it is covered by a thin glass cover, ¼ in. in diameter; the whole is then turned down and fastened to the slide by the adherence with the grease, while the small cover prevents the running of the liquid. The plant or animal under examination finds itself confined in a sort of minature Ward's case. When not under observation, the growing slide is laid flat in a shallow plate with water just above the line of junction of the two slips of glass, where, by capillarity, it creeps up to the central cell, where evaporation keeps the contained atmosphere in a state of constant and healthy saturation.

VI. *Copal Varnish*. I find this varnish dries very rapidly if slightly heated, or even if placed on a previously warmed slide. I have many hundred slides of diatoms prepared in copal varnish, and my friend, Mr. Van Heurck, of Antwerp, who was the first to use this material, has many thousands. The varnish to be used is what is called the "pale copal," and its consistency ought to be that of oil. It is much pleasanter to use than Canada balsam, does not make bubbles, and its refractive index is not very different from that of balsam, and does not interfere with the solution of diatom markings. I have of late made many preparations in copal, dispensing with the cover-glass altogether. The drop of copal is placed on the diatoms and heated lightly over the spirit-lamp. It soon takes the consistency of amber, and is hard enough to sustain wiping and brushing with a soft brush with impunity. The optical aberrations produced by the cover-glass are thus done away with.

ASTRONOMICAL MEMORANDA.

Professor C. A. Young has examined the 70 lines given on Angström's chart as common to two or more substances. Of these 70 lines, 56 were seen distinctly double, or triple; 7 single; and in regard to the remaining 7 there is still an uncertainty. The instrument used was a diffraction spectroscope with collimator and observing telescope, each of 3-inch aperture and about 42 inches focal length, and a Rutherford grating of 17,300 lines to the inch. The apparatus was strapped to a 12-foot equatorial provided with a driving clock, and powers magnifying from 50 to 200 diameters were used. A large prism with a refracting angle of 20° was placed between the object glass of the

equatorial and the sun to throw out the parts of the spectrum not under examination, and a concave cylindrical lens was used next the eye to reduce the apparent width of the spectrum, and thus increase its brightness.

From Professor Young's observations it thus appears that the coincidences are only near approximations, but a careful investigation by bringing together the bright-line spectra of the metals and the solar spectrum must be made in order to settle the question conclusively.

Mr. E. J. Stone has presented to the Royal Astronomical Society the complete sheets of his great Catalogue of Southern Stars, observed during his superintendence of the Royal Observatory, Cape of Good Hope. This very important work contains the places of between twelve and thirteen thousand stars, including, in addition to the stars observed by Lacaille, a considerable number of stars falling within similar limits of magnitude. "A stereographic projection showing the distribution of the stars contained in the Cape Catalogue, 1880, between 110° and 180° N. P. D.," has been lithographed by Mr. Stone.—*Nature.*

W. C. W.

As noted in the issue of last week, the volume of reports on the total eclipse of 1878, has been recently issued from the Naval Observatory at Washington. A few separate copies of the report of Mr. D. P. Todd, assistant in the office of the American Ephemeris and Nautical Almanac, have been reprinted, mainly for distribution among the gentlemen who co-operated in observing the duration of totality along the limits of total eclipse. Besides the usual observations of contacts, Mr. Todd had planned a search for supposed intra-mercurial planets, having provided himself with the four-inch comet seeker belonging to the Naval Observatory. At his station, however (Dallas, Texas), clouds intervened to such an extent that δ Cancri, a fourth magnitude star near the sun, could not be seen. This station was almost the only one of any importance at which clouds interfered on the day of the eclipse. Mr. Todd describes in his report a new method of procedure in the observation of total eclipses, whereby it would seem that the question of the existence of intra-mercurial planets might be speedily settled. An arrangement was concluded between Professor Newcomb (observing in Wyoming), and himself, whereby, if the former should observe any such object, its approximate position should be telegraphed immediately to the southern station for verification—there being about twenty minutes of absolute time intervening the arrival of of the moon's shadow at Wyoming and its reaching Texas. As Professor Newcomb observed no unknown object, there was, of course, no occasion for carrying out this scheme; but it will readily appear that, had the weather been clear at the southern station, and had the position of the objects seen by Professor Watson, been telegraphed for verification, the question of small planets near the sun might have been in a much less uncertain condition than it now is. It is to be hoped that astronomers may utilize this scheme on the occasion of the next total eclipse on the 16th of May, of next year. Eleven sketches and one lithograph plate of the corona accompany this report, but they do not exhibit any details of structure worthy of note. But by far the most importantant portion of Mr. Todd's report relates to the observations of duration of totality, which were made at his solicitation at numerous points along the northeast and southwest limits of total phase. This series of observations will afford a very accurate correction of the longitud of the node of the lunar orbit, whenever the geographical positions of the several stations have been determined with sufficient accuracy to be used in the computation.

NOTE ON SUN SPOTS IN JANUARY, 1881.

To the Editor of "SCIENCE:"

1st, at noon: 5 groups, 11 spots. One spot quite large and close to east edge. Air very tremulous, making observation bad.

7th, 3 P. M.: 1 group, 3 spots. Two are large; nearly north of centre. Air bad. Observation with spyglass, power 36.

8th, 1 P. M.: 1 group, 4 spots. Air very bad.

10th, Noon: 1 group, 6 spots. Air very bad. Power 50.

11th, 2½ P. M.: 2 groups, 9 spots. One group of 7 spots 3' from west edge. Two little spots and faculæ at east edge. Air pretty good.

17th, Noon: 1 group, 2 spots. A large spot near halfway from centre to N. W. margin. Observation with spyglass, power 36.

24th, 10½ A. M.: 3 groups, 23 spots. 12 spots, 2 quite large, south of centre. Air poor.

18th, Noon; 5 groups, 66 spots. One quite large and 5 good size, near west edge. Only good observation this month. The sun was hid most of the time.

Telescope 4.6 inches aperture; Power 100, except otherwise noted.

The number of solar spots has been slowly increasing since March, 1879. But it looks likely that the next maximum will be considerably more than eleven years from the last one, which occurred about August, 1870. The following minimum was nearly nine years afterward. It is generally about seven years from maximum to minimum, then four years to the next maximum. So I think it probable that the period, this time, will be about thirteen years, making the next maximum in 1883.

WM. DAWSON.

SPICELAND, IND., February 2, 1881.

CHEMICAL NOTES.

FORMATION OF BASES FROM SUBSTITUTED ACID AMIDES.—O. Wallach and Iwan Kamenski conclude, from their experiments, that if a base is formed by the action of phosphorous penta-chloride from a substituted amide of monobasic acids with a short carbon chain, two molecules of the amide enter into reaction in such a manner that hydrogen is derived from the hydrocarbon radicle pertaining to the acid in order to form hydrochloric acid.

ZINC CHLORIDE AS A REAGENT FOR ALKALOIDS, GLYCOSIDES, ETC.—A. Jorissen has found that the following bodies produce characteristic reactions with pure zinc chloride: Strychnine, bright rose; thebaine, yellow narceine, olive-green, delphinine, brownish red, berberine, yellow; veratrine, red; quinine, pale green; digitaline, chesnut-brown, salicine, violet-red; santonine, violet-blue; cubebine, carmine red. In case of strychnine the reaction can be produced with 1 decimilligrm. of the hydrochlorate. Brucine and aconitine, if present, interfere. To obtain the blue coloration characteristic of santonine, the mixture during evaporation must be continually stirred with a glass rod drawn out to a point. Digitaline gives first a green solution, similar to that produced by heating with hydrochloric acid. After evaporation there remains upon the porcelain a chestnut-brown spot which quickly blackens. The salicine reaction can be used for detecting the fraudulent addition of this body to quinine sulphate. Albumenoid substances, if heated for a time with the zinc chloride solution, leave a violet stain upon the porcelain, which may be distinguished by its instability from the colorations mentioned above. As a rule it quickly blackens. The author's method of operating is as follows: A solution of the alkaloid or its hydrochlorate is evaporated to dryness upon the water-bath, say in the inside of the lid of a porcelain crucible; two or three drops of the test-solution—1 grm. fused zinc chloride in 30 c.c. concentrated hydrochloric acid and 30 c.c. water—are placed upon the residue, and dried up afresh on the water-bath. The coloration begins at the outer edge and spreads inwards as the water is expelled.

BOOKS RECEIVED.

ON CERTAIN CONDITIONS OF NERVOUS DERANGEMENT. By William A. Hammond, M.D. Published by G. P. Putnam's Sons, 182 Fifth Avenue, New York, 1881.

The recent lecture of Dr. Beard before the New York Academy of Sciences, on "Mesmeric Trance," appears to have revived an interest in this subject, and new works bearing on Hypnotism are promised by those who have of late given attention to the phenomena in question.

The work before us by Dr. Hammond, therefore, comes at an opportune moment, for it not only explains very fully the author's views on "Hypnotism," but all the other conditions of nervous derangement which are evidently allied to the same class of mental disturbances. Thus we have chapters on Somnambulism, natural and artificial, including Hypnotism, various phases of Hysteria, the Hysteroid Affections, Stigmatization, Supernatural cures. Some of the causes which lead to sensorial deception and delusional beliefs.

Although the present work is a reprint of a previous book published by the author in 1876, Dr. Hammond states that he has thoroughly revised, and added largely to the subjects now considered, and also "omitted every thing specially relating to spiritualism."

Turning to the subject of Hypnotism, we are somewhat surprised to find it classed under the heading of Artificial Somnambulism, especially as we understood Dr. Hammond to state in a recent lecture before the University of the city of New York, that he attributed the phenomena to quite another cause, and for this reason he proposed to dispense with the term Hypnotism, which implies "sleep," and suggested the introduction of the word "Syggnosticism," meaning union of thought, or sympathy of thinking between two persons. The subject is also complicated by finding two such authorities as Dr. Hammond and Dr. Beard giving conflicting explanations of the phenomena.

Studying the one case of Hypnotism given by Dr. Hammond as the result of his experience, it appears to come to any other conclusion than that the phenomena presented in Hypnotism are merely manifestations of disease.

The instance we refer to was that of a young lady of great personal attractions, who up to a certain time was in a normal condition.

We first find that her nervous organization became depressed and demoralized by a great domestic bereavement, and further prostrated by fatigue, excitement and grief. The trouble commences by the young lady showing symptoms of chorea, the muscles of the face being in almost constant motion. The next step was that she talked in her sleep, and later she walked in her sleep and became a confirmed somnambulist. In the latter condition she walked about the house, struck a match and lighted the gas, seated herself in a chair and looked fixedly at the portrait of her lost mother.

While gazing at the picture she was subjected to various experiments by Dr. Hammond, her olfactory nerves received no impression from the fumes of sulphurous acid gas; she failed to perceive the sour taste of lemons or the bitter taste of quinine; scratching the back of her hand with a pin, pulling her hair and pinching her face appeared to excite no sensation, thus exhibiting all the phenomena of Hypnotism.

The next stage of this case develops a power in the patient of inducing the hypnotic state at will. Her process was to fix her attentions by reading a book and fixing her eyes steadily to reflect as if in a reverie, when she would presently pass to a perfect hypnotic condition.

Without professing to give a final opinion on the phenomena of Hypnotism, we direct attention to this authentic case presented by Dr. Hammond as showing what appears to be the evolution of Hyprotism.

First we find the subject in health, with all the functions and conditions of life normal. Secondly, the body and nervous organization is subjected to a great mental strain, developing a modified Hypnotic condiition. Thirdly, the disease becomes chronic and all the phenomena of Hypnotism are established and the patient is subject to hysteria, catalepsy and ecstasy, three conditions Dr. Hammond considers present in confirmed Hypnotism. There must be a final stage, the form of which may depend on circumstances. Under judicious treatment perhaps a normal condition of the nervous system may be restored, while, on the contrary, a further development of the disease may result in a total breaking up of the nervous system, followed by mania.

These reflections are suggested by the work before us, but in the present condition of the question it is impossible to arrive at any satisfactory conclusion. Drs. Hammond and Beard are not agreed even on the fundamental principles involved, and the former employs two terms for the phenomena, which are antagonistical to each other. It is therefore evident that ample opportunity is presented for a more thorough examination of the question, the result of which would doubtless improve our knowledge of mental diseases, many of which are at present inexplicable.

PAPILIO.—Devoted to Lepidoptera exclusively.—Vol. 1, No. 1, January, 1881.—Mr. Henry Edwards, No. 185 East 116th street, New York City.

This Journal is the authorized organ of the New York Entomological Club, and will be issued about the fifteenth of each month (excepting the two mid-summer months) the subscription being $2 per annum.

The first number contains many articles of interest to entomologists, and a full-page colored illustration of the beautiful insect Edwardsia brillians, from a specimen captured in N. W. Texas by the late Jacob Boll.

Entomologists will welcome this Journal, which in the hands of Mr. Henry Edwards will, doubtless, be maintained at a high standard, aod command success.

USE OF GLASS-WOOL IN FILTRATION.—F. Stolba and R. Böttger. Both these authors point out that glass-wool is attacked by various liquids, including hot water.

DETECTION OF PRE-FORMED UROBILINE IN URINE.—One hundred c.c. of urine are gently shaken up with 50 c.c. of perfectly pure ether; the ether decanted off and evaporated. The residue is taken up in absolute alcohol, and is rose-colored with a green fluorescence. The experiment does not always succeed.—E. SALKOWSKI.

A COLORING MATTER FROM CARBON DISULPHIDE.—If carbon disulphide is agitated with semi-fluid sodium amalgam, and if the paste-like mass is mixed with water, there is produced a hyacinth red liquid, whilst mercury and mercury sulphide are deposited. The solution contains the sodium salt of a yet unknown acid, somewhat soluble in hot water, and more readily in alcohol. It dyes yellow, orange, and brown shades on wool and silk.—C. REICHL.—*Polyt. Notizblatt*, 35, 151.

HOMOFLUORESCEINE: A NEW COLORING MATTER FROM ORCINE AND ITS DERIVATIVES.—On heating solutions of orcine with caustic alkalies and chloroform, the liquid becomes purple and then fiery red, and on dilution shows a strong greenish yellow fluorescence. This reaction is exceedingly sensitive. On neutralizing and adding bromine water a compound is formed from the fluorescent coloring matter resembling eosine; its alkaline alcoholic solutions appear cherry-red by transmitted light with yellow fluorescence. Though many of these compounds have splendid colors, only the nitro-derivatives are suitable for dyeing. Hexa-nitro-mono-oxy-homo-fluoresceine dyes silk a brilliant orange, the penta-nitro-diazo-amido-monoxy-homo-fluoresceine compounds a gold yellow and cyamic acid a light reddish yellow.—H. SCHWARZ.

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PROFESSOR WATSON'S SUCCESSOR.

Prof. E. S. Holden, U. S. N., has been detached from the Naval Observatory, and granted a year's leave of absence, in order to take charge of the Washburn Observatory, of the University of Wisconsin.

The sudden death of Prof. Watson left his plans in a very unfinished state. He had partially completed, at his own expense, a "solar observatory" which bears his name. His plan was new, and he intended to re-discover the intramercurial planet, Vulcan, which he reported during the total eclipse of 1878. At the bottom of a hill, sloping at an angle of about forty-five degrees, a small building with a deep cellar was built. A tunnel about eighteen inches in diameter and fifty-five feet long, parallel to the earth's axis, connects this cellar with a pier at the top of the hill which is to support a heliostat. As the tube is pointed directly towards the north pole, it is necessary to give the heliostat but one motion in order to keep the sun in the field of view of the observing telescope placed in the cellar at the bottom of the hill. The object of the long tube was to cut off as much of the stray light as possible, and to enable the observer to examine objects close to the sun's limb.

The Washburn Observatory is provided with an excellent 16-inch Clark equatorial, which is ready for work. Fauth & Co., of Washington, are making a good 3-inch transit instrument, similar to that of the Princeton College Observatory, and it is probable that later a 6-inch meridian circle will be ordered from Repsolds, of Hamburg.

The work to be carried on for the coming year consists, mainly, of a systematic review of the heavens, following up Herschel's work. It is probable that this work will be done by Professor Holden and Mr. S. W. Burnham, in concert. The transit instrument will be used by Mr. G. C. Comstock in maintaining a time-service, and incidentally in obtaining a set of Right Ascensions of the sun, and an extended series of observations of Polaris.

The State of Wisconsin has provided for the printing of the publications of the Observatory. The volumes will be issued at irregular intervals. No. I. will be a History of the Observatory under Prof. Watson's administration, together with the reduction tables prepared by his directions, and No. II. will probably contain Burnham's General Catalogue of Double Stars for 1880.

It is said that during the memorable battle of Waterloo, competent judges pronounced Wellington more than once a defeated general, and that, according to the rules of war, he should have retreated on Brussels. But Napoleon looked in vain from La Belle Alliance to see his foes fleeing before him, for Wellington had given the order "*stand fast, we must not be beaten*," and in a few hours he was marching victoriously on the road to Paris.

During the last twelve months we have heard much of another man, who was supposed to be defeated in every point, and was reported by the English scientific press to have abandoned the battle field at Menlo Park, and to be slowly retiring on California. Professional men and others, who accepted such views on the subject, have from time to time, with a persistence worthy of a better cause, proclaimed aloud that Edison was beaten, and that, in their opinion, his whole system of electrical lighting must end in failure.

But Edison, like Wellington, in the midst of many difficulties, merely closed up his ranks and gave the order "*we must not be beaten!*" The historical parallel may be carried further; Edison may be said to have marched on Paris, and entered the enemy's Capitol, for having accomplished his task at Menlo Park, and perfected his system of electric lighting in all its details with the most perfect success, we now find him installed in sumptuous offices in Fifth avenue, New York city, putting into practical operation the full results of his previous experiments.

A company has been formed which will control and place Edison's system of electrical lighting on the market, but "the Master" will himself superintend all the details of the construction department, until a district has been finally laid out and found to be working satisfactorily. Progress has been made in this direction, and the Edison lamps are being placed in position as fast as they can be produced at Menlo Park, under the direction of Mr. Upton.

Taking a retrospective view of the last eighteen months, one may well pause to ponder over the immense amount of work accomplished by Edison during

that period. Scientific investigations of the most complicated nature have been successfully carried on, the ordinary beaten paths of research of the Chemist, the Engineer and the Electrician have been cast aside, and original methods of exploring the whole domain of science employed with indefatigable perseverance. The very text books and scientific literature on which others have relied, proving unreliable, were rejected, and Nature, at its fountain head, consulted in solving the these problems.

With such methods and indomitable will, and with the constant and valued co-operation of Mr. Charles Batchelor and Mr. Francis R. Upton, the great work has been successfully accomplished.

The arrival of Edison in New York with his corps of skilled electricians and engineers, occurs at an opportune moment. Deaths from suffocation caused by the escape of the ordinary illuminating gas have multiplied of late, and as we now write the bodies of two women who have died from this cause, await burial. During the last few days a building on Broadway suffered from a violent explosion of illuminating gas, making the second within a few weeks. In the first instance many persons were injured, and in the more recent case one hundred persons escaped death only by the force of the explosion taking a fortunate direction. With the acceptance of Edison's system of electric illumination, these dangers to health and life, to which we have been so long exposed, become as things of the past, except where voluntarily encountered, and to this extent Edison may claim to have conferred a benefit to which the whole world will be heir.

We are under obligations to the Marchioness Lanza for a fine translation of a paper by the renowned Professor Rudolph Virchow, of Berlin, entitled "*Organic Healing Power.*" This paper, involving many points of general scientific interest, will be produced in our next issue.

Virchow is now in his 61st year, and it is 36 years since he was challenged by Count Von Bismarck to fight a duel, on account of Virchow (who was an advanced liberal) having defeated Bismarck's project to obtain money from the Parliament to create a German navy.

AMERICAN CHEMICAL SOCIETY.

The February meeting of the American Chemical Society was held on Friday evening, February 11, 1881. The meeting was called to order by Vice-President Leeds, after which the following gentlemen were duly elected members of the society, viz.: Messrs. N. Gerber, James F. Slade, Theodore M. Hopkey, Professor F. N. Venable, and E. K. Dunham. Dr. E. R. Squibb then took the chair and Professor A. R. Leeds read the following papers:

I. Upon the invariable production, not only of ozone and hydrogen peroxide, but also of ammonium nitrate in the ozonation of purified air by moist phosphorous.

II. Upon the action of ozone, oxygen and nascent oxygen upon benzine.

III. On a new class of aromatic sulphurous acids.

Mr. J. H. Stebbins, Jr., followed with some remarks on tetra-azo-compounds, substances to which he has paid particular attention, for it will be recollected that a whole series of the di-azo-colors were originally produced by him.

Professor W. G. Levison then gave the Society the results of some recent experiments by him on polarized light. On the conclusion of this paper, the society was adjourned. M. B.

New York, February 17, 1881.

NEW YORK MICROSCOPICAL SOCIETY.

The third annual reception of the New York Microscopical Society was held on February 14th, 1881, at the rooms of the Academy of Sciences. The annual address of the President was delivered by Professor R. Hitchcock, who selected as his subject: "The Relations of Science to Modern Thought," on the conclusion of which the meeting resolved into a *conversazione*, when a variety of interesting but familiar objects were exhibited.

THE annual meeting of the German Chemical Society took place December 22, 1880, on which occasion the following officers were elected for the present year: President, A. Baeyer; Vice-Presidents, A. W. Hofmann, L. v. Barth, F. Hoppe-Seyler, H. Landolt; Secretaries, F. Tieman, A. Pinner; Vice-Secretaries, E. Bauman, Eug. Sell; Treasurer, J. F. Holtze; Librarian, S. Gabriel. M. B.

THE SOCIETY OF TELEGRAPH ENGINEERS, (England).

On Wednesday, last week, Prof. G. C. Foster, F. R. S., president, read his inaugural address before the Society of Telegraph Engineers and Electricians, the principal thing dwelt upon being the practical importance of a trustworthy system of electrical measurements. The Society, he said, was not merely a professional one, but was concerned with the scientific principles which underlie the practical operations of electricity. The present practical applications of electricity owed their existence to scientific discoveries made just over 60 years ago. Reference was made to the investigations of Oersted in 1820, and Davy in 1821. Induced electrical currents enabled the electric light to cease to be a scientific marvel and become of practical interest to municipal corporations and limited liability companies. Davy first produced an electric light by the passage of currents from a battery of 2000 cells between carbon points. Oersted, Ampère, and Faraday traced out the fundamental laws of the phenomena of induction. In the ordinary course of scientific discovery, the qualitative aspects of phenomena first attracted attention. Quantitative knowledge followed later by degrees. "Absolute values of constants" could only be given when a phenomenon was sufficiently well known for its laws to be expressed in definite mathematical formulæ, or when methods for the determination of such values could be devised. But when definite results had to be produced as part of a commercial undertaking, that point became of the utmost importance from the very first. Examples were given. During the past 100 years an unknown large number of electrical machines had been made for more or less scientific purposes; but after all that experience it was a question as to who could draw up a specification for an electrical machine which should, with a given number of revolu-

tions, produce a known quantity of electricity, or which would charge a condenser of one microfarad capacity to a given potential. Knowledge as to the power of a galvanic battery was much more definite. Everything in that respect could be stated with exactitude. If knowledge of the practical uses of electrical machines were comparable with that in respect to the galvanic battery, knowledge as to the efficiency of the former would soon be equally definite. The necessity of proper standards based on numerical data was understood in more than one branch of physics; but the present remarks were directed only to electricity, which had in recent years undergone almost a complete transformation. After describing the principles upon which a system of electrical measurement should be founded, the steps taken by Ohm, Weber, Oersted, and others, in arriving at definite laws, were related and tabulated for comparison. Weber's system had been extended by Sir W. Thomson, and the practical applications of electricity in its early days produced the necessity of being able to express results upon a coherent system of standard units. For that purpose the committee of the British Association on electrical standards was appointed, and the B. A. unit resulted. The data upon which that unit was founded had subsequently been verified, and at the present time a redetermination of its accuracy was about to take place. The absence of any standard resistance coils was pointed out, and the suggestion made that it would reflect credit on the Society if it at once set to work with the view of establishing a definite standard resistance, with which instruments used for every-day practical purposes could be occasionally verified and adjusted. A paper on "Some Experiments on Induction with the Telephone," by Mr. A. W. Heaviside, was then read. In the discussion which followed, Prof. Hughes, Mr. Stroh, Prof. Ayrton, and others took part.

FURTHER NOTES ON THE BRAIN OF THE IGUANA AND OTHER SAUROPSIDÆ.

By Edward C. Spitzka, M. D.

I would add to the observations published in No. 7, Vol. I. of Science, relating to the brain of the Iguana, the following:

1st. The ganglionic intumescence upon the inner edge of the cerebral hemisphere, which I supposed to represent the homologue of the molecular basis of the Fascia dentata of Tarini in the mammalia, is more voluminous in the middle of the hemispheric length, than in the posterior third. The homologization of the entire inner wall of the hemisphere with the *Cornu Ammonis* of mammals gains strength from the fact that in the Opossum the Cornu Ammonis extends almost along the whole inner hemispheric wall, and is but slightly folded as compared with that of the Rodentia. That the elevation which I supposed to correspond to the *fascia dentata* and *tænıola cinerea*, might be interpreted as one of the thalamic tubercles, which I considered an open question at the time of my writing the first commmunication, and which I now hold to be disposed of definitely as well as the other supposition.

2nd. There is a molecular accumulation at the base of the cerebral hemisphere, in the common basilar gray, and beneath the elevation of the *corpus striatum*, which may correspond to the lenticular nucleus.

3rd. At and above the level of the emerging third pair of nerves, there is a beautiful nucleus of large multipolar cells, resembling the cells of the auditory nucleus (that is of the large celled division of that nucleus) in contour and in dimensions. This cell group in its situation corresponds to the *nucleus tegmenti* of mammals. I would here note that throughout the animal range, the cells of the *nucleus tegmenti* and the special division of the auditory nucleus referred to seem to keep step in development. This fact would add another link to the chain of evidence attempted by Meynert, who surmised that an auditory tract passed through the cerebellum to the *brachium conjunctivum*, (and therefore through this cell group) on its way to higher projecting fields.

4th. The so-called *nucleus dentatus* of the cerebellum, which should be termed simply *nucleus cerebelli*, since it is not dentated even in all the mammalia, is clearly present in the cerebellum of the Iguana. It can be found at the junction of the cerebellar peduncles with the main cerebellar mass, and consists of well marked cells of moderate dimensions.

5th. The "fasciculus from the habenulæ to the tegmentum" so-called by Meynert, but which Gudden and his pupils correctly state to run from the habenulæ to the *ganglion interpedunculare*, has not been yet identified in animals lower in rank than the mammalia. I find it well-developed occupying exactly the same relations and presenting the same histological peculiarities as with mammals in the Iguana.

6th. The fourth pair does not reach the valve of Vieussens in levels lower than those in which the root has its origin, as in the turtle (*Nanemys guttatus, Chelydra*) and the mammalia, but distinctly arises in the same level in which it reaches the valvule where it decussates. The nerve itself, however, emerges in levels superior to the latter.

7th. While the cells of the oculomotoriotrochlearis nucleus, and those of part of the auditory origin are of large dimensions, those of the abducens, facial, and motor-trigeminal origin are remarkably small. The reduction in size of the cells is as might be inferred accompanied by a reduction in size of their nuclei. This fact suffices to dispose of the recently advanced claims, that motor cells have larger nuclei than sensory ones. The reduction in size of these motor groups and their presenting such a contrast to the great development of the cells in other motor groups in the Iguana, has to my mind much of the enigmatical. The largest cells in the nervous system of the Iguana, are the multipolar cells of the reticular field, (my *ganglion reticulare* in mammals) those of the auditory origin and *nucleus tegmenti* are of the same or nearly the same dimensions.

8th. The mesencephalic nucleus of the fifth pair is represented as in other reptiles by round cells, sunk in the niche between the two optic lobes; they are not spread out on the contour of the central tubular grey, as in mammals, but concentrated more at the median line. Some of the cells can be identified beneath the inter-optic lobes.

9th. The cells of the substantia ferruginea of man are represented by a group of numerous small ganglionic bodies, whose connection with the fifth nerve is clearer than in the mammalia.

10th. The auditory nerve fibres send a powerful strand which decussates with its fellow in the raphe. In its course each strand traverses or circumscribes the posterior longitudinal fasciculus. This same strand is found in the mammalia, but in the latter it is deeply seated; in the Iguana it is more superficial, and the erroneous inference might be drawn that this strand in the reptile is equivalent to the *striae acustici* of mammals. The latter are however, absent in reptiles, and although in some species visible eminences are formed at the floor of the fourth ventricle, crossing at right angles the longitudinal eminences of the posterior longitudinal fasciculi; these are the homologues of the more anterior and concealed part of the auditory decussation of mammals.

11th. In no reptile have the nuclei of the columns of Goll and Burdach been identified. In the Iguana I can readily identify them, although much smaller than the corresponding nuclei of the mammalia. Their demarcation is, however, distinct.

12th. In the Iguana as in the turtle there is an accumulation of numerous multipolar cells at the raphe in the

level of the junction of the cord and oblongata. In addition a group of remarkably attenuated cells is found at the origin of the spinal accessory. These cells are so much elongated and their protoplasm has been so much narrowed that but for the discovery of a nucleus in one or the o her cell, one might consider them a bundle of axis cylinders. These are better developed in turtles than in the Iguana, and better in fresh water species than in the *Thallassochelys mydas*. In no turtle have I found the cells of the raphe very large, but in the Iguana I have discovered a few very large cells in the same level and location as those first described by Dr. J. J. Mason for the Alligator.

13th. In my first paper I indicated the existence in the Iguana of a hitherto undiscovered pair of lobes or tubercles between the optic and post optic lobes. I have also indicated their homology with a concealed pair in the turtle and alligator. At the time I did not describe the topographical relations minutely. Normally—if I may use the expression—as in the turtle and alligator, the newly discovered ganglia lie at the margin of the central tubular gray of the mesencephalon, in the anterior part of the *corpora quadrigemina*. As we go more posteriorly they are found to extend more dorsally, until in the turtle, for example, they nearly touch in the median line just at the posterior fifth of the optic lobes, where they cease. In the Iguana the relations are the same, but instead of terminating before the posterior margin of the optic lobes, they extend further backwards and prominate at the surface of the brain, as two sharply marked buttons. Their structure is the same in all reptiles so far examined, a molecular basis and small roundish cellular elements. In anterior levels nerve fibres can be seen entering them in strands, from the arched fibre mass which is found beneath the deep gray layer of the optic lobes. Although all surmises as to the function of the inter-optic lobes are as yet strictly hypothetical; yet from the fact that they are directly connected with the central tubular gray, and are under the fascicular subjection of the optic lobes, and that they are well developed in reptiles, and poorly, if at all, developed in mammals, one might suspect them to have some relation to the innovation of the Harderian gland, just as the mesencaphalic nucleus of the fifth pair may be looked upon as the probable centre for the innervation of the lachrymal gland proper.

RECENT PROGRESS OF SCIENCE.

Rev. Samuel Fleming, LL.D., Ph. D.

The progress of science within our own times has been wonderful. Prof. Helmholtz uses the following language: "The contemplation of the astounding activity in all branches of science may well make us stand aghast at the audacity of man, and exclaim with the chorus in the Antigone: Who can survey the whole field of knowlege? Who can grasp the clues, and then thread the labyrinth?" Every department of science has been vastly extended, and every votary of science stimulated to untiring efforts to survey this field, not only, but to enter the secret chambers of knowledge to find the treasures concealed from the human mind, until modern discoveries, modern analysis, and modern invention have combined to make those hitherto hidden facts of science known, and available for practical benefits to human society.

The exact science, Mathematics, has found ample room for the application of its principles and methods of determining the content of all material existences and relations. The sublime science, Astronomy, has reveled in its excursions into illimitable space, adding new triumphs, discovering new facts pertaining to the constitution of the stellar universe, and the relations of the celestial masses, measuring, by the agency of light, the immense distances, magnitudes and motions of the tiny objects which the natural eyes behold in the expanse above, and in former times regarded as "fixed stars." The profound science, Geology, has carried us back into the illimitable depths of past duration, to contemplate the usually slow process by which the earth has been changing from its primordeal, nebulous condition, to that in which it has become fitted for living and rational beings, adding new testimonies of the rocks to the truth of Scripture, expressed by the significant language: "Of old hast thou laid the foundations of the earth." The widely related, efficient science, included in the scope of terrestrial Mechanics, has found abundant use for its forces, and the practical application of its dynamics, in the constantly increasing demands of human society. The splendid and delicate science, Chemistry, has exulted in its new and valuable discoveries in the realm of atoms and molecules, verifying the atomic theory, and adding new evidences that many of the supposed elements of matter are really compounds, and must yield to the searching analysis which finds them to be but molecules composed of two or more atoms. The vast and richly diversified science, Biology, has yielded its living evidences of the progressing series of organic natures, and of the vast scope of its history, extending its relations to ancestries, the periods of whose origin belong to the immeasurable epochs of palæontological history, The crowning, all-conserving science, Anthropology, has added new evidences of its superiority in importance, as it stands highest in the scale of associated sciences; and while it has maintained this highest rank by maintaining the honor of its subject-matter, its votaries have found its latest and greatest achievement in the evidences of a formal psychical constitution as the basis of mental action.

It is not the aim of the writer to pursue the history of the development of the sciences, exhaustively, but to indicate some of the lines of progress.

The brilliant discoveries in Astronomy, within the past few years, have added largely to the wealth of this noble science, fascinating the student, and inspiring to new achievements. Previous to the present century, the solar system included seven primary planets as having at that time been discovered. In the year 1800 a new planet was discovered, and designated an asteroid, or small star,—but it is more properly called a planetoid, or small planet. The name by which this is known is Ceres, after the reputed originator, or god, of corn. It was an event of so great interest to astronomers that it was announced with much eclat that "The long-expected planet between Mars and Jupiter had been discovered." Soon after, three more were observed. Since that time, by means of the greatly increased power of telescopes, more than two hundred have been added by discovery, all being very small. Many others will be found. The problem still to be determined has been, whether these planetoids are "fragments of a broken world," as formerly supposed, or separate condensations from cosmic matter, instead of forming one large body, as in the case of other primary planets. It is not probable, however, that a cosmic mass exploded at any one period, producing such fragments in such positions in their orbits as they maintain, nor that such original mass was so dissipated by the action of a propulsive or radiate force at one time, as to resume its original nebulous state. The second hypothesis is the more probable, viz.: of separate condensations from original nebulosity.

Neptune, one of the largest planets, and nearly twice the distance of Herschel from the sun, was discovered in 1846 by M. Challis, of Paris, and its elements and orbit determined by Le Verrier. The discovery of this planet furnished a satisfactory explanation of the aberrations of the planet Herschel, caused by the approximations of Neptune, though distant, at its nearest point, more than a billion and a half miles. This increase of the number of the solar family furnished an additional illustration, on a grand scale, of the laws of universal gravitation and

of celestial mechanics. Added to this have been discoveries abundantly confirming the theory of stellar motion in groups, clusters and nebulæ, "the places of more than 200,000 stars having already been determined," and we have some conception of the vastness of human achievements, and of the possibilities still awaiting discoveries in this illimitable "field of ether."

The universality and laws of primary force, denominated gravitation, have been subjects of exceeding interest, as they pertain to this primary mode of motion. The fact of an attractive Force acting either upon or within bodies by which they tend to approach each other, arrested the attention, about the year 1600, of the elder Galileo, who extended the principle to all terrestrial bodies, Newton, eighty years afterward, studying this principle, and at the suggestion, it is said, of the fall of an apple, found that there was a definite increase of velocity of bodies approaching the earth, and also that the same kind of attractive force must apply to the moon, while a centrifugal force, either generated from the attractive force, or originated from an extraneous force, continued this secondary planet around the earth. This was the first grand step toward the discovery of the laws of gravitation, applicable to the motion of the earth around the sun, and, generally, to all planets. More recently the principle has been applied to comets, stellar and other masses.

Geology, while below chemistry in the order of nature and classification, had made far less progress in development at the commencement of the present century, a fact which might have been presumed, inasmuch as the latter science has ministered especially to the wants of mankind. According to Buckland, it was at that time "without a name." The general features of geology had been sketched by Leibnitz and Hooke more than a century previous. Near the beginning of the present century the outlines of the subject were classified into three general divisions of formations—the primitive, the secondary and the tertiary. These became the subjects of investigation, historically, in the order named. The first, especially, by Werner, of Germany, who examined chiefly the primitive and transition rocks. The second by Wm. Smith (English), whose observations were first published in 1799. The third by Cuvier and Brougniart whose works upon "Organic Remains" and "Mineral Geography" were published in 1808. During the past half century this science has advanced with other sciences, with vastly increased interest and success, rendering this one of the most fascinating, especially in more recent times, in yielding its stores of facts pertaining to the glacial period, the deposition of metallic substances, experiments showing the order and conditions of the cooling processes, resulting in the different mineral states, and the wonderful revelations of paleontological history, together with many other facts of great interest, but which cannot, in this paper, be especially given. These give abundant confirmation to the theory that immense periods of time, measured by millions of years, have passed during this history, dissipating the doctrine formerly held by many as taught in the Scriptures, that the heavens and earth were created, out of nothing, about six thousand years ago.

Among the departments of science which minister to the wants of human society, none has awakened the spirit of invention and improvement at all to compare with that of Mechanics. With the increase of knowledge, there has been a correspondingly increasing demand for instruments of discovery and analysis, not only, but for the application of scientific skill in the invention of motive powers and the means of the transmission of intelligence, as well as implements of handicraft, of agriculture, etc, The steam power, first utilized by the invention of a machine in 1655, and improved by Watt in 1774, inaugurated its grand work for human society in 1806, when Robert Fulton, after repeated experiments, applied this power to the propulsion of vessels, first on the Hudson river, amazing the thousands who witnessed the successful experiment, and introducing a new propelling power to vessels upon the sea, now bearing their burdens, estimated by millions of tons, on every river and over every lake and sea of earth. This power has added incalculable millions to the material wealth and strength of every civilized nation. The last world-wide application of this power, besides its innumerous minor applications to all kinds of mechanical work, was inaugurated in 1821, when it was successfully applied to the propulsion of railroad trains.

In 1819 Electro-magnetism was first applied to mechanical purposes; and in 1831 the Magnetic Telegraph, for the transmission of intelligence, was invented and successfully applied. And now, even the comparatively coarse medium, air, has aided in business and social communications, at trifling expense, by means of the recently invented Telephone and the Phonograph.

Chemistry has shared richly in the results of recent scientific progress, and has ministered richly to the wants of human society. Three centuries ago, Paracelsus boasted of possessing the "philosopher's stone", by which the baser metals were said to be transmuted into gold; but he gave a new direction to the efforts and objects of Alchemy, insisting that its chief aim should be the preparation of medicines of different kinds for different diseases. But Chemistry, as a science, must date its commencement two centuries later, when the analyses of distinguished scholars, as Scheele, of Sweden, and Dr. Black, of Glasgow University, and the Academies of Science at Berlin and Paris, determined important principles of this science.

The discoveries of Sir Humphrey Davy, in the early part of the present century, gave a new impetus to this branch, leading to chemical analyses and the establishment of chemistry as a science. These have been followed by eras of progress which have brought the subject to a high degree of perfection. Now, the four elements of the ancients, and of the alchemists of comparatively recent times—earth, air, fire, and water—have been found by successive analyses to contain sixty-five elements, the last four having been detected by the new and wonderful method called the Spectrum Analysis. It may be here stated what this method is, for the gratification of any whose attention may not have been called especially to it.

It is well known that a spectrum is an image formed by the light of the sun, or any other luminous body, either as direct or reflected rays, passing through a triangular piece of glass called a prism. The colored lines thus formed by differently refracted and dispersed rays, reveal the nature and qualities of the elements contained in the luminous body by the different colors, combinations, and the phenomena presented, compared with previous results of experiment in the laboratory, upon light reflected from different mineral substances. It has been found that every kind of mineral substance, whether in the form of a solid, gas, or nebulous matter, when in a state of intense luminosity, possesses the capacity to emit a specific color, with its accompanying mixed lines. This being known, when a new body is analysed by its light, its constituents are determined by the lines of light. Thus the solar envelopes, protuberances, etc., of the sun are examined by the analysis of the solar spectrum. By this method, the character of comets, meteors, or other celestial masses are determined. By this the problem of the sudden appearance or disappearance of stellar masses is explained, by determining the state of the mass thus emitting light, and the conditions of luminosity. What the telescope has failed to determine in respect to the elements and qualities of bodies, or the nature of nebulous masses, whether such masses are clusters of stars in the infinite distance, or of original, unformed nebulous matter, the spectroscope has accomplished; and what has been held by most astronomers as a theory, has become

confirmed as a fact, that, as Prof. Schellen says, "luminous nebulæ actually exist as isolated bodies in space, and these bodies are masses of gas." Thus, clusters, groups, stars and planets, are in process of genesis from primeval cosmic matter, and Cosmology may be regarded as a science, established by the aid of art in the construction of larger telescopes, and their new associate in the field of stellar research, the spectroscope; these bring within the scope of observation new facts, and confirm the generally received theory of the nebular constitution and the genesis of the stellar and planetary systems from such original cosmic matter.

The conservation of all the lower departments of science to the wants of man, in his individual and social relations, gives a vast superiority of rank to Anthropology. In recent times, the chief points of practical importance in the progress or development of this science have pertained to Sociology. Researches in special lines of investigation have furnished many facts of great interest pertaining to antiquities, archives of ancient cities, inscriptions upon rocks, hieroglyphics and monuments, which have yielded abundant fruits to explorers, and vastly increased the knowledge of particular races and languages; while increasing evidences are furnished that the antiquity of the human race is much greater than that indicated by the generally accepted chronology.

In the department of Philology, great progress has been made during the period of our own times. Comparative Philology is no longer confined to the Latin and Greek of the ancient languages, and two or three of the modern languages, but every language of the globe is yielding rich fruits bearing upon history as well as philology; especially has the Sanscrit, the mother of all the Indo-European languages, received special attention, resulting in the establishment of professorships of the Sanscrit in several colleges.

But questions of the highest interest pertain to Psychology, especially relating to our psychical nature and its connection with our physiological constitution, to the phenomena of "Unconscious cerebration," and other facts which have elicited research in the modes of receiving and retaining sensations and the memory of facts, and into the medium of transmitting such impressions. Such inquiries have led to the adoption of the following theory of accounting for these phenomena, viz.: that the psychical constitution is not simply mental or spiritual, but is *dual* or two-fold, consisting of two substances we may conveniently term respectively *etheral* and *spiritual*. The following rational deductions are given as the only satisfactory hypotheses pertaining to our interior being, viz.: That the great rapidity of the transmission of impressions, being at least 100 feet per second from the extreme parts of our physical system to the brain, or requiring but one-fifteenth of a second to produce a sensation, involves the necessity of the existence of an ethereal substance permeating the nerves, and hence called "nervous ether," which forms the elementary substance of the formal psychical nature. That, as the physical germ is the initial organism of the future physical body, "potentially alive," in the germinal state, so this nervous ether contains the psychical germ or initial organism of the future psychical body, potentially perfected, and which emerges, in its real or developed form, upon the death of the physical body, or properly its separation from the soul, or interior being. That the psychical nature, while connected with the physical, forms the basis of vital action, continuity and identity; and that the mechanism of thought and feeling involves the necessity of two psychical centres of activity, corresponding with the brain and heart, viz.: the psychical *sensorium*, which is the seat of intellectual action, the basis of sensation, memory, etc., and the psychical *cardium*, the seat of the emotional and sympathetic affections.

Scientific progress has both increased the number of special sciences and extended the limits of those previously known. This has created the necessity of the division of scientific research, inducing students to pursue single lines of inquiry, the result being more thorough and extensive knowledge of the respective departments, which have become the common heritage. Examples of this devotion to special sciences are now numerous in every department, as in the case of the late Prof. L. Agassiz, who devoted many years to the study of animalculæ. In the history of plants and animals, species, genera, and even classes have been multiplied, as individuals have devoted their lives to these subjects, with all the helps at command, leaving no depths unexplored. The anatomist and physiologist no longer confine attention to the human structure, but find in comparative anatomy and physiology many types and characteristics brought forward and perfected in the higher orders, or old forms substituted by new, till finally, in the human constitution the completed form best adapted, not to the lower purposes of physical strength and endurance, by which the animal subserves human ends, but the best form for the higher ends of intellectual, moral and social natures by which man is evidently distinguished above the brute.

This division of labor has been found essential in application to the numerous sciences now demanding vastly increased forces of professional teachers in colleges and universities. Now, a college can scarcely claim the name of a liberal institution of learning in which one professor is required to associate sciences so unnaturally connected as Mathematics and Moral Philosophy, or Chemistry, Botany and Pharmacy, as in some European colleges a century ago. A comparison of the courses of study and the professorships in colleges in our country during the past thirty or forty years will exhibit the marked advancement of the sciences, and the increased requirements of the present time. In 1837, Geneva College, now Hobart College, Geneva, N. Y., of which the writer was a student, contained a professorship of "Mathematics and Natural Philosophy;" of the "Latin and Greek languagess;" of "Modern languages, History and Belles-Lettres," to which was added Rhetoric and two other mixed professorships. For the year 1849—1850, the catalogue of Western Reserve College, Hudson, O., of which the writer had been a theological student, exhibits the following: The institution embraced three departments: General Science, Medicine, and Theology, besides a preparatory department. Five professors gave instruction in General Science, or the Literary department; one of which was the professor of the "Latin and Greek;" one of "Chemistry, History, Medical Jurisprudence (in the Medical department), and Natural History,"—the latter embracing several branches, including Geology! and one professor of "Modern Languages." Great advancement upon this order is now exhibited in the principle colleges of our land. I here name only three: In 1875, Lafayette College, Easton, Pa., had twenty professors and adjunct professors, besides tutors, assistants and lecturers—twenty-seven in all. The University of Wooster, O., in 1876, had thirteen instructors in the Literary department, and the same number in the Medical department. The Michigan University, Ann Arbor, Mich., in 1877, had, in all departments, fifty-five instructors.

CAUSE OF THE BLUE COLOR OF CERTAIN WATERS.

By Prof. John Le Conte.

The consideration of certain facts clearly indicates that the real cause of the blue tints of the waters of certain lakes and seas, is to be traced to the presence of finely-divided matter in a state of suspension in the liquid. We have seen that Sir I. Newton, and most of his successors as late as 1869, ascribed the blue color of certain

deep waters to an inherent selective reflecting property of its molecules, by which they reflected the blue rays of light more copiously than the other rays of the solar spectrum. Since the researches of Soret, Tyndall and others, this selective reflection has been transferred to the finely-divided particles which are known to be held in suspension in greater or less abundance, not only in all natural waters, but even in the most carefully distilled water. When the depth of water is sufficiently great to preclude any solar rays reaching the bottom, then the various shades of blue which are perceived under similar conditions of sunshine, will depend upon the attenuation and abundance of materials held in suspension; the purity and delicacy of the tint increasing with the smallness and the degree of diffusion of the suspended particles. Moreover, it is evident that Tyndall is quite correct in assigning to "true molecular absorption" some agency in augmenting "the intense and exceptional blueness" of certain waters; for it is obvious that the "blue of scattering by small particles" must be purified by the abstraction of the less refrangible rays, which always accompany the blue during the transmission of the scattered light to the observer. It seems to be very certain that were water perfectly free from suspended matter and coloring substances in solution, and of uniform density, it would scatter no light at all. "But," as Tyndall remarks, "an amount of impurity so infinitesimal as to be scarcely expressible in numbers, and the individual particles of which are so small as wholly to elude the microscope," may be revealed in an obvious and striking manner when examined by a powerfully concentrated beam of light in a darkened chamber. If the waters of the lakes and seas were chemically pure and optically homogeneous, absolute extinction of the traversing solar rays would be the consequence if they were deep enough. So that to an observer floating on the surface, such waters would appear as "black as ink," and apart from a slight glimmer of ordinary light reflected from the surface, no light, and hence no color would reach the eye from the body of the liquid. According to Tyndall, "in very clear and very deep sea water, this condition is approximately fulfilled, and hence the extraordinary darkness of such water." In some places, when looked down upon, the water "was of almost inky blackness—black qualified by a trace of indigo." But even this trace of indigo he ascribes to the small amount of suspended matter, which is never absent even in the purest natural water, throwing back to the eye a modicum of light before the traversing rays attain a depth necessary for absolute extinction. He adds: "An effect precisely similar occurs under the moraines of the Swiss glaciers. The ice is here exceptionally compact, and owing to the absence of the internal scattering common in bubbled ice the light plunges into the mass, is extinguished, and the perfectly clear ice presents an appearance of pitchy blackness" ("Hours of Exercise in the Alps;" "Voyage to Algeria to Observe the Eclipse," Am. Ed., N. Y., 1871, pp. 463–470). In like manner the waters of certain Welsh tarns, which are reputed to be bottomless, are said to present an inky hue. And it is more than probable that the waters of the Silver spring, whose exceptional transparency has been previously indicated, would, if they were sufficiently deep, present a similar blackness, or absence of all color by diffuse reflection.

It remains for us to explain the cause of the green tints which the waters of certain lakes and seas assume under peculiar circumstances. These green colors manifest themselves under the following conditions, viz: (*a*.) In the finest blue water, when the depth is so small as to allow the transmitted light to be reflected from a bottom which is more or less white. Thus, a white sandy bottom or white rocks beneath the surface of the Lake of Geneva, or the Bay of Naples, or of Lake Tahoe, will, if the depth is not too great or too small, impart a beautiful emerald green to the waters above them. (*b*.) In the finest blue water, when a white object is looked a through the intervening stratum of water. In the blue waters of the sea this is frequently seen in looking at the white bellies of the porpoises, as they gambol about a ship or steamer. In a rough sea, the light which has traversed the crest of a wave and is reflected back to the eye of the observer from the white foam on the remote side, sometimes crowns it with a beautiful green cap. In March, 1869, I observed this phenomenon in the magnificent ultramarine waters of the Carribbean sea. A stout white dinner plate secured to a sounding line, presents various tints of green as it is let down into the blue water. Such experiments were made by Count Xavier de Maistre, in the Bay of Naples, in 1832; by Prof. Tyndall, in the Atlantic ocean, in December, 1870, and by the writer in Lake Tahoe, in August and September, 1873. (*c*.) In waters of all degrees of depth when a greater amount of solid matter is held in suspension than is required to produce the blue color of the purer deep waters of lakes and seas. Thus, Tyndall, in his "Voyage to Algeria to observe the Eclipse," in December, 1870, collected 19 bottles of water from various places in the Atlantic ocean between Gibraltar and Spithead. These specimens were taken from the sea at positions where its waters presented tints varying from deep indigo blue, through bright green to yellow green. After his return to England, he directed the concentrated beam from an electric lamp through the several specimens of water and found that the blue waters indicated the presence of a small amount of suspended matter; the bright green a decidedly greater amount of suspended particles, and the yellow green was exceeding thick with suspended corpuscles. He remarks: "My home observations, I think, clearly establish the association of the green color of sea water with fine suspended matter, and the association of the ultramarine color, and more especially of the black indigo hue of sea water, with the comparative absence of such matter." ("Hours of Exercise in the Alps;" "Voyage to Algeria to observe the Eclipse," Ed. cit. ante, pp. 464 et 467.)

There is one feature which is common to all of the three above indicated conditions, under which the green color manifests itself in the waters of the lakes and seas, viz: When a white or more or less light-colored reflecting surface is seen through a stratum of intervening water of sufficient purity and thickness. Condition (*c*,) is obviously included; for it is evident that a background of suspended particles may, under proper conditions, form such a reflecting surface.

Inasmuch as under these several conditions, more or less of transmitted light is reflected back to the eye of the observer, it is evident that the rays which reach him carry with them the chromatic modifications due to the combined influence of the selective absorption of the water itself, and the selective reflection from the smaller suspended particles. Hence, the chromatic phenomena presented, being produced by the mingling of these rays in various proportions, must manifest complex combinations of tints, under varying circumstances relating to color of bottom, depth of water, and the amount and character of the suspended matter present. In the explanations of the green color of certain waters by the older physicists, we recognize the full appreciation of the influence of selective reflections in the productions of the phenomena; but they seem to have overlooked the important effects of the molecular absorption. We have seen that Sir I. Newton regarded the green tints of sea-water as due to the more copious reflection of the violet, blue and green rays, while those constituting the red end of the spectrum are allowed to penetrate to greater depths. ("Optics, loc. cit. ante.") Sir H. Davy ascribes it, in part, to the presence of iodine and bromine in the waters, imparting a yellow tint, which, mingled with the blue color from pure water, produced the sea-green. ("Salmonia, Collected Works," Vol. 9, p. 201.) In like

manner, Count Xavier De Maistre ascribed the green tints to the yellow light, which, penetrating the water and reaching the white bottom or other light-colored submerged objects, and being reflected and mixed with the blue which reaches the eye from all quarters, produces the green. ("Bibl. Univ.," Vol. 51, pp. 259–278, Nov., 1832; also Am. J. Sci., first series, Vol. 26. pp. 65–75, 1834.*). On the other hand, after Bunsen, in 1847, had established that chemically pure water extinguished the rays of light constituting the red end of the solar spectrum more copiously than those of the blue extremity, so that the transmitted tints were more or less tinged with blue, some chemists were inclined to attribute the green color of certain waters to the presence of foreign coloring substances. Thus Bunsen himself explained the brown colors of many waters, especially of the north-German inland lakes, as produced by an admixture of humus; but he considered the green tints of the Swiss lakes and silicious springs of Iceland as rising from the color of the yellowish bottom. (Vide. loc. cit. ante., p. 44, et seq.) Similarly we find that Wittstein, in 1860, from chemical considerations, concluded that the green color derives its origin from organic admixtures, because the less organic substance a water contains the less does the color differ from blue; and with increase of organic substances the blue gradually passes into green, and ultimately into brown. This is likewise the view taken in 1862, by Beets, for he insists that in all waters the observed color of the liquid is that of the transmitted light, and not, in any case, of the reflected light. Moreover, he maintains that Newton, De Maistre, Arago and others were mistaken in classifying water among those bodies which have a different color by transmitted light to that which they have by reflected light. (Loc. cit, ante.)

We have already shown that if the waters were chemically pure and perfectly free from suspended particles, the red rays of the traversing solar light would be first absorbed and disappear, while the other colored rays pass to greater depths, one after the other being extinguished in their proper order, viz., red, orange, yellow green, blue and violet, until at last there is a complete extinction of the light in the deeper mass of the liquid. But the presence of suspended particles causes a part of the traversing solar light to be reflected, and according as this reflected light has come from various depths, so will the color vary. If, for example, the particles are large, or are abundant and freely reflect from a moderate depth, and prevent reflection from a greater depth, the color will be some shade of green.

When the water is shallow and a more or less light-colored bottom, or a submerged object reflects the transmitted light to the observer through the intervening stratum of liquid, it is evident that the chromatic tints presented must be due to the combined influence of the selective absorption of the water itself, and the selective reflection from the smaller suspended particles.

In other terms, under these conditions, the tints are produced by the mingling of the blue rays with the yellow, orange or red; so that the resulting hues must generally be some shade of green. In short, all the facts established by modern iuvestigations seem to converge and point to the admixture of the blue rays reflected from the smaller suspended particles with the yellow orange and red rays reflected from the grosser matters below, as the true physical cause of the green tints of such waters.

The establishments of the very important function of solid particles held in suspension in water, in producing chromatic modifications both in the scattered light and in the transmitted light, serves to reconcile and to harmonize the apparent discrepancies and contradictions in the views of physicists who have investigated the color of water.

We have already seen that Sir I. Newton and most of his successors, as late as 1847, regarded water as belonging to the opalescent class of liquids, in which the diffuse reflected light and the transmitted light present more or less complementary tints; the former partaking more of the colors constituting the blue end of the solar spectrum, while the latter presented more of the hues belonging to the red extremity. On the contrary, the more recent and more accurate experiments render it quite certain that in distilled water the rays of the red end of the spectrum are more copiously absorbed than those of the blue extremity; so that the emergent transmitted tint is yellowish green or greenish blue. At first view, these results appear to be discordant and irreconcilable; but, it will be recollected, that while even the most carefully distilled water contains a sufficient amount of suspended matter, to scatter enough light, to render the track of the traversing concentrated solar beam visible, yet, in this case, the selective reflection of the blue rays, due to the suspended particles, is not adequate to neutralize the selective molecular absorption of the rays toward the red end of the spectrum. Nevertheless, as has been previously shown, the addition of very minute quantities of diffused suspended matter confers on distilled water the dichroitic properties of an opalescent liquid.

The presence of an extremely small amount of suspended solid corpuscles, by selectively reflecting the shorter waves of light, is sufficient to neutralize and overcome the selectively absorbent action of the molecules of water on the longer waves; and thus, to impart yellow, orange or red tints to the transmitted beam. Moreover, it is very questionable whether any natural waters are sufficiently free from suspended matter to deprive them of these dichroitic characteristics.

Under this aspect of the subject, the views of Newton, derived from the observations of Halley, those of Hassenfratz deduced from his own experiments, as well as the explanations of the green tints of certain waters given by De Maistre, Arago and others, completely harmonize with the conclusions deducible from modern researches, provided the property of selective reflection is transferred from the aqueous molecules to the finely-divided particles held in suspension.

As a striking illustration of the slight causes which sometimes transform the purest water into an opalescent or dichromatic liquid, it may be interesting to detail one of my own experiences. On the 21st of Dec,, 1878, the series of glass tubes employed in my experiments (as previously indicated), being filled with distilled water, the transmitted solar beam presented when received upon a white screen in a darkened room, the usual yellowish-green tint of my winter observations. On the 24th of December, or after an interval of three days, durwhich all parts of the apparatus had remained *in situ*, I was much surprised to find that the transmitted solar beam was enfeebled, and presented an orange red color with no tinge of green. Puzzled to discover what could have produced so marked a change in the optical properties of the liquid, the "scientific use of the imagination" pictured the possible development of ultra-microscopic germ, infusoria, *bacteria*, *confervæ*, etc. The next day (December 25th), the same phenomenon presented itself, when I called the attention of my assistant, Mr. August Harding, who had kindly prepared the arrangement of the tubes, to the anomalous change that had taken place in the color of the transmitted beam. He suggested that as he had used alcohol in cleaning the glass plates, closing the ends of the tubes, and as the plates were secured to corks by means of Canada balsam, the alcohol absorbed by the corks, being gradually diffused, dissolved some of the balsam, which solution, mingling with the water, might produce a fine resinous precipitate, which might stifle the transmitted beam and

* Similarly, Arago has very ingeniously applied the same principles to the explanation of the varying colors of the waters of the ocean under different circumstances, showing that when calm it must be blue by the reflective light, but when ruffled the waves acting the part of prisms, refract to the eye some of the transmitted light from the interior, and it then appears green. ("Comptes Rendus," tome vii., p. 219, July 23d, 1838.)

scatter the rays of shorter wave length, thus leaving the orange-red rays predominant in the emergent light. This view was speedily verified by a critical examination of the track of the traversing beam. A sensible turbidity was visible, in the darkened room, at the extremities of the column of water adjacent to the corks securing the glass plates; and the light diffused latterly at these portions, when examined by Nicol's prism, was found to be distinctly polarized. The emergent beam examined by the spectroscope, exhibited orange and red in full intensity; but the yellow and green were greatly diminished. Ten days later (January 2, 1879) the solar beam traversing the same column of water emerged much brighter than on Christmas day, and the tint was orange tinged with yellow and red. This long repose caused, doubtless, some of the resinous precipitate to become more generally diffused, or to subside, and thus diminished the turpidity of the liquid. The recognition of the dichroism imparted to water by the presence of finely-divided particles in suspension, serves, likewise, to harmonize the conflicting views promulgated by physicists who have studied the chromatic phenomena presented by this liquid. Some claim that the rays of higher refrangibility are more copiously withdrawn by absorption; while others maintain that the rays of longer wave lengths are more absorbed. In many cases the chromatic tints ascribed to selective molecular absorption are unquestionably due to selective diffuse reflection from the ultra-microscopical corpuscles which are held in suspension. (Vide Jamin's "Cours de Physique," 3d ed., tome 3, p. 447, *et seq.*)

ON THE IMPORTANCE OF ENTOMOLOGICAL STUDIES.*

"Occasionally, at the present day, we may hear insects and entomologists spoken of as 'bugs' and 'bug-hunters' —epithets applied in derision to what are regarded as petty objects and trivial pursuits. Such views only betray an ignorance which is equally pi iable and inexcusable. The study of insects has assumed an importance in its direct application to agriculture, horticulture and sylviculture, second to no other department of natural history. It has called to its aid some of the best intellect of the country. Its literature has become extensive and assumed a high rank. Our State governments, in response to demands made upon them, are appointing State Entomologists. Our General Government is making liberal appropriations for entomological work in the Department of Agriculture at Washington, and also for sustaining a special United States Entomological Commission, now in the third year of its operations, charged with the investigation of a few of our more injurious insects.

"The study of insects assumes an importance in this country far greater than in any other part of the world. No where else does mother earth yield in such variety and in such abundance her agricultural products; after supplying to repletion our own people, the excess is distributed to every quarter of the globe. Few, surprisingly few, of these varied products are native to our soil. Nearly all of our fruits, grasses, cereals and vegetables, and perhaps three-fourths of our weeds are of foreign importation—mainly from Europe. With their introduction, very many of the insects that preyed upon them were also introduced, or have been subsequently brought hither. But unfortunately for us, the parasites which preyed upon them and kept them under control, have for the most part, been left behind. As the result, the imported pests, in their new home, find their favorite food-plants spread out in luxuriant growth over broad acres, where they may ply their destructive work without hindrance or molestation, until some native parasites acquire the habit of preying upon them.

"The grand scale upon which our crops are grown as no where else in the world—demanding for their gathering the invention of special mechanical contrivances, and that horse power should be replaced by steam—has also as its attendant inevitable evil, an enormous increase of insect depredations. This may be illustrated by a reference to our apple-tree insects. * * * * * * "In like manner, any and every crop cultivated on a large scale offers strong invitation to insect attack, and wonderfully stimulates insect multiplication."

PROFESSOR J. A. LINTNER.

* From an address before The Farmers' Club, Onondaga Co. N. Y.

CLOUD COLORS.

This P. M., from about 3.30 to sunset, I was witness to a remarkably vivid display of cloud-colors; and thinking that a full description of the phenomena may perhaps help to the understanding of the conditions of the higher atmosphere, I have written out what I saw. The day had been the warmest of the season. The night before was cloudy, and the temperature hardly fell below the freezing point. Light clouds prevailed through the day; at 3.30 the standard and maximum thermometers stood together at 62°, while the maximum sun thermometer registered 119°. The day had been quite still, the direction of the very light wind being from the S. E. The clouds in the neighborhood of the sun were of two varieties, the lower a fleecy and tufted cloud of the cumulus order, moving pretty rapidly from a little north of west, and frequently exhibiting a rapid spiral movement in the filaments, the other would be called cirro-stratus, though not precisely the typical cloud of that name, as portions were quite free from any appearance of structure. In the less dense portions an arrangement in parallel fibres was, however, quite apparent,—one set nearly horizontal, the other inclined at about 45°, the south end upward. The horizontal arrangement predominated, while the other was visible here and there in a detached streamer and occasionally in striæ upon the longer belts, which, hence, were not, as is usual with this cloud, striated perpendicularly to the direction of the bands. These cirro-stratus clouds, which also moved from the west, though with a much less velocity than the lower ones, were the only clouds which showed the rainbow colors. These were exceedingly intense, and changing every moment with such rapidity as to make it very difficult to decide upon the order of the colors, the more so as every filament had its own rainbow, and all were shifting. The red was, however, generally nearest the sun, though sometimes bordered inwardly with intense yellow. The most perfect succession of colors which I caught was in a cloud extending horizontally northward from the sun, in which for a brief interval all the seven colors could be traced following one another, not in the direction of the sun, but vertically, the red uppermost. The violet was, however, so very brilliant as to suggest the beginning of a new rainbow at its bottom, and in a moment this cloud had adopted the form which was most common throughout,—bands of red above and below, with a broader band between of yellow or green or blue. This blue tint was often exceedingly brilliant, tipping both ends of filaments, which were of dull hue in the centre, and bordered above and below with parallel stripes of red. A purple shade was occasionally distinct, surrounded by other colors. This undescribably beautiful display continued over the whole S. W. quarter of the sky, until the sun had been out of sight behind the mountains for more than half an hour.

Though the clouds upon which the colors were observed were of the order in which halos are formed, yet the appearance had very little in common with the halo,—of which we have had a good example within a week. The colors were not only not concentric, but were exhibited successively by different clouds in every direction from the sun, and *at all distances*, from 30°, or, perhaps, 40°, to

not more than 3° or 4°. In fact, about four o'clock the transmitted light was of a splendid green color, tinting the white walls of my room as though through the stained glass of a church. About the time I first noted the colors a strong north wind sprung up, continuing in gusts through the afternoon. F. H. LOND.

COLORADO SPRINGS, *January* 29, 1881.

NOTE ON DR. HENRY DRAPER'S PHOTOGRAPH OF THE NEBULA IN ORION.*

By MR. RANYARD.

*Read before the Royal Astronomical Society, Jan. 14, 1881.

Dr. Draper has sent me an enlarged copy of a photograph of the nebula in Orion, which he succeeded in taking on the night of the 30th of September last. Dr. Draper remarks that September is not the best time of the year, so that he hopes to obtain still better results next summer. The photograph was taken with an exposure of 51 minutes. He does not mention the instrument with which it was taken, but I conclude that it was with his great 27-inch reflector. On the photograph are nine white spots of various sizes; these represent 13 stars in and about the nebula, for the four stars of the trapezium are merged together by reason of over-exposure. In the corner is another small photograph taken with a shorter exposure, and showing three of the four stars of the trapezium. This is not the first occasion on which the stars of the trapezium have been photographed. I, and no doubt many others, have succeeded in obtaining photographs of them. But it is, I believe, the first photograph in which any trace of the nebula is shown. And Dr. Draper may, I think, be very much congratulated on the great success he has attained. The photograph shows the whole of the brighter nucleus of the nebula—sometimes referred to as the "Fish's head." I have compared it with the different drawings of the nebula by Bond, Herschel, Liaponnov, Lassell, Secchi, the Earl of Rosse, and Tempel, and find that it does not correspond exactly with any of them. The drawings differ very greatly amongst themselves, and they differ in type as well as in minor details. They do not appear to differ continuously in order of time, so that the drawings do not afford any proof that the form of the nebula is changing. Photographs will of course afford much more valuable evidence with respect to any such change in the future. The photograph does not show any stars of less than the 9½ magnitude, showing that the brighter masses of the nebula registered themselves on the plate when stars of the 10th magnitude left no trace. If in the future some much more sensitive method of photographing is devised, it will be necessary to contrive some plan by which the brighter parts of the nebula and the light of the brighter stars may be cut off from the sensitive plate during the greater part of the exposure, so as to prevent the irradiation from the brighter parts encroaching over the area occupied by the fainter parts. At present, however, we are very far from being able to photograph, with the sensitive silver compounds* made use of, all that can be seen with the human eye. But even if photography does not make any further advances, photographs such as these will be of very great value in showing the relative brightness of the brighter parts of the nebula.

Mr. Common: I do not agree with Mr. Ranyard, that we must look to photography to explain or prove any change in the form of the nebulæ, because various kinds of plates give different results, and you would not have the same effects produced by the same colored light. I should rely much more on accurate drawings than upon any photographs. If we compare these drawings, here you have [pointing to Father Secchi's drawing] a dark mass with a slope of light running from the left-hand corner down to the right hand. In the other [Lord Rosse's drawing] there is no division, except a large space divided into channels. The latter is wrong and the former clearly right. Before you give details you ought to represent the chief features of the nebula, because it is the features that most readily indicate change. With regard to Mr. Ranyard's remark that no star smaller than the 10th magnitude is shown, there are, I think, two—these fainter stars under the trapezium, which are certainly less than the 10th magnitude.

Mr. Ranyard: I have here the magnitudes given by Liaponnov, and he gives one as the 9th magnitude and the other as the 9th to the 10th magnitude.

Mr. Common: Before we can discuss this photograph we want to know the instrument it is taken with, the focal length, in order to know the size of the image, and the kind of plates used, and the mode of development. If you want to detect any change in the form of the nebulæ you must entirely rely on the hand drawings.

Mr. Ranyard: I think that some considerable scientific use may be made of these photographs; they will at least enable us to compare the relative brightness of the different masses of the nebula as shown on any one photograph, for as far as we know, there is no great difference in the spectrum of different parts of the nebula, and so we have no reason to suppose that the photographic effects of different parts of the nebula in any one photograph would not be proportional to the light.

Mr. Stone: With regard to discrepancies in drawings, I never knew two persons asked to make a drawing of the same faint object make them exactly alike. It is evident that observers draw that which happens to arrest their attention, and one feature will strike one observer, while the attention of another is attracted by something else. A very good instance of this occurred during the eclipse of 1874. Two observers were sitting side by side drawing the corona. The one drew a small nearly quadrilateral corona, while the other drew a large corona with great rays in the equatorial regions. Before a totality was over the observer who had drawn the small corona looked at his neighbor's drawing, and, on looking up again at the corona, recognized the outline which his neighbor had drawn, and commenced to put it on paper when the eclipse ended. There is therefore a great element of uncertainty about drawings, one observer overlooks one part, or is struck by one part, and another by something else.

Mr. Rand Capron: I think that Mr. Commom is right, that photographs of objects taken with different instruments and plates will probably never usefully bear comparison; but I agree with Mr. Ranyard that photographs of the same object taken from time to time with the same instrument and the same plates can most usefully be compared.

Mr. Burton said: I should like to suggest that the difficulty which Mr. Ranyard has referred to, with regard to the irradiation from stars interfering with the fainter parts of the nebula, might be got over by placing a prism of small angle, made of quartz or Iceland spar, between the object-glass and the photographic plate. The images of the stars would be drawn out into lines, while there would be three or four images of the nebula which would not interfere. The principal plane of the prism might then be turned round into a different position-angle, and another photograph taken, so that the spectra of the stars would fall in another direction.

Mr. De La Rue said: I recollect very well the time when the Earl of Rosse's drawing was made. I compared

*[Note by Mr. Ranyard.] It seems probable that the small pencil of light, which passes through the pupil of the eye from the faintest object perceived, produces an actual change in the matter of the rods and cones, which is rapidly obliterated by the circulation and vital processes going on about the retina. This is now, I believe, pretty generally agreed to by physiologists. If in the future the matter acted upon in the rods and cones can be isolated, and the change produced by light can be rendered permanent, it seems probable that, by means of large lenses and reflectors, we may some day obtain photographs of objects too faint to be visible with the naked eye.

it with the nebula with very great interest at the time, and I cannot agree with Mr. Common in preferring Father Secchi's drawing. It seems to me that the Earl of Rosse's drawing is much the more accurate in respect of details. As regards contour and outline, that depends very much upon the amount of light, which impresses one man's eye rather than another's so that the general outline may be extended much more in one case than in another. Lord Rosse's drawing does not give the whole sweep of the nebula, and does not take in so extensive a field as Father Secchi's drawing. Lord Rosse's drawing is better seen in the black upon white print than in the white upon the black ground.

Mr. Common said that there was a great black channel in the nebula, which is well shown in Father Secchi's drawing, but is lost in the Earl of Rosse's drawing. The latter drawing seemed to him too full of detail.*

Mr. Ranyard said although the actual brightness of various parts of an object like a nebula or corona cannot be judged of from the opacity of corresponding parts of photographs, yet a photograph will enable one to tell with great certainty which is the brightest region of the object photographed, and it affords a very valuable permanent photometric scale, by which various degrees of brightness of one region relatively to another may be judged of. For example, Dr. Draper's photograph shows that a nebulous mass on the preceding side of the trapezium is the brightest region of the nebula. This does not correspond with any of the drawings. It is of course possible that the actinic light of the nebula does not correspond with its luminosity as observed by the eye, but this supposition is not very probable, as the spectroscope does not show any striking differences in the composition of the light of the nebula. The photograph enables us to judge very well of the relative magnitudes of the stars involved in the nebula. I have compared the magnitudes of the images of the stars in the photograph as enlarged by irradiation, with the magnitudes of the same stars as given by Liaponnov, and I find that they correspond very accurately. No doubt it may also be assumed that the brightness of various regions of the nebula may be compared with equal safety by noting the opacity of corresponding parts of the photographic film. With regard to Father Secchi's drawing and the drawing of the Earl of Rosse, I agree with Mr. De La Rue that I rather prefer the Earl of Rosse's. It shows a much smaller region of the nebula, and I must remark that I have not much faith in the existence of these outlying nebulous structures shown in Secchi's and Tempel's drawings. If such structures exist the nebula would occupy an area of more than a degree, and it ought to be seen with the naked eye better than with any telescope. Every one is familiar with the way in which a faint structure like the tail of a comet—which can be easily seen with the naked eye—is lost when viewed with the best of telescopes. A telescope of whatever aperture will not increase the brightness of an object occupying a sensible area.

Mr. De La Rue: Lord Rosse's drawing does not embrace such a large area as Secchi's, and you do not see the contour definitely marked as you do in Secchi's. If you cover those parts of Secchi's drawing down to the extent of Lord Rosse's drawing then the difference of outline that strikes Mr. Common would to a great extent disappear.

Mr. Mitchell: If you get a definite chemical compound with which you make your photographic plate, and can obtain a definite exposure, and know the other conditions of temperature, and so on, I think that it can not be doubted that you would have a more reliable record than if the varying conditions of the brain, at one time and another, have to be taken into account. If the condition of one man's brain has to be compared with the condition of the brain of another man, physiological difficulties come in which may be avoided by means of photography. In comparing photographs you have only mechanical differences and physical conditions to consider, which certainly involve much less complication than physiological differences.

* [Note by Mr. Common.] Reference to the drawings here mentioned was only made incidentally, and with regard to one point. As to which of the two is the better one, I have no doubt in my mind, nor need any one have who looks at them with a recollection of the real object. What I wanted to point out was, that owing to a proper contrast not having been made in Lord Rosse's drawing, the general appearance, or what we would tails the leading features, was lost, and a drawing excellent in all the decall fails in these leading features.

ASTRONOMY.

MAGNITUDE OF JUPITER'S THIRD SATELLITE.

On the evening of February 2, Jupiter was passing near the star B. A. C. 303 (73 Piscium, and the opportunity was taken at the Observatory of Harvard College to compare photometrically the third satellite of the planet, with the star. Three observers took part in the work, and four sets of measurements, each consisting of eight single comparisons, were made. The result obtained was that the star was fainter than the satellite by 0.38 magnitudes of Pogson's logarithmic scale. For the magnitude of the star we have 6.16 by the mean of the available estimates on record, and 6.17 by the observations made at this observatory with the meridian photometer. The resulting magnitude of the satellite is 5.28 or 5.29, in close agreement with the value, 5.24, found by a very different method, in the Annals of the Observatory, Vol. XI., p. 276.

SWIFT'S COMET.—We are indebted to Prof. Pickering for the following list of dates on which observations of Swift's Comet (1880 e), were obtained at Harvard College Observatory, by Mr. Wendell:

1880, Nov. 3,	1880, Nov. 27,	1880, Dec. 28,
" 8,	" 29,	" 30,
" 9,	Dec. 2,	" 31,
" 11,	" 3,	1881, Jan. 1,
" 18,	" 4,	" 3,
" 19,	" 7,	" 7,
" 21,	" 11,	" 8,
" 22,	" 19,	" 18,
" 23,	" 22,	" 20.
" 26.	" 23.	

URANIA.—The first number of the new *International Journal of Astronomy* contains in a very convenient form of 24 demy 4to pages, a number of interesting articles. Among others are the following papers: "Observations of the Spectrum of Comet 1880 d, (Hartwig) at Dunecht," by Copeland and Lohse. "A New Planetary Nebula," by Dr. Copeland. "Observations of Comets 1880 b, c, and d, at Dunecht. "Uber die Auflösung der Lambert'schen Gleichung für Parabolische Bahnen, by Professor Klinkerfues.

PROF. WILLIAM A. ROGERS, of Cambridge, has recently made a visit to Washington to compare the copies of the English and French standards of length, with the standards of our Government deposited at the Coast Survey Office. Prof. Rogers obtained very accurate copies of the yard and metre during January and February, 1880 having made a trip to Paris and London for that purpose.

WE learn of the recent death of Baron Dembowski, the well-known double-star observer, at the age of 69. For upward of twenty-five years he had devoted himself to the re-measurement of the stars of the Dorpat Catalogue, and for this work was awarded in 1878 the gold medal of the Royal Astronomical Society.

W. C. W.

BOOKS RECEIVED.

ASTRONOMY FOR STUDENTS AND GENERAL READERS. By SIMON NEWCOMB, L. L. D., and EDWARD S. HOLDEN, M. A. Second Edition, Revised. Henry Holt and Company. New York, 1880. $2.50.

It may be supposed that the joint efforts of Dr. Simon Newcomb and Professor Edward S. Holden to write a work on Astronomy has resulted in the production of a work which may be accepted by the public as a reliable and able exposition of the subject.

The attempt, however, to compose a text book in Astronomy which should be equally applicable to the class of a college, and to the general reader, was a task which presented few elements of success; we are not therefore surprised to find that the authors candidly state in their preface that in spite of the title selected for the book that the work was principally designed for the use of those who desire to pursue the study of Astronomy as a branch of liberal education.

Regarded in this light the work is a great success, for the general reader will find by a careful perusal of this manual that he has mastered all the leading points in the study of Astronomy in sufficient detail, to enable him in the future to fully comprehend whatever he may read on this subject. The work in question may well serve as a model for those desirous of writing scientific manuals; in simple, but forcible language, the most complicated explanations are presented in a form that may be comprehended by a reader of ordinary intelligence without mental effort, while the interest of the student is maintained throughout.

The description of astronomical instruments and their uses forms a valuable portion of the work, and all the details of observatory work are explained by the aid of good illustrations; thus all the methods by which astronomical research is carried on at the present day are described by one who was himself at that time a member of the corps having in charge one of the most completely equipped observatories that has yet been organized.

The three branches, into which Astronomy is now divided, are all ably treated by the authors, and it is not difficult to detect the plan adopted by the authors in dividing their work.

We regret this manual was not considered worthy of a good index, for on this account the book is valueless as a work of reference. In future editions it would be well to remedy this unnecessary defect.

CIRCULARS OF INFORMATION OF THE BUREAU OF EDUCATION. NO. 4, 1880. Rural School Architecture, with illustrations.

NO. 5. English Rural Schools. Washington, Government Printing Office, 1880.

The first paper (No. 4) presents a concise yet complete treatise on the proper construction, heating and ventilation of school buildings, prepared by Mr. S. M. Clark, a well-known architect of Boston. The aim of the paper is not so much to lay down rules to be inconsiderately followed, as to give principles and directions suggestive of the plans best to be followed under a variety of circumstances.

This is a thoroughly practical paper, and the whole subject has been well handled by Mr. Clark, and the pamphlet cannot fail to be most useful to School Boards and Committees. The Commissioner of the Bureau of Education deserves the thanks of all heads of families for ordering the production of this timely publication, which, however, merely applies to rural districts, and we trust the manual treating on buildings for high schools, academies and colleges will be published without delay, as it is a matter of common notoriety that the health of children in many of the large cities, is sacrificed in consequence of the school-rooms being constructed without regard to hygienic principles.

No. 5—Is a description of the condition of rural schools and the progress of elementary education in the rural districts of England, written by Mr. Henry W. Hulbert, late of Middleberry College, based on his personal information. He does not attempt to enforce lessons from his facts, but leaves these to the reflection of the reader.

The facts presented by Mr. Hulbert are most interesting, and would appear to indicate that the effort to educate the masses of the people is making slow but steady progress against the opposition raised against it by certain classes.

We have the authority of Mr. Heller, of the *National Union*, that there was a great cry at first, but advanced education would increase the crime of the land. Of course the contrary has been the real result, and it is stated that there is manifestly less coarseness of manners among the lower classes.

It is admitted, however, that a certain restlessness has been created by advanced education, and "that it has driven children into towns to seek what they consider higher situations, and in some cases it has led to emigration."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

DR. FLEMING'S CLASSIFICATION OF SCIENCE.

To the Editor of SCIENCE:

There are numberless ways in which knowledge may be classified, as the numerous systems of classification put forward show, and it appears to be a very common notion that all knowledge may be put in a serial order showing the elements in logical dependence. However this may be in metaphysical matters, it is certainly not possible to do so in physical philosophy, for the various manifestations of energy are mutually co-related, and starting with any one of them it is possible to develop almost any of the other forms. In the scheme of Dr. Fleming this does not appear, but in the plan of it he places the doctrine of the correlation of forces high up in Physico Dynamic Science instead of making it almost the first division of Physics. The latter as now understood is the science of energy, and energy always involves two factors, one a mass and the other a velocity. When motions are considered in their geometrical relations, apart from mass, the science is known as Kinnematics and as a branch of pure mathematics, it has nothing more to do with physics proper than has geometry, though all problems in physics are more or less mathematical problems, but they become *Dynamic* when mass is involved. Inasmuch as masses of all dimensions, from an atom to the sun, follow the same laws, it surely is not a scientific proceeding to make a grand division here of Astronomy as distinct from the more general division of Mechanics. Astronomy so far as pertains to the genesis of the Stellar Universe is only a development or application of mechanics to large masses of matter. Again the author is mistaken when he says, "Then Natural Philosophy monopolizes the whole field. Now Chemical Philosophy has taken the rank of a distinct department." The fact is that since the discovery that chemism is dependent upon *mass*, the science has been swallowed up entirely in physics, and every so-called chemical problem is a pure physical problem. Chemism is one of the correlated forms of energy and the logical importance is the same as that of heat and electricity.

B. D.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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P. O. Box 3838.

SATURDAY, FEBRUARY 26, 1881.

INDEX FOR SCIENCE.

In accordance with the promise made in our first number, we have prepared an elaborate Index for Volume One.

Genera and species are printed in italics; also the names of recently discovered stars. There is a separate Subject and Authors' Index, the whole having 4576 distinct references. "SCIENCE" thus becomes a valuable standard work of reference, which should be found in every library. The Index has been sent to regular subscribers; others can obtain it on payment of twenty cents.

THE NEW YORK AQUARIUM.

With the closing of the New York Aquarium the city will lose an institution that might have been made a source of instruction to the people, combined with an agreeable place of recreation, and the causes of its failure to be remunerative may be studied with advantage by those who desire to have a permanent public aquarium in this city, thriving on a paying basis.

We observe that the present proprietor, Mr. Charles Reiche, makes the assertion "that such a place is not appreciated by the people." We consider that such a statement has been amply refuted by the very fair amount of patronage received, even at a price for admission which was practically prohibitory to the majority of those who would have visited the place in thousands.

Neither do we believe that the faults of the management can be charged with the failure, and we have as little faith in the other reasons which have been suggested. In our opinion the whole enterprise was killed by being loaded down with heavy expenses, and too profuse expenditure by those who controlled the finances.

Unfortunately, there was too much money at command from the start, and by the time experience of the proper course to pursue had been gained, the capita had been squandered, and the demoralization which finally led to the ruin, had set in.

To saddle the enterprise with a rental of $10,000 a year for the bare ground on which the building stood was to court ruin, but all the outlays were made on the same extravagant basis. Then came the fatal mistake of appealing for support to the few affluent, and making each admission fifty cents, instead of trusting to the multitude who could and would have paid twenty-five cents.

Even under these circumstances we are now told by Mr. Reiche that *for a time it did pay*. We think this very convincing proof that under more economical management and with a less pretentious establishment, success would have been secured.

On behalf of many scientific men, we extend our thanks to Mr. Reiche for the liberal facilities he has throughout extended to those who desired to visit and make use of the aquarium for scientific purposes; to such the place has always been open and a cordial welcome given. Under instructions, the officers in charge have been courteous in offering the fullest facilities for study and freely gave such specimens as could be spared. How little such opportunities have been appreciated and used by naturalists within reach of the institution reflects little credit on those who should have seized the occasion with avidity.

Unfortunately the facilities were too great, and too conveniently at hand to be appreciated, and because they were offered as a gift they were neglected. The New York Aquarium had the benefit of the services of the best professional collectors in this country, and the coast from Maine to Florida was constantly searched for living species of rare and interesting forms of animal life, and yet many naturalists preferred to waste their time and money, travelling hundreds of miles, to obtain objects which could be had at their very doors.

The same results have happened in Europe under similar circumstances. When Mr. Lloyd, of London, was asked if he thought the aquarium at the Channel Islands would answer, he replied, that he feared it was too near home, too convenient of access; for said he, "I have known persons prefer to travel from to the Bay of Naples to collect specimens, which I had in my aquarium at the Crystal Palace."

We trust that steps may be taken to preserve the fittings of the New York Aquarium, and that they may be replaced in some part of the city where a site will be inexpensive, and that a plan may be arranged for maintaining it on a remunerative basis, which in our opinion should not be a difficult matter; but to secure success we should advise the institution to be placed in the charge of some well-known professional naturalist, who could be well named by Professor Spencer F. Baird.

SMITHSONIAN INSTITUTION.

In its annual report for 1880 the Smithsonian Institution purposes publishing a bibliography of American Anthropology for that year. The list will include not only the titles of works in that special branch, where-ever issued, but also the publications of American scholars in all departments of this science, and you will confer a favor on the establishment by sending it a copy of each of your works upon the subject published during the year 1880. Should this be impracticable, however, please send a list of your own memoirs and of those of the scientific associations with which you are connected, bearing upon Anthropology, in each case giving the full title, author's name, edition, imprint, size, number of pages, maps, engravings, etc. If the publication forms a part of a periodical or of the proceedings of a scientific association, the fact should be distinctly stated. In the case of separate works, references to periodicals in which reviews have appeared should also be given.

In order to give permanent value to this list and to obviate delay in the appearance of the volume, you will oblige the Institution by complying with its request as soon as possible.

SPENCER F. BAIRD,

Secretary Smithsonian Institution.

WASHINGTON, D. C., February 1, 1881.

SCIENTIFIC SOCIETIES OF WASHINGTON.

THE BIOLOGICAL SOCIETY.—At the Biological Society, Friday evening, February 11, the entire evening was spent in discussing the annual address of President Theodore Gill, delivered at the previous meeting. Dr. White and Professor Ward, in company with the President, reviewed the arguments which have been offered by various naturalists, including Professor Burt G. Wilder and Dr. Coues, for the existence of antero-posterior symmetry in the vertebrates. The conclusion reached was, that, while there are many and very plausible reasons in favor of this view, on the whole, the weight of testimony is on the opposite side. Dr. King gave a description of one or two cases of hermaphroditism which had come under his notice. This was the occasion of an interesting discussion as to the meaning of the term and the possibilities of the phenomenon in the human subject. Professor Ward brought forward the arguments of Haeckel for the establishment of a kingdom of nature intermediate between the Vegetable and the Animal Kingdoms.

THE ANTHROPOLOGICAL SOCIETY,—The Anthropological Society met on Tuesday evening, February 15, Vice-President Mason in the chair. The following papers were announced: "Some peculiarities in the use of moods in the principal Neo. Latin Languages."—By H. L. Thomas. "Aboriginal burial cave in the Valley of the South Shenandoah."—By Elmer R. Reynolds. "Amphibious aborigines of Alaska."—By Ivan Petroff. Mr. Thomas, the translator of the State Department, announced that the object of his paper was to follow the history of the Latin rules respecting the sequence of moods in complex sentences in the languages of Southern Europe, commonly called Romance or Neo-Latin. The author directed attention to the fact that numerous editors of the last few centuries had made changes in the moods of Latin verbs in order to bring them under certain fixed rules from which the Latins had never varied. By numerous citations from very old editions these changes were exposed.

The next point elaborated was the national peculiarties which had manifested themselves in the adoption of Latin rules. The Portuguese, Spanish, French, Italian and Romance proper, had all affected the Latin usages in the use of the subordinate subjunctive, but had done this, so to speak, in their own way: which gives to the subject a special ethnologic value.

The author justly paid a high tribute to the opinion set forth by Professor Fay, in a communication made to the Society last year, that we are not to look in classical Latin but in the old Roman folk-speech for the ancestor of these borrowed forms. During the discussion which followed by Professor Antisell, Dr. Welling and the chair, the interesting question was mooted whether in the advance of scientific certitude the use of subjunctive or doubtful forms were not sloughed off.

Dr. Reynolds gave a brief but highly interesting description of a visit to a cave in Page valley, Virginia, near the celebrated Luray cavern, containing numerous human remains. The Smithsonian Institution had sent out many hundreds of circulars, to every post-office in the United States, but had failed to receive information of a single mound or permanent remain in the valley of Virginia. Dr. Reynolds, in the short space of a month traced twenty-five mounds, ossuaries, forts, ateliers, and bone caves. The paper was illustrated with a large collection of human bones, stone implements, and pottery.

AMERICAN CHEMICAL SOCIETY.

THE February *Conversazione* of the American Chemical Society took place Monday evening, the 21st inst. No papers were read, but a number of interesting specimens were exhibited. Among these was a quantity of that poisonous alkaloid, nicotine, which Mr. William Rupp, one of the curators of the society, had himself prepared. Mr. P. Casamajor, by means of a microscope with an 8-10 objective, showed a simple way to distinguish between pure sugar and that adulterated with glucose. The former crystalizes in large and characteristic forms while the glucuse appears much finer, and as poorly defined cystals. So that when the two are mixed no difficulty would be had in distinguishing the adulterated from the pure, provided a microscope was used.

A large piece of glass painted with Balmain's Luminous Paint was exhibited by Mr. M. Benjamin. This paint was discovered in 1877 by Mr. Balmain, an English chemist, and has recently been brought to this country. It possesses the peculiar power of phosphorescence, or the property of absorbing light during the daytime, and then emitting it in the darkness. It is prepared by calcining oyster shells with sulphur, and treating the resulting calcium sulphide with the proper articles necessary to form a paint.

Its uses are numerous; miners lamps are painted with it, and used instead of the ordinary safety lamp; it has been suggested that screens coated with this paint be used for illuminating purposes along the galleries of mines. Its marine applications are very important. the painting of life buoys, and also stationary buoys, so that they can be seen at night-time, the hulls and rigging of ships treated in this manner might prevent collisions. Divers costumes painted with it are found to yield light after the diver has descended, in fact, sufficiently so to enable him to distinguish quite minute objects.

Tunnels may be illuminated by this paint. It has been successfully employed to light railway cars at night time. The time of night is readily told from clocks and watches whose faces are coated with this substance. Signs and advertisements are among the many uses to which it may be put. More appplications will suggest themselves to every one. M. B.

N. Y., Feb. 22, 1681.

ORGANIC HEALING POWERS.

A LECTURE BY RUDOLF VIRCHOW.

[*Translated from the German by the Marchioness Clara Lanza.*]

Andrew Jackson Davis, who is called the "Great Prophet" by his German adherents, thus begins a chapter in his "Harmony"[1] entitled "The Philosophy of Disease:"

"The improvements and progress which have been made in pathological science, are not by any means in keeping with its actual value and antiquity." And then he adds the following:

"The age of a science or doctrine has but little to do with its reliability, importance or progress. Indeed, the great maturity of any doctrine is almost a positive proof that it originated in ignorance, superstition and error."

The "Great Prophet," who conceives all his ideas without the aid of study, and who, moreover, by a peculiar direction of his will, turns from the confining influences of the material world in order that he may enter the "highest state," has entirely overlooked the fact that the ancient science which he disdains, proceeded from precisely similar revelations as those which he produces with so much pride.

Welcker, in his magnificent work upon the "Art of Healing Among the Ancient Greeks,"[2] has given very impressive descriptions of the Epiphania which occurred more than 2000 years ago in the Temple of Æsculapius, and they now possess a double interest in regard to American Spiritualism or spiritualism of any kind, (if we consider for a moment how philologists a quarter of a century ago investigated the question) as to whether the so-called incubation of the Æsculapians was identical with modern clairvoyance. Those seeking to be cured from disease obtained revelations while sleeping or dreaming in the sanctuary of God. Hence medical literature arose, for the afflicted wrote a description of their cures upon the pillars of the Temple or else upon certain consecrated tablets, and from them the forefather of medicine, Hippocrates, collected in the Temple of Kos those memorable "Predictions" which can be considered one of the principle sources of our scientific knowledge.[3]

Did all this spring from "ignorance, superstition and error?" The point perhaps cannot be contested, but it contains, nevertheless, a large portion of veritable experience, and Hippocrates, not withstanding his direct descent from heathens was a too critical and (remarkable as it may seem) a too worldly person not to expose everything which partook merely of a sacerdotal or superstitious character.

In his writings and in those of his followers, there is nothing supernatural to be found. The gods no longer heal the sick. Nature does it, and nature, moreover, does not act in accordance with instantaneous inspiration. On the contrary it is subject to "divine necessity," or rather we should say to eternal and also divine laws.

Since the remote period before referred to, opposition has been openly declared between science and superstitious therapeutics. The latter even now has certainly not died out. The countrymen of the "Great Prophet," that is to say, the medicine men among the North American Indians still boast of their immediate intercourse with the Great Spirit, and perhaps it is the proximity of these people which promotes the increase of spiritualism throughout the United States. One of the nations of North Asia[4] beats a magic drum, while a certain people in South Africa blow an enchanted trumpet in order that the evil spirits of disease may be dispelled. However, we do not need to go so far for examples of this kind. In our immediate neighborhood the traditions of heathenism rise up secretly and flourish, while superstition concerning mystical healing powers is capable of continually bringing forth fresh fruit.

Conjuring, however, during the past century has rapidly declined. I, myself, remember that during my childhood many people of the middle classes where I lived believed in fire conjuring. Even at the present day you will scarcely find one German city where the worth of a fire brigade is not undervalued on account of the possible termination of a conflagration may have in consequence of conjurations.

In one of those old Greek writings, which, on account of its age, has been attributed to Hippocrates, and has for its subject Epilepsy, or the divine disease[5], which, at that time was treated by magic, the author says that those who conferred divine names upon diseases were merely magicians, purifiers, pious beggars, and coxcombs, who gave themselves the airs of God-fearing individuals, but who, in reality, knew no better how to conceal their perplexity than by taking refuge behind the deities.

How many years have passed since then! The Olympian Gods have been shattered for ages; even Christianity has by degrees become an old religion, and yet with it all, epilepsy has not ceased to be the subject of conjuration and magic.

Superstition, no matter how degraded, will always outlive faith. The fathers of the church belonging to the first Christian century, fought and struggled in vain against the traditions of heathenism. Chrysostom said that a Christian had better far endure sickness and death than have his health restored and his life lengthened by means of amulets and exorcisms. But the Christians would not listen to this voice, and in the end the Church was forced to make amends. When it erected its places of worship upon the very ground where formerly were temples and sacrifices, and changed the heathen festivals into Christian ones, new methods of supernatural cure were instantly put into practice. Even the kings by God's favor did not hesitate to adopt this sort of accomplishment—not only the most Christian kings of France, but also those of England, until the first representative of the House of Hanover mounted the throne, Catholic and Protestant alike cured scrofula by discourses and sundry calming influences. At that time the disease was called "Kings' Evil," just as epilepsy was termed the "divine disease."

Such obtuseness in regard to traditional superstition may seem astonishing, not to say alarming. It lies, however, deeply imbedded within the human mind. How long has the fear of ghosts at night been kept up, while scarcely anyone dreads spirits in broad daylight? According to the testimony of Signoria Coronedi, people in Bologna burn daily the combings of their hair, to the end that no witchcraft can be perpetrated upon them, and I remember distinctly that when, as a boy, my hair was cut, the clippings were carefully thrown into the stove.

The inhabitants of some of the Malay Islands fear that a magician will have their lives in his power should he take the remnants of their meals and burn them in a peculiar sort of ashes called *Nahak*. Everywhere we find the same childish tricks performed by men in the lower orders of life that they may create fictitious personalities, endow living or inanimate bodies with imaginary powers and trace out the superior force of spirits in purely natural incidents. This is nowhere to be seen so plainly as in the origin and cure of disease, and if the source of various maladies is referred to enchantment, possession or dispensation, it arises mainly in regard to the cure to be effected.

[1] Andrew Jackson Davis, M. D. *Harmonious Philosophy Concerning the Origin and Destiny of Man—His health, disease and recovery.* Leipzig, 1873, p. 93.

[2] F. G. Welcker, *The Art of Healing Among the Ancient Greeks. Bonn,* 1850, p. 95, 112, 151.

[3] *Magni Hippocratis Opera Omnia*, Edit. Kuhn, Leipzig, 1825, Vol. 1, p. 234.

[4] O. Peschel. *Knowledge of Nations.* Leipzig, 1874, p. 274.

[5] Hippocrates. *De Morbo Sacro.* Welcker, p. 587.

The reason can easily be comprehended. While we are familiar with the natural causes of maladies we are still in want of a well organized acquaintance with their natural preceding incidents. By taking an unprejudiced view of the case, we can easily see that even Hippocrates had recourse to nature in curing diseases. Physics, he designated the basis upon which the healing incidents rested, and there can be no doubt that this term was the same to him as is to us the tautological epithet of the "physical nature of man." If you read attentively the part in which he mentions this, you can no longer doubt that he had the whole question of man's bodily formation foremost in his mind. Taken in this sense, the healing powers belonging to the body itself must consequently be natural or physical organic forces.

The idea, however, was in a certain measure a prophetic one. Knowledge at that time was not sufficiently extensive to admit of, or to supply any explanation of it. Even the most favorable and clear sighted observations relating to natural incidents in healing, led to nothing more than a superficial, and to a certain extent, brief conception of the events. This sufficed certainly to establish their situation, and also furnished abundant cause for application of remedies at certain times and on particular parts of the body, remedies which seemed adapted to facilitate the natural course of events, to favor it, or in case it remained concealed, to bring it forward.

There have been numerous attempts to explain all this. One school after the other produced its doctrines, but each one of them was based upon imperfect or voluntary suppositions. Each new step of progress in the knowledge of various occurrences which take place in the human organization overthrew the opinion under consideration and produced another. Of course this did not conduce to strengthen faith in regard to scientific medicine.

It was only during the period of spiritual inactivity when nature's perceptions remained for a long time unchanged as in the early portion of the middle ages, and the Church as well as Medicine adopted natural science in its system of teaching, that medical doctrines gained for themselves the recognized character of stability. It was then that the physician attained aristocratic honors. However secondary schools then arose and dilettanteism pushed forward into existence. So it was at the time of the German revolution, the French revolution and the formation of a new German kingdom.

At no period whatever has mysticism been wanting. A peculiar form of it deserves to be especially mentioned. It is called mystical calculation; its origin lies buried in the most remote practical teachings. Hippocrates himself, observing a country which up to this day is shunned on account of its malarial influence, has established with minute exactness the duration of the feverish maladies which arose from the marshy districts with peculiar regularity. He not only ascertained the precise duration of the fever, but also the days when a decided crisis would appear. The numbers acquired served to denote when the treatment should be discontinued as the critical days, the 7th, the 11th, etc., designated the proper time for the administration of remedies. In this way the calculating system became celebrated, and as it was made a subject of universal contemplation before the days of Hippocrates by the various philosophical schools, we cannot be surprised that those who succeeded them thought to recognize in the theory more than mere expressions concerning the legitimate relation of things to each other.

During the Middle Ages astrology formed a close alliance with medicine, and the constellations occupied the places of the ancient oracles. But even subsequent ages have repeatedly had recourse to conceptions which nearly approach those of the Pythagoreans. Particularly towards the close of the preceding century, discoveries in the departments of electricity and magnetism caused the biological sciences to adopt the theory of polar attraction, a doctrine in which the heterodoxy of animal magnetism, and its companion spiritualism is firmly rooted. In the Pythagorean philosophy, a two-fold existence was supposed to be at the root of everything, and the circulation of this doctrine has resolved itself, so to speak, into the "Great Prophet" of America, according to whose conception Providence is a moving substance formed of positive and negative proportions, and which acts upon matter in different ways through the agency of the number 7.

Among all these attempts to grasp the phenomena in a determined manner, an effort comes to light which is in every way worthy of recognition. It has been shown that the human intellect has no more a universal and spiritual form which can establish the relation and conception of things, than it has a material one. Calculation produces the definite value by which we are enabled to assign things to their proper places. It is for this reason that intricate natural sciences, physics and chemistry partake every day of a more mathematical character. The descriptive natural sciences follow timidly in their footsteps, and even physiology and psychology have already been made to travel over the same road. How then, could medicine escape?

However, the numbers 2, 3, 4, 7 and 10, do not suffice to explain the infinite multiplicity of things, even if the combination of ten numbers serves to account for each calculation. Every reckoning about actual things rests upon observation and not upon inspiration. The more difficult the calculation, the more complex must have been the preceding observation which went to supply the elements of the reckoning. This is true, earnest work, such as no one individual is capable of producing. One workman assists the other, and one generation helps another, not only in transmitting results, but also their aim and object.

It will be a difficult task, nevertheless, for any generation to recognize self-acting forces in numbers. If two objects attract each other it is not owing to the things themselves. And there is no number in existence which possesses healing powers, and no talisman compounded of numbers which possesses active force. The numbers supposed to play an important part in disease only serve to give those versed in art the means by which they may discover the time and duration of the malady and arrange their mode of action accordingly.

But just as Astronomy is incapable of moving the moon or planets by means of numbers, so is the physician unable to produce any effect upon the course of disease or recovery by the same process. Numbers are not remedies, for remedies are actual things, which stand at the disposal of medical art; are actually applicable, and which possess in a certain sense real powers of healing. When we consider them, however, we come to a lengthy and apparently increasing contention which is embodied in medical history in the names of physiologists and technologists. Physiologists are those who seek healing powers within the physical organization itself, while technologists think to recognize them in such means or influences which exist independently of the patient and are directed toward him.

It is true that the physiologist does not altogether despise remedies, but they only serve, in his opinion, to set the organic powers at large. The technologist, on the contrary, intrenches upon the organism. He forces life into artifical conditions. He "orders" and "prescribes" where the physiologist is satisfied with existing circumstances and comes forward as Nature's servant.

Of course a long time has elapsed since the controversy between these two schools was at its height, but in some recent accounts it appears again, not only in specified cases of treatment, but also in a general sense.

Not many years ago blood-letting was a daily occurrence in every hospital, and indeed in almost every private practice. Now it has become so rare that young physi

cians are scarcely acquainted with it. When I was a young hospital assistant I was frequently forced to perform cupping four or five times in one morning. Singularly enough the change came at a time when we were the least prepared for it. In cases of inflammation of the lungs, where the most audacious blood letting was considered an almost irrefragable means of restoring the patient, they began in the Universal Hospital at Prague to observe the natural course of the disease without the application of any remedies. They contented themselves with giving the patients plenty of fresh air, good attendance, greater cleanliness than they were in the habit of getting and strict dietetic surveillance. In the way of medicine, they got nothing, and yet very favorable statistics were obtained. In this way physiology gained a victory over technology, and at the first step reached the highest form—nihilism.[6]

Since then a certain reconciliation has taken place. A firm conviction arose that hospital practice could not merely be influential to private practice—that the hospital, with its manifold contrivances, its order and regimen, possessed provisos and remedies which in a private family, even a wealthy one, could only be imperfectly established, or else not at all—and finally that the nihilism of the hospital physician could not be transmitted to families.

Of course, both physiology and technology will continually enlarge in the future, the more so as experience gains new perceptions and increased power. This, we all know, is inevitable, and the public, which might justly reprove medicine for its scientific changeability, should bear in mind constantly that it is the fate of humanity to be fickle, not only in regard to science, but also every other matter, from the State to the Church. We can only hope that changes everywhere will be made with as much honest intention as they generally are in regard to science.

It would, perhaps, be possible to check trivial fluctuations if people could only agree better as to proper healing objects. This is precisely the point over which scientific men find it so difficult to attain to a uniformi y of opinion. When a physician is called upon to cure he has the case before him, represented by the patient— a unity so to speak. And yet the malady itself gives the impression of another unity. It has the appearance of some strange being which has implanted itself in the individual. It has been not improperly termed a parasitic organism, which lives in or upon the system of the patient. Numbers of times it has been asserted that a strange existence has penetrated into the sick man and "possessed" him. All these ideas unite in the practical task of expelling the disease by driving it forcibly from the body. Is it not perfectly evident that a double existence takes the place of the former unity? Can any conclusion be drawn from such premises, except that the "case" must be regarded as dualistic? If the physician has the patient *and* the disease before him; if he is to separate the one from the other; if the practical endeavor is to act *against* the disease and *for* the individual, can it be a question of a unitarian conception?

Truthfully speaking, such an idea has never properly existed. Even in cases of sickness which were termed rather figuratively universal, it was always understood that a more or less large portion of health should remain undisturbed. It was this remainder that caused "reaction" according to some schools, and led the battle against strange intruders. Paracelsus, in the Middle Ages, expressed these thoughts in the most worthy manner. Let us take up the point and imagine a defensive battle whose seat of action is the human frame. Who are the combatants? On one side we have the disease, in the other the healthy portion. The latter, of course, can go forth with no other weapons of defense and attack than those previously possessed. Where can new ones be found? The means of resistance must necessarily spring from the physical system itself. Thus far the ideas are simple enough. But if we see that the struggle is carried on according to a military principal, that it has a tendency to cure, and that the means of reaching this end are apparently, purposely and systematically chosen and put into action, what power shall we consider the decisive one? What is the leading principle, and where are we to look for it? The generality of physicians say with Hippocrates, it is Nature. But do we not, so to speak, run around in a circle when we first of all call the legitimate formation of the body nature, and then again have recourse to the same term when we wish to explain how this arrangement resolves itself into a systematic unitarian course of action? Have we not a substance to deal with in the first case, and a force in the second—and an organized force too, a force with designs and purposes—a species of spirit in fact? Paracelsus was firmly convinced on this latter point. He designated the decisive power the *Archæus maximus*, which corresponded to *spiritus rector*, or leading spirit.

Georg Ernst Stahl, the celebrated clinical lecturer, in the beginning of the past century, went a step further. He set up the soul itself, the *anima*, as the decisive principle. But at that time the philosophy of the unknown was not yet invented, and it was difficult to demonstrate that the hitherto thinking and conscious soul could here work in an entirely unconscious manner, and yet be systematic withal. It was also extremely hard to trace the diseases of cattle, the *morbi brutorum*, or the maladies of plants to a soul, if we did not wish to run the risk of losing the conception of the term by this extensive generalization.

Toward the close of the past century we became more and more inclined to admit the existence of an organic force secondary to the soul—some called it vitality, others natural healing power. Those inclined to the former opinion endeavored to unite a given relation of the healthy organism with an effort directed upon itself. Those who adhered to the latter idea were firmly convinced that a peculiar regulating force existed.

At all events, the much sought for *unity* was driven further and further into the background by the sudden appearance of these new forces. There was no longer merely a *dyas*, but a *trias*. The disease, the remaining healthy portion of the body, and the particular force which ruled it. And no matter what special term was employed to designate the latter, it always partook of a distinctly spiritual character. Many attempts were made to reduce it to a scientific quality; to construct it according to a physical dynamic system; to interpret it as a particular form of electricity or magnetism.

However, as soon as the matter was entered upon seriously, and all the systematic plans and workings investigated, natural science became instantly transformed into a spirit.

Nevertheless, assistance was frequently deemed necessary. The course of the struggle was observed more minutely, and if it was found to be too weakly conducted either by vitality or natural healing force, endeavors were made to strengthen both, or at least to incite them to greater activity. But if the battle was found to be sustained with more force than necessity required, pains were taken to moderate and reduce the action. Thus arose a classification of conditions pertaining to disease—asthenic, sthenic and hypersthenic, names derived from *sthenos*, signifying strength.

It would lead us entirely too far from our course, should we attempt to expound the history of the various healing systems. It may suffice to say that every one of them, to use a common expression, has left its traces behind, and that an acute eye can easily detect them. According to our present ideas all these systems rest upon an erroneous conception of life and disease, inasmuch as

[6] *Archives of pathological anatomy*, 1849, Vol. II., p. 14.

they endeavor to attribute a more or less personal significance to each of these terms. The perception thus becomes figurative and typical.

Modern medical science has utterly renounced this tendency to personification, where the pre-supposed force does not correspond with an actual demonstrable body. It further separates simple forms from compound ones, although, according to the mode of observation they may possibly produce the impression of unity. For instance, the human organism appears to be a compound form, although we may correctly apply to it a personal expression. Each particular cell can be interpreted as a personality, for they are all self-existing and self-acting, and their power emanates from their own construction — their *physics*. In this sense the human body is not a unity in the strict *material* meaning of the word, but on the contrary a plurality, a collective form, and in a certain degree, a state. There likewise exists no one force which rules it and establishes its action, but on the contrary, a coöperation of many forces which are inseparable from the living element. Even the greatest phenomenon in human life, the spiritual I, is therefore no steady, immovable capacity, but a very changeable one.

If the human organic structure appears to us a unity it is chiefly due to three circumstances: First, in the construction of the vascular system and in the blood circulating through it, there is another perfectly accorded system which pervades the entire body, effects the material intercourse of the various substances, and constitutes a certain dependence of the parts upon the blood. For a long time, therefore, people looked for the source of life merely in the blood, and endeavored to explain all the incidents pertaining to disease and cure by means of the blood alone. When it appeared to be impure it was refined with inappropriate substances. When there was apparently too much or too little, it was drawn off, or attempts were made to produce it. In the second place, in the formation of the nervous system to which man's highest powers are attached, namely, the intellectual, we find an organization extending throughout the entire body, converging to the brain and spinal marrow, and which on one side is qualified to adopt outward impressions and conduct them to the great centre, while on the other side it possesses the capacity to eject any impulse directed upon other portions of the body by causing them to make particular assertions of activity or else to limit them.

Diseases such as fever, for instance, can only become intelligible by referring the great number of collected phenomena which come under this category, to the nervous system.[7] What wonder then that there is continually a fresh attempt to explain disease and cure by means of the nervous system?

But there is still a third point. This is the enormous mass of tissues of which the body is built up. The compound construction of countless numbers of cellular elements which are organized in the most varied manner, and are capable of producing the greatest diversity of results. Many of them, such as the muscles, appear in a high degree to be simple bearers of strength. The blood would be an immovable mass if the muscles of the heart and vessels did not circulate it mechanically. Other tissue formations, as the glands, superintend various things, the act of secretion, for instance, which represents a no less declaration of force. But each of these regulations, every one of these so-called organs is again a plurality compounded from endless elementary organisms, the cells. And when we see that the nervous system is just as complex, that the vessels, the heart and the blood are likewise compound combinations, it is well proved that every observation which does not apply to a compound element must be external and superficial.

If such a conception upon first sight results in a detachment of the body, a total breaking up of the perception, a further contemplation will show that these innumerable elements do not exist in juxtaposition. Accidentally or indifferently, they belong to each other on account of their common descent from a simple element which insures a certain original resemblance and relation among themselves just as there is among the descendents of one family.

This is the "divine necessity" of Hippocrates in its modern form. It does not merely assume the material of all elements to be one organism, but it also concludes that it must form certain combinations by means of which the effect of the different elements through each other produces a legitimate arrangement of the general principles.

Such organizations undoubtedly occur in the vascular and nervous systems, and they exist also in the great masses of superfluous tissues. For even as the vessels and nerves influence these latter, so on their side they influence them. Thus arises a reciprocity of effect which can be beneficial or otherwise, according to circumstances.

As long as the effect is beneficial, so long will the organization appear to be in harmony. And we can experience it in our consciousness as a sensation of well-being. If the effect should be injurious on the contrary, we say disease has entered the system, and we experience a feeling of discomfort. These sensations do not relate solely to bodily conditions, but to those of the mind, also. There is moral as well as physical indisposition.

In a figurative sense, we might say *equilibrium* instead of harmony, and *loss of balance* instead of discord. In many cases such designations would have an actual significance. The distribution of the blood is arranged to a certain extent, according to simple hydro-dynamic principles. An increase in one part necessitates a decrease in another. The electricity existing in the nerves can be interpreted in a purely physical sense. Here are tensions and accumulations, there evacuations and discharges of electricity. Even the usual incidents pertaining to the growth of the tissues provide us with numerous examples. If one part increases in strength, another diminishes. A suitable instance of these antagonistic phenomena is given in the difference of incidents pertaining to growth between the male and female sexes.

From these remarks we alresdy see that any disturbance of the harmony or equilibrium does not merely affect the common sensations, and therefore the nervous system, but also other parts of the body, and it can be readily understood that one disturbance will act upon this portion, and another upon that, etc.

All the parts do not stand in equal relation to each other, and those whose mutual dependence is the closest will, of course, be the soonest affected, while the others will be influenced in a lesser degree or else not at all. We designate the closer relationship as *sympathy*.

All these connections exist uniformly in sound, healthy bodies, and in order to explain them, we have no need to refer to the soul, vitality, or any other special spiritual force. When a diseased disturbance of the equilibrium occurs, they represent what we call *organic healing power*.

In order to obtain a full comprehension of this it is not actually necessary to say much concerning the healing itself. The theoretical discussions which have taken place in regard to this point, and the practical inferences derived from them, have often become very much confused inasmuch as entirely opposite relations have been drawn together by means of them.

The old word medicine, which is almost synonymous with our modern term therapeutics, led to the misunderstanding that the entire practical energy of the physician should be directed to one particular point of the bodily condition inasmuch as his chief task is to cure. A closer reflection will show, nevertheless, that this is by no means the case.

Only a certain portion of medical power, although it

[7] Virchow. *Fever. Four Discourses upon Life and Disease.* Berlin, 1862, p. 129.

may be the greater part, has reference to the curing of disease. Important branches of medicine allude to circumstances of sound health supervised by the physician in order to prevent disease. Every year our activity in this respect increases.

Besides the removal of the various causes of disease there is another cure which we designate as the *curatio causalis*. A foreign body such as a bullet, a glass splinter, etc., penetrates into the organism and remains there. Frequently, if not always, the removal of this body is the proviso of a cure. This of itself, however, is not sufficient, for the cohesion through which the foreign body passed must first be united, and the natural connection re-established, before the actual restoration can be acknowledged.

Very often restoration is spoken of when the case in question consists merely of a disturbance or a simple deficiency. If a person breaks his leg he is not ill. He cannot walk, of course, and an actual malady can proceed from the fracture if the surrounding parts become inflamed and the nerves excited. But the fracture itself is no illness, although it may become the cause of one. In spite of this, however, the sufferer always hopes to be "cured" by the physician.

Now it is unquestionably true that the same principle of observation cannot be applied to all such cases, otherwise we should become hopelessly embarassed. A broken knee will never set itself; therefore the physician is not to rely at all upon nature but simply upon his own skill; but he does not occupy himself with the phenomena by means of which the fracture will be re-united. That happens by itself. The medical influence in question is certainly technological. It is by means of force that the physician brings the pieces together in a position which as nearly as possible corresponds to the natural one. It is by means of force that he holds them thus. But all that is not a cure, but merely the stipulation for one. The broken part finally grows together in a very bad shape, and the re-establishment of the connecting portions occurs only with a very unfavorable position of the fracture. Nature in this case works most powerfully.

Every restoration of a broken bone is also physiological, and the physician only endeavors to let it occur undisturbed and under the most propitious circumstances. This "only" is of very great importance to the patient, for a fractured bone which heals crosswise or crookedly can infringe upon the use of a limb for life. But when we come to investigate all the theories of healing we must remain firm in stating that recovery from fracture is not caused by the physician. *The cause of the cure is due to the surrounding tissues.* They produce a new tissue, which forms over the scar.

We now come to *actual diseases*. They are not mere disturbances or yet definite conditions. An actual disease is an incident, also a succession of conditions, one preceding from the other and affecting vital parts. No lifeless object, no dead body ever becomes subject to disease. An animal or a plant can become diseased, but only while they are alive and only in such parts as are endowed with life. Therefore, every disease is a demolition to sound health, for the same part cannot at once be sick and well. Disease is also an incident pertaining to life. We call those incidents disease which deviate from the typical form of life and which are at the same time affected by the danger to which they are exposed, for disease strives towards death, be it local or general, and, consequently, it struggles against health.

If disease is incidental to life, it must be allied to certain living portions. Therefore we say the disease is "seated," and it is frequently one of the physician's most difficult tasks to discover precisely where this seat may be. But I must correct myself. In many cases the disease is located in several places. If a person has inflammation of the lungs, he usually has a violent fever in addition. In this case the inflammation is situated in the lungs and the fever in the centre of the nervous system —two entirely different places. Is all this one disease? Even at the beginning of the present century inflammation of the lungs was put under the category of fevers. Now it is considered as local inflammation. Still, it is the fever principally that is treated, while the inflammation is left to Nature. I will not enter into the fact that among many people who suffer from inflammation of the lungs, the stomach and kidneys also become diseased. What I have already said will suffice to show that the mere investigation made to discover the location of the disease leads us from the idea that it can be a unity. Unity only exists in so-called imaginary maladies. It is entirely figurative, a simple fancy, an abstract. In reality, most diseases are distinct pluralities, some existing in which the number of locations is countless.

It remains further to be said that in reference to diseases the word "cure" has many significations. If the term in plain language means wholeness without injury, it should designate the entire and complete re-establishment of the condition. Such an interpretation as this speaks badly for technology. If one has a tumor on the knee and the leg is amputated, curing denotes none the less a complete reestablishment. But it does not always agree with physiology either.

There is scarcely a single form of inflammation of the kidneys which admits of complete restoration; hardly one example of inflammation of the brain which does not always leave certain defects. These diseases therefore, are cured but imperfectly, and yet we may say the patients are quite restored because in spite of the deficiencies, new relations and connections take place in the body which cause the equilibrium of the actions performed.

As an example of the most perfect cure that we know of, I might mention inflammation of the lungs. Although it happens that in the course of a few days five, eight or even ten pounds of matter are deposited in the lungs through which the air inhaled should penetrate, we see, nevertheless, that again within a short time the entire mass is loosened and gradually disappears. This is the consequence of mere natural circumstances. But it requires only trivial aggravations, insignificant want of foresight, slight renewal of deteriorating causes, to interrupt this natural incident; then no relief can occur. On the contrary, the masses of matter remain firm like dead material; they break in pieces; the tissue surrounding them becomes impaired and thus the first step is taken toward that insidious occurrence called consumption. Therefore, the timely advice of a careful physican is very important even if he does not cure, and consequently no one should confidently imagine that all can be satisfactorily arranged independently of him.

Every incident of disease arises either from a defective nutrition or formation, or else from some disturbance of the local actions. A compound disease frequently includes all of these reasons at once. Defects of nutrition and formation are generally classed under the category of *organic imperfections*, because in both cases local alterations take place in the organism. For this reason the equalization of the disturbances occurs generally very slowly. The defects can only be removed gradually, and the normal condition established by degrees. Functional imperfections on the other hand can often be removed in a moment, because the inward construction does not change and the local action is altered merely by unusual excitation or oppression. The more the disease is confined to functional blemishes, the quicker it can be removed.

In any case whatsoever, the cure is obtained by complete restoration of the bodily harmony. It consists of a balancing and regulation of the disturbed relations, and indeed, an equalization through inward bodily resources. The healing powers are situated in the vital portions of the organism. These parts nourish themselves, and produce adequate conditions. They bring forth actions

which serve to direct, relieve, and repair certain defects of the equilibrium. Even when the physician's utmost power is exerted, when the part in question is cut off or destroyed, then also, restoration of the bodily equilibrium is necessary before any tolerable result can be produced. Also, when the healing powers remove certain imperfections, when an acid is neutralized by an alkali, or when a dormant faculty is roused into fresh activity by any excitation, the cure can only be perfect if the natural relations return again, or else if new ones are formed. Every outward effect is only a means by which to lead the inward formation of the body to free and regular action.

No physician can trust wholly to nature, but neither can he produce by art that which takes place naturally in the body. That is the work of the organic healing powers. Every medical man must rely upon their efficiency, but at the same time he has no right to sit idle with his hands in his lap in consequence. On the contrary it is frequently necessary to employ the most forcible interference in order to regulate the action properly. In particular diseases, how much nature is able to perform, and how much the physician is compelled to do, can only be ascertained by personal experience, and can be determined *a priori* by no theory. On the other hand, how far, in certain cases, medical treatment must extend, and how far the natural course is to be influenced by the physician, is not merely a question of experience, but frequently one of scientific value, which only an educated and cultured physician is capable of undertaking. Experience alone, in the medical world, produces only adventurers who perhaps may succeed now and then, but for whom self-reliance is always a risk. Such experience as is led and regulated by Science alone is capable of removing all barriers, and able to designate the realm in which nature and the physical organic forces have the supreme command.

Separation of Cadmium and Zinc.—In a memoir inserted in the *Annales de Chimie et de Physique* (Series 4, vol. 30, p. 351), M. Riche described a process for the determination of zinc, either by the decomposition of the acetate or by the electrolysis of the solution containing sulphuric acid. Several researches on the same subject have since been published by different authors. MM. Beilstein and Jawein, whilst confirming the results of Riche, employ the following process:—The nitric or sulphuric solution of zinc is mixed with caustic soda until precipitation ensues, and then with potassium cyanide till the precipitate is re-dissolved; the electrolysis is then effected with four Bunsen elements. The determination of cadmium has been effected by the same chemist under the same circumstances by means of the current from three elements. M. Millot has recently given a process for the determination of zinc by electrolysis of a solution of this metal in potassa. M. Edgar Smith obtains a precipitate of metallic cadmium by passing a strong current through a solution of the acetate. These procedures have the defect of not serving for the separation of cadmium and zinc, as the two metals are precipitated simultaneously. They may be separated as follows:—The solution containing the two metals in the state of acetates is mixed with 2 or 3 grms, sodium acetate, and a few drops of acetic acid. The current from two Daniell elements is then passed through the solution as described by M. Riche in his memoir. The cadmium alone is deposited in a crystalline layer at the negative pole, the zinc remaining in solution. The process requires the aid of heat, and requires three to four hours for quantities of 0·180 grm. to 0·210 grm. cadmium, and as much zinc. The deposit is effected in the crucible, and the liquid is then drawn off and serves for the determination of the zinc, according to M. Riche's process The deposit is washed first with water, then with alcohol, dried, and weighed. If the zinc and cadmium are present as sulphates the author recommends precisely the same method. Or the sulphuric solution may be mixed with ammonia and ammonium sulphate.—A. Yver.

MANUFACTURE OF YEAST WITHOUT ALCOHOLIC FERMENTATION.

A method of manufacturing yeast without alcoholic fermentation, and without the formation of subsidiary products has been patented in England by Dr. J. Rainer, of Vienna. The process is carried out in the following manner:—The vegetable albuminous substances in the corn cereals or other vegetables, or such refuse of industrial establishments as bran cornings, malt residuum, gluten, and the like, are extracted with the aid of from 15 to 20 parts by measure of water, made slightly alkaline. They are then either peptonized by adding an excess of lactic acid (about 4 per cent.) or mineral acids (about .25 per cent. of phosphoric acid, or about .4 per cent. of either sulphuric acid or hydrochloric acid) at a temperature of from 55 to 100 degrees Fahrenheit, or they are at once macerated in dilute solutions of the above acids and simultaneously converted into peptone. A portion of the albuminous substances (from 5 to 10 per cent. of the total weight) in the dried cornings will be already transformed into peptone by the process of vegetation. The albuminous substances in cereals, maize, or other vegetables, and in bran and malt residuum are transformed into peptone by the addition of diastase. In order to effect the conversion it is sufficient to add to one part by weight of the albuminous matter when dry, one part by weight of dry malt, or five parts by weight of cornings. As stated the liquid in which the albuminous matter is to be transformed into peptone must contain lactic acid (4 per cent.), phosphoric acid (as much as .25 per cent.), sulphuric acid or hydrochloric acid (about .4 per cent.), because the presence of an acid is absolutely necessary in the process of converting these substances into peptone.

A temperature of about 100 degrees Fahrenheit is the most suitable for the conversion of the substances into peptone, and a period of from 18 to 20 hours will be sufficient to effect it. It may, however, be also carried out at lower temperatures during a correspondingly longer time. In working cornings it is superfluous to add malt, because the diastase contained in the cornings is more than sufficient for the process of conversion into peptone. Therefore it is only necessary in this case to use one of the above-named acids in the proportions given. The slimy pectates contained in the cornings as well as in other materials are dissolved by the combination of diastase and acids. When the preparation of pure peptone is required the pectates may be separated by an endosmotic apparatus or dialysator, in such a manner that the peptone is dialysed through proper membranes in water, while the gelatinous pectates remain as a residuum. The acids are neutralized by means of soda, or by saturating the liquid with basic phosphate of lime. The prepared peptone liquid, with or without a percentage of sugar, may be shipped as a saleable article, or it may be delivered in a dry state, or as a syrup or extract obtained by boiling the liquid down in a water bath, by steam, or preferably in a vacuum. The liquid containing peptone may be separated from solid matter (hydrocarbons, vegetable fibre, or the like) by simple extraction, maceration, or pressure, or by centrifugal action, or it may be carefully cleaned by filtration or settling. It is advisable, however, before cleaning by filtration or settling to naturalize any acid present by means of soda, or to saturate the liquid with basic phosphate of lime, the latter being preferable because the phosphoric acid required by the yeast is thus abundantly furnished to it. In order to start the growth of yeast, gelatinized starch is added after being transformed in the usual way into dextrose by boiling with an addition of mineral acids. In the place of starch thus prepared an addition may be made of maltose, molasses, or sugar mixed with beer-yeast or compressed yeast. The amount thus added should correspond to the percentage of pep-

tone in the liquid, being one-half of the dry weight of the peptone. The hydrocarbons should, however, always be only from .5 to 1 per cent. of the weight of the entire liquid, and should even then serve exclusively for the formation of the walls of the cells of the yeast.

The vegetation of the yeast will take place most satisfactorily at temparatures varying from 57 to 64 degrees Fahrenheit. At a higher temperature losses may easily occur by reason of the partial conversion of the sugar used into coagulated acid or into alcoholic fermentation, instead of furnishing the yeast with substance for cells. The yeast is either propagated, as is the custom in Holland, in shallow vessels in which the depth of liquid is about five inches, so that a sufficient quantity of atmospheric air has access thereto; or it may be better and more safely effected in vats made of wood, glass, masonry, cement, or other suitable material, into which atmospheric air is conducted by suitable distributors through tubes or pipes by means of blowers or compressers.

Instead of atmospheric air alone it is more advantageous to use air containing an increased amount of ozone or of oxygen partially converted into ozone. The latter is prepared by successively adding hydrogen dioxide to the propagated liquid. The percentage of ozone in the air is increased by means of phosphorus, or by causing it to pass through a closed vessel in which permanganate of potassa is mixed with the necessary quantity of mineral acid. The air thus enriched with ozone is then allowed to pass into the propagating liquid.

The growth of the yeast will be completed within from 6 to 8 hours after every sufficient addition of dextrose, maltose, or other material, according to the density of the propagating liquid used, the temperature of the latter, and the amount of the ozone in the air. The percentage of peptone of the mass may amount to from 1 to 2 per cent. or more of its weight, while only from one-half to one per cent. of dextrose or other hydrocarbons is added at each time, in order to be sure to prevent the formation or coagulated lactic acid or alcoholic fermentation.

When the entire amount or bulk of the dextrose or other sugar added to promote the growth of the yeast has been consumed after from six to eight hours, a further quantity thereof, say, from .05 to .10 per cent. is added. The peptone may also, after having been consumed, be added in portions, or may be allowed to flow in gradually and continuously. The same propagating liquid made by successive replacement of the matter consumed remains in use for weeks or months, unless it is rendered impure by other substances, or by subsiding fermentation is made unfit for further use. In the same manner as the materials necessary for the propogation of the yeast are added the yeast produced may be successively withdrawn, and only the yeast suspended in the liquid remains behind as the germ for the ferments of alcohol to be afterwards formed. The yeast is obtained either by skimming it from the surface of the liquid or by separating it from the propagating liquid by filtration, or finally by gathering it after tapping the vats from the bottom upon which it is deposited in a compact layer. In working on a large scale it is advisable to place the vats in terraced batteries in order to effect the transfer of the propagating liquid from one vessel to the other with facility. In order to produce yeast as free as possible from subsidiary ferments the propagating liquid may be prepared in a more dilute state, that is to say, with a percentage of peptone of only from .75 to 1 per cent. The hydrocarbons (dextrose, maltose, or the like) may also be added in smaller quantities, for example, as a first dose about .33 per cent. and then every 3 hours about .05 per cent.

The greater part of the peptone present will then be transformed into yeast in from 12 to 15 hours, a sufficient supply of pure air, if necessary, conducted through sulphuric acid or oxygen containing ozone, being provided, and the entire process being carried on at a temperature varying from 54 to 63 degrees Fahrenheit. The whole liquid is then cooled by a suitable apparatus, or by adding cold water or ice; the best temperature being from 45 to 50 degrees Fahrenheit. Within from 36 to 48 hours the yeast obtained will settle on the bottom of the vat. The propagating liquid may be allowed to flow away. The yeast obtained by this improved process is purified and condensed in the usual manner, but in order to increase its durability phosphate of lime amounting to from 4 to 5 per cent. of the total weight of the yeast to be made may be added before compressing it.

Experience has shown that from 250 to 300 parts of pure and active compressed yeast may be obtained from 100 parts of pure peptone. For the growth of that quantity of yeast only about 200 parts of dextrose or sugar are required.

MICROSCOPY.

We have received the February issue of the Journal of the Royal Microscopical Society, now edited by Mr. Frank Crisp, one of the secretaries of the society. It contains a valuable and interesting original paper, with two full-page illustrations, and the proceedings of the R. M. C. A summary is also presented of current research in those departments of science, depending upon the use of the microscope for their advancement. The amount of information thus gathered may be estimated from the fact that the present number is a volume of one hundred and seventy-two pages. The Journal appears bi-monthly, and costs one dollar (4*s*.) for each part.

The President of the Royal Microscopical Society announced that a fund had been provided for the presentation of two gold medals annually, without regard to nationality—one for the person who should originate any important improvement in the microscope, or any of its accessory apparatus, or in any other way eminently contribute to the advancement of the microscope as an instrument of research. The second gold medal was to be awarded "in respect to any researches in any subject of natural science carried on wholly, or in a great part, by means of the microscope, or of the recipient having in other ways eminently contributed to the advancement of research in natural science in connection with the microscope."

The two medals were to be known respectively as the "Microscopical" and "Research" medals of the Society. For reasons which are not stated, the offer of this fund was declined by the Council of the Society.

The war of Apertures of Microscope Objectives has again broken out in the R. M. S. In this instance Mr. Shadbolt was the aggressor, who claimed that his paper demonstrated beyond dispute the following facts, viz.:

"That a dry lens can have as large an 'angular aperture' as an immersion one, and that the assumed difference of aperture between dry and immersion lens does exist."

"That no lens can have an 'aperture' of any kind which exceeds that of 180° angular in air."

"That, consequently, the table of 'numerical apertures' published on the cover of the *Journal* of the Society is erroneous and misleading, and should at once be discontinued."

In reply, Mr. Crisp asserted that Mr. Shadbolt was in error, and the victim to a misplaced confidence in a fundamental fallacy, viz., "the supposition that equal angles in different media, as air and oil, are optically equivalent."

A correspondent, who is an authority on this subject, will offer an opinion on this matter. We believe, however, that Mr. Crisp is correct in his views, and that the society has exercised a wise discretion in putting a stop to a discussion, which had become wearisome and unprofitable.

Mr. Crisp showed how a few moss-grown English microscopists had persistently refused to countenance

the use of immersion objectives, which are now in universal use, and accepted as a valuable improvement.

The use of oil was suggested by Amici, as far back as 1844, by Oberhauser in 1845, and Wenham in 1855 and again in 1870, and only admitted in practice in 1878, so that it appears to have required 34 years to convince microscopists of a fact, that might have been settled in a week and this due to "persistence in a fallacy." Such being the case it is surely time for these fallacies to be shelved, and we are glad to find the R. M. S. has taken such a view of the case.

FLUORESCENT BODIES.

If we put some common paraffin oil, or a solution of sulphate of quinine, into a glass tube or other suitable vessel, and then look through it, the liquid will appear quite colorless; but if we allow the light to fall upon it, and then view it at a little distance and at a certain angle, some parts of the liquid will present a delicate sky-blue tinge. The effect in the case of quinine is heightened if the source of light is burning magnesium wire.

The large number of substances belonging to this class are termed fluorescent bodies, because they exhibit phenomena similar to the examples above given. The term itself, however, was suggested to Prof. Stokes by a particular kind of fluor-spar which shows this property.

Again, if we cause a room to be darkened, and allow only blue light (*i. e.*, by covering a hole in a window-shutter with cobalt-blue glass) to fall upon a glass vessel filled with water which has been standing some minutes, on floating a strip of horse-chestnut bark upon its surface, in a few moments a stream of bluish grey fluid (æsculin) will be seen slowly descending from the bark, hanging, in fact, like a bunch of barnacles from an old ocean waif. Of if, under the same arrangement of light, or by using even more powerful absorbents of the ordinary rays (such as a solution of ammonio-sulphate of copper or one of chromate of potash), we look at a piece of what is commonly termed canary glass—*i. e.*, glass colored with an oxide of the metal uranium—it will be seen to glow as it were with rich greenish yellow rays, just as though it were itself a source of light; or if we take a solution of a uranium salt (the normal acetate) the phenomena are very striking when examined under the same conditions, and still more so by the electric light. But the salts of aniline—a substance which is the parent, so to speak of mauve, magenta, and other brilliant colors—are singularly rich in exhibiting these effects.

A very beautiful experiment may be performed with the aniline red ink now so commonly in use. It affords, at one and the same time, an admirable illustration of Prof. Tomlinson's submersion figures and of the phenomena under consideration. If we take a long cylindrical glass vessel, or one with parallel sides, fill it with water, which is allowed to settle, and then gently deliver a drop of the red fluid to the surface, the drop begins to contract, and slowly from its centre descends in the form of a tube; the denser parts of the coloring-matter presently form a thick circular rim at the end of the tube,—but this is only for a moment, for a wavy edge appears upon this rim, then expands into a triangular parachute with a thickened edge, and from the extremity of each corner two or three smaller tubes descend; these in like manner pass through the same phases as the parent stem or tube.–*E. R. Hodges (Journal of Science, London.)*

INTRA-MERCURIAL PLANETS.

A collection of the observations published in the report of the Total Solar Eclipse of 1878, will give, perhaps, the best idea of the present state of the question of the discovery of Vulcan and other planets revolving within the orbit of Mercury; and it may be of some interest to present the matter in the form of a chart showing the ground covered by different observers, who, during the time of totality, devoted themselves to the search for such bodies. For this purpose, the space swept by the six observers, Newcomb, Hall, Wheeler, Bowman, Todd and Pritchett, has been indicated by different shading on the accompanying chart, which is merely a copy of that prepared by Prof. Hall for the use of observers of the eclipse, and published with the instructions issued from the United States Naval Observatory.

The two objects, "*a*" and "*b*," discovered by Prof. Watson, and thought by him to be planets, have been indicated upon the map thus : ⨀. The two discovered by Swift, also announced as intra-mercurial planets, have been marked thus : ⊗.

Swift's two stars are described as equal in brightness, of about the fifth magnitude, and 8′ apart; on a line with the sun's centre. Each had a round red disk, and each was free from twinkling. The object farther from the sun was at one time thought by Swift to be ϑ Cancri, and the other a new planet. The diameter of the field of view was 1.°5.

Watson's star, "*a*," is described as being "between the sun and ϑ Cancri, and a little to the south;" of a ruddy color and about 4th magnitude, or fully a magnitude brighter than ϑ Cancri, which was seen at the same time. The star, "*b*," was also of a ruddy hue, and is given as the 3rd magnitude.

Watson used an aperture of 4 inches; magnifying power of 45 diameters; Swift, an aperture of 4.5 inches; power of 25 diameters. We see by inspecting the chart, that the place of one of Watson's stars (that of which he was the more certain) was covered by Wheeler with a 5-inch aperture; power 100; by Pritchett, 3.5 inch aperture, power 90; and by Bowman with a 3.5 inch aperture and power of 30 diameters. The place of Swift's two stars was examined by Bowman and Wheeler, and one of the stars appears just in the corner of Pritchett's sweep. Finally, the whole ground was covered by Todd with a 4-inch aperture and power of 20.

Of these observers, Wheeler and Pritchett possessed telescopes with optical power at least equal to that of Swift, or Watson, and Bowman's glass was of sufficient power to show any object as large as the 5th magnitude,—but nothing, not already upon the chart, was found.

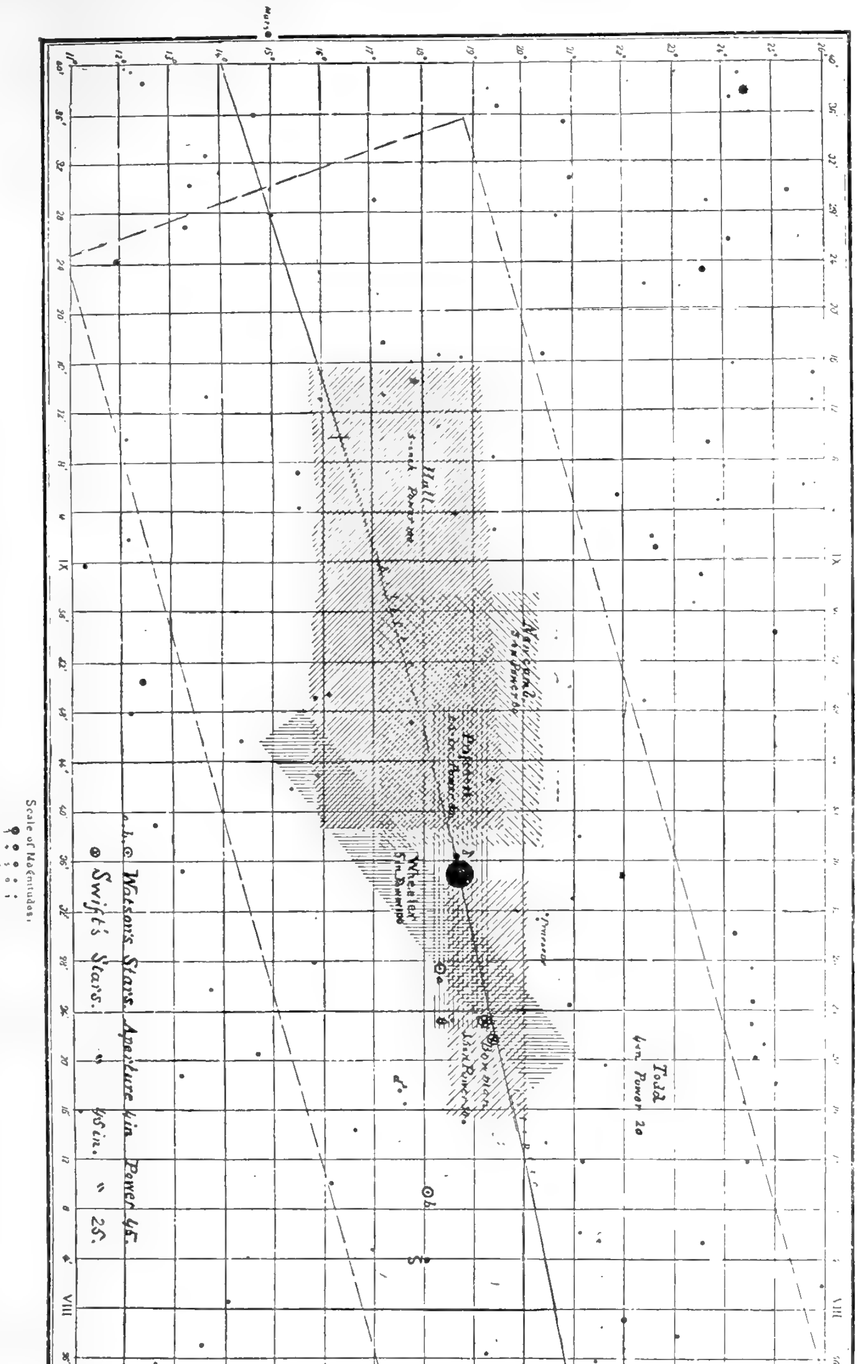
This should be borne in mind, however, that several of the observers were enabled to make but very hasty sweeps,—not devoting so much of their attention to the subject as Watson did, and, indeed, at Mr. Todd's station clouds interfered seriously with the work. And, on the other hand, it appears that Prof. Watson devoted a large part of his time to sweeping on the east side of the sun.

A glance at the chart will show that Watson's stars have about the same relative positions and magnitudes as ϑ and ζ Cancri, and that Swift's stars as far as relative position is concerned, resemble closely d^2 Cancri and B. A. C. 2810, or the pair of stars similarly placed on the other side of the sun. The probability of an error in pointing the telescope, which would account for such a misidentification as has been suggested, has been thoroughly discussed by Dr. C. H. F. Peters in the *Astron. Nach.*, No. 2253, p. 323, and Dr. Peters' paper has been answered by Prof. Watson in the next volume, *Astron. Nach.*, No. 2263, p. 101.

It is not the intention of this article to consider again the question of the identity of the stars seen by Watson and Swift, but merely to point out the evidence upon which the discovery of "Vulcan" rests, and to call attention to the fact that the existence of an intra-mercurial planet is not yet admitted by the majority of astronomers of the present day.

W. C. W.

WASHINGTON, D. C., *February* 24, 1881.



Planets and Stars in the Vicinity of the Sun, on July 29th, 1878.

TO ILLUSTRATE ARTICLE ON INTRA-MERCURIAL PLANETS.

BOOKS RECEIVED.

A MANUAL OF ZOOLOGY for the use of students, with a general introduction on the principles of Zoology—by HENRY ALLEYNE NICHOLSON, M. D., D. Sc., Ph. D., etc., Professor of Natural History in the University of St. Andrews. Sixth edition, revised and enlarged,. William Blackwood and Sons—Edinburgh and London, 1880.

This Manual of Zoology has become so fully recognized as one of the most complete and reliable guides to a knowledge of this subject, that but few words are necessary in giving notice of the issue of a new edition.

The study of Zoology is constantly bringing new and interesting facts to the surface, hence the necessity for frequent editions of manuals treating on the subject, to keep pace with discoveries. Professor Nicholson has availed himself of the present opportunity to thoroughly revise his work, and bring forward arrears of facts which have accumulated during the past two years, and in accordance with the views of many distinguished naturalists he has raised the order of ***Echinodermata*** to the rank of a sub-kingdom. This alteration necessitates the abandonment of the ***Annuloida*** as a sub-kingdom, and the reference of the Scolecida to the Annulosa.

Professor Nicholson forestalls criticism for such action by candidly admitting that this arrangement is far from being wholly satisfactory, but asks that it may be provisionally adopted as the best under the circumstances, taking into account our present knowledge.

A number of excellent illustrations have been introduced in the present edition, and the student will now have the benefit of over 450 wood-cuts.

The general plan of this book is admirable, and following each chapter is a list of the best works and memoirs relating to the animals belonging to each sub-kingdom.

There is one feature of this work which in our opinion gives it a special value to students, and that is an excellent glossary of about 1000 words. The index is also ample and carefully arranged.

The present work of Professor Nicholson is the latest and best Manual of Zoology, and we recomend it strongly to those interested in such studies.

LIFE ON THE SEASHORE, OR ANIMALS OF OUR COASTS AND BAYS, with illustrations and descriptions. By JAMES H. EMERTON, author of Structure and Habits of Spiders. Naturalists' Handy Series No. 1. George A. Bates, Salem, Mass., 1880.

This charming little work is the first of a series of handy books suitable for amateur naturalists, a class now happily on the increase.

The author has provided a pleasant companion which should be in the hands of all visitors to our coasts, ensuring a never failing fund of amusement, leading insensibly to one of the most delightful of scientific studies.

Mr. Emerton states "I have tried to give such explanations of some of our common animals of the New England coast as have been often asked for by persons little acquainted with zoology, and to give such directions about collecting and observing them as have been found useful to students who come to the shore for a short time in the summer to study animals that they before knew only from pictures."

The book is divided into four parts, treating separately animals which are found "between the tides," "near low water mark," "surface animals," "bottom animals." The reader will find this an excellent arrangement. We find above one hundred and fifty excellent wood cuts, which faithfully represent the objects described in the body of the book; the sensational and misleading illustrations to be found in a somewhat similar work find no place in this volume. We can therefore recommend Mr. Emerton's work as not only a reliable guide, but one which will create a healthful desire for knowledge in those who are so fortunate as to possess it.

CHEMICAL NOTES.

CONTRIBUTION TO A KNOWLEDGE OF SAPONIFICATION OF FATS.—The name fat is generally applied to a mixture of the tri-glycerides of palmitic, stearic, and oleic acids. As regards the animal fats this assumption has been in all cases verified, but the vegetable fats display certain not un important deviations. J. König, J. Kiesow, and B. Aronheim, in saponifying vegetable fats, obtained invariably less glycerine than is required for forming the glycerine-ethers of the fatty acids—a fact pointing to the conclusion that free fatty acids must be present, since the quantity of cholesterine occurring in the plants is too small to combine with the fatty acids. For saponification, potassium and sodium hydrate were used along with the other basic oxides, the latter substances being considered equal in value to the former, the only difference being that the products in the one case are termed "soaps," and in the other "plasters." It was assumed hitherto that the tri-glycerides, like other ethers, were completely decomposed by the above named ethers into salts of the fatty acids and glycerine, and that equal quantities of glycerine were obtained in all cases. For the saponification of fats and the separation of the products, J. König had proposed a process which consists essentially in treating the fat operated upon with an excess of lead oxide in presence of water at 90° to 100°. Dr. von der Becke, when attempting at his request to saponify cacao-butter in this manner—in order to discover a process for detecting the sophistications of this product—found that it could not be saponified with lead oxide, at least not in this manner. It was found on further experimentation that the quantity of glycerine obtained on saponification with potassium hydrate was in all cases considerably the highest. In the easily saponifiable fats, butter, lard, and olive oil, the difference was found less manifest, but it was much more distinct in those which are hard to saponify. Cacao-butter and tallow, if saponified with lead oxide, yield scarcely traces of glycerine. A mixture of an easily saponifiable fat like butter with cacao-butter gave the same quantity of glycerine as if butter alone were employed. It is possible that the reaction when once set up may extend itself. Hence it appears that in the case of some fats the method of saponification with oxide is not trustworthy, and that when the accurate determination of the proportion of glycerine in a fat is required, the saponification must be effected with potassium hydrate.

CONTRIBUTIONS TO THE CHARACTERISTICS OF THE ALKALINE EARTHS AND OF ZINC OXIDE.—The alkaline earths and zinc oxide if their hydrates, carbonates, and nitrates are heated to complete decomposition, are obtained in the following specific gravities. Lime is obtained amorphous from the hydrate and carbonate, but in regular cubic crystals from the nitrate; in either case of the sp. gr. 3.25. Strontia is obtained from the hydrate and carbonate amorphous, and of sp. gr. 4.5, but from the nitrate in regular crystals and of sp. gr. 4.75. Baryta is obtained from the hydrate in optically one- or two-axial crystals, of sp. gr. 5.32: but from the nitrate in regularly cubic crystals of sp. gr. 5.72. Magnesia is always obtained in the amorphous form of sp. gr. 3.42. Zinc oxide is obtained amorphous from the hydrate and carbonate of sp. gr. 3.47, but from the nitrate in hexagonal pyramids of sp. gr. 5.78.

Prof. Pritchett, of the Morrison Observatory, Glasgow, Mo., has made arrangements to drop a Time-Ball at Kansas City.

DETERMINATION OF SILICON IN IRON AND STEEL.—One grm. iron or steel is placed in a porcelain crucible with 25 c.c. nitric acid of 1.2 sp. gr. When the reaction is over 25 to 30 c.c. dilute sulphuric acid—1 part acid and 3 water are added, and the solution is heated till the nitric acid is entirely or nearly expelled. When the residue is sufficiently cool water is cautiously added, and the contents of the capsule are heated till the crystals are perfectly dissolved. The solution is then filtered as hot as possible, and the residue washed first with hot water, then with 25 to 30 c.c. hydrochloric acid of sp. gr. 1.20, and finally again with hot water. After drying and ignition the silica is obtained snow-white and granular.—T. M. BROWN.

SCIENCE:

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MICROSCOPES AND THEIR OBJECTIVES.

WE are told by one maker of microscopes that he has orders in advance which will prevent his undertaking new work for at least four months from the present time. Supposing his statement to be true, and we heartily trust it is, it would appear to show that the number of those undertaking microscopical investigations is largely on the increase, and as the probabilities are that many of those now investing their money in microscopes and objectives, are doing so with little experience to guide them in their selection, it may be useful at this moment to take a review of the microscope market.

The purchase of the microscope stand and the objectives to use with it will be considered separately. They are usually purchased together, but there is no reason for doing so, and we would like to see each handled by a distinct branch of trade. To make a good microscope stand needs only the skill of a good worker in brass, under suitable direction. On the other hand, the manufacture of objectives, and the other optical parts of a microscope, requires the skilled labor of an optician.

In regard to the microscope stand, we would state that many improvements have been recently made, so that to avoid being saddled with one which may be considered obsolete, it would be as well to go directly to one who manufacturers his own stands, and direct him to make one to order; by so doing, the additional advantage will be secured of obtaining an instrument specially suited for particular work—a very important point.

The temptation is great to name one or two microscope stands which, in our opinion, are perfect in workmanship, designed on the best model and, withal, quite moderate in price; but to do so would court misinterpretation of our motives; so we may state that such firms as Bausch & Lomb, Beck, Bullock, Grunow, Schrauer, Slidel, Zentmayer, are all reliable American manufacturers, and that most of these firms now produce such an instrument as we would advise, at a cost of about 40 to 50 dollars for a Monocular stand, not including accessory apparatus or objectives. We have just seen an instrument, for the latter price, having perfection of workmanship and the latest improvements.

In regard to microscope objectives, the greatest caution should be employed by the inexperienced at this moment, for after twenty-five years experience in purchasing objectives, the present price-lists of opticians appear to us a perfect chaos of quotations.

In the first place, the objection has been raised by purchasers that object glasses of a certain focal length and stated aperture, vary in their linear magnifying power, among different makers, so that a quarter-inch which, for instance, should give 200 diameters with an A eye-piece, is found to be a 4–10, allowing only 120 diameters if purchased of another maker, or perhaps it will give 225 diameters similar to a 1–5th, when obtained from a third manufacturer—even when the conditions are alike. This, no doubt, originated in one of the tricks of the trade. A makes a 1–4th which, in resolving power, equals the 1-6th of B; in consequence, A claims at once a superiority of workmanship, and perhaps secures a reputation for objectives, when, if the truth was known, the 1-4th was in fact a 1-6th.

It must be remembered also that all objectives vary in quality even from the same maker, and that one may be given to an inexperienced person which is very far from the supposed standard of excellence; with some makers not more than one in twelve would be accepted by an expert.

Lastly, there appears to be a feeling that considerable improvements are imminent in the manufacture of objectives, rendering those of yesterday commercially valueless. If we may judge by a price list just forwarded, a panic appears to have commenced among those holding objectives made as recently as four years ago. By a circular, we are informed that the objectives of one of the most esteemed makers are now offered at prices 50 per cent. lower than those charged by the maker. "These lenses are of the *best quality* and *perfectly new*," "simply to close out our stock of these objectives." This offer is made by an optician in the same city with the original maker. Objectives which cost $150 can be had for $75, and others as follows: $110 for $55, $50 for $30. $40 for

$20, and one very noted objective for which the maker asks $60 is offered for $27.20.

We also notice the production of a 1–10th objective of 180° aperture, by a maker of reputation, which is sold at $25. A subscriber recently called at our office and stated that a 1-6th by the same maker, also sold at $25, divided the 19th band of Nobert's plate.

We mention these facts to show the variations in the present cost of microscope objectives, which must be perplexing to inexperienced purchasers. The regular price of a first-class 1–10th, of 180°, is about $85, and it would be interesting to compare the $25 article and note results; and it would also be useful to note how the cheaper glasses perform their work, as compared one with another.

As any expression of opinion on the merits of these objectives would be useless, without they were personally tested by us, we refrain from offering any advice on the subject. Microscope objectives are not struck in a die like medals, but are the result of manual operation, in which the individuality of the artist may be recognized and developed. In art the relative merits of the master are appreciated by the connoisseur, and a standard of value established; the same rule applies to optical instruments when perfection of work is aimed at. When Professor Asaph Hall discovered the satellites of Mars, it was necessary to have a telescope which would show an object six miles in diameter at a distance of 35,000,000 miles; when called upon to perform this feat, Clark's 32-inch objective responded in a manner which enabled Professor Hall to make one of the most important of recent astronomical discoveries. To appreciate this performance of the Washington telescope, we may state that it was equivalent to a person stationed at New York seeing an object at Boston which was two inches in diameter.

Such is the class of work we desire to find in microscopic objectives; probably there are only one or two men in this country able to produce it; but it is difficult to speculate as to what the future may bring forth.

WALKER PRIZES IN NATURAL HISTORY.

The Boston Society of Natural History offers a first frize of $60 to $100, and a second of $50, for the best memoirs, in English, on the following subjects: For 1881, The Evidences of the Extension of Tertiary Deposits seaward along the cost of Massachusetts; for 1882, The Occurrence, Microscopic Structure, and use of North American Fibre-plants (treating especially of the fibres employed by the native races); for 1883, Original Unpublished Investigations respecting the Life-History of any Animal. Prizes will not be awarded unless the papers are deemed of adequate merit.

THE ODONTORNITHES.

EXTINCT TOOTHED BIRDS OF NORTH AMERICA.

We merely desire in this place to acknowledge the receipt of the monograph, on the *Odontornithes*, an extinct order of toothed birds of North America, prepared by Professor O. C. Marsh, and published by order of the United States Government.

A review of this work is now in course of preparation by one well able to present Professor Marsh's discoveries in all their integrity, and we propose to publish the same with illustrations, which will convey to the readers of "SCIENCE" a fair estimate of the value of this work, which is considered by many to be one of the most important contributions to science, issued by the National Government at Washington.

Reserving our review of Professor Marsh's monograph for a future occasion, we now offer his own explanation regarding the work, as conveyed in a few introductory remarks:

"The remains of birds are among the rarest of fossils, and very few have been discovered except in the more recent formation. According to present evidence, the oldest known birds were imbedded in the Jurassic deposits of Europe, which have yielded three individuals belonging to the genus *Archæopteryx*, so well preserved that the more important characters can be determined. The only other remains of birds found in the Mesozoic of the Old World are a few specimens from the Cretaceous of England, which are too fragmentary to throw much light on the extinct forms they represent.

"The earliest traces of birds hitherto found in the strata of this country are from the Cretaceous, although we may confidently predict their discovery in the Jurassic beds, if not at a still lower horizon. There is at present no evidence whatever that any of the three-toed impressions in the Triassic, described as the foot prints of birds, were made by birds; and the proof now seems conclusive that nearly all of them are the tracks of Dinosaurian reptiles, bones of which occur in the same deposits.

"In the Cretaceous beds of the Atlantic coast, and especially in the green-sand region of New Jersey, various remains of birds have been found and described by the writer. These fossils, although often in excellent preservation, occur mainly as isolated bones, and hence their near affinities have not as yet been determined with certainty.

"Along the western slope of the Rocky Mountains, and especially on the adjoining plains in Kansas and Colorado, there is a series of Cretaceous strata remarkably rich in vertebrate fossils. The deposits are all marine, and, away from the mountains, they lie nearly horizontal. They have suffered much from erosion, and are still wasting away, especially along the river valleys. These beds consist mainly of a fine yellow chalk and calcareous shale, both admirably adapted to preserve delicate specimens, and here have been found the extinct birds which form the subject of the present memoir.

"The geological horizon of the known *Odontornithes* is in the Middle Cretaceous and corresponds to the strata named by the writer the "Pteranodon beds." The latter are included in sub-division number three, in Meek and Hayden's section. The accompanying fossils are Mosasauroid reptiles, which are very abundant; Plesiosaurs allied to Pliosaurus; Pterodactyles of the genus *Pteranodon;* and many fishes. With these occur Rudistes, and occasionally Ammonites, Belemnites, and various other Cretaceous invertebrates.

"The first bird fossil discovered in this region was the lower end of the tibia of *Hesperornis*, found by the writer in December, 1870, near the Smoky Hill River in Western Kansas. Specimens belonging to another genus of the *Odontornithes* were discovered on the same expedition. The extreme cold, and danger from hostile Indians, rendered a careful exploration at that time impossible.

"In June of the following year, the writer again visited the same region, with a larger party, and a stronger escort of United States troops, and was rewarded by the discovery of the skeleton which forms the type of *Hesperornis regalis*, Marsh. Various other remains of *Odontornithes* were secured, and have since been described by the writer. Although the fossils obtained during two months of explorations were important, the results of this trip did not equal our expectations, owing in part to the extreme heat (110° to 120° Fahrenheit, in the shade) which, causing sun stroke and fever, weakened and discouraged guides and explorers alike.

"A considerable part of these Cretaceous deposits still remain unexplored, and in the Autumn of 1872, a third expedition through this territory was undertaken by the writer with a small party. Additional specimens of much interest were secured, including the type of the genus *Apatornis*, and one nearly complete skeleton of *Hesperornis*—an ample reward for the hardship and danger we incurred.

"The specimens thus secured by these various expeditions have since been supplemented by important additions, collected in the same general region by different parties equipped and sent out by the writer, who no longer could give his personal supervision to work in that fiield. The fossil birds procured in this region, since 1870, by these different expeditions, include remains of more than one hundred different individuals of the *Odontornithes*. These are all in the Museum of Yale College, and form the material on which the present volume is based.

"A study of this extensive series of bird remains brings to light the existence, in this class, of two widely-separated types, which lived together during the Cretaceous period, in the same region, and yet differed more from each other than do any two recent birds. Both of these types possessed teeth, a character hitherto unknown in the class of birds, and hence they have been placed by the writer in a separate sub-class, the *Odontornithes*. One of these groups includes very large swimming birds, without wings and with the teeth in grooves (*Odontolcæ*), and is represented by the genus *Hesperornis*. The other contains small birds, endowed with great powers of flight, and having teeth in sockets (*Odontotormæ*), and biconcave vertebræ; a type best illustrated by the genus *Ichthyornis*. Other characters, scarcely less important, appear in each group, and we have thus a vivid picture of two primitive forms of bird structure, as unexpected as they are suggestive. A comparison of these two forms with each other, and with some recent birds, promises to clear away many difficulties in the genealogy of this class, now a closed type; and hence they are well worthy of the detailed description and full illustration here devoted to them.

"The fossil birds now known from the Cretaceous deposits of this country are included in nine genera and twenty species. These have all been described by the writer, and are represented, at present, by the remains of about one hundred and fifty different individuals. This is evidence of a rich and varied avian fauna in America during Mesozoic time, and likewise indicates what we may expect from future discoveries.

"The present volume is the first of a series of Monographs designed to make known to science the extinct vertebrate life of North America. In the investigation of this subject, the writer has spent the past ten years, much of it in the field, collecting, with no little hardship and danger, the material for study, and the rest in working out the characters and affinities of the ancient forms of life thus discovered.

"During this decade, the field work extending from the Missouri River to the Pacific Coast has so predominated as the subject unfolded, that a plan of gradual publication became a necessity. The more important discoveries were briefly announced soon after they were made, but only where the specimens on which they were based admitted of accurate determination. The principal characters of the new groups were next worked out systematically, and published with figures of the more important parts. When the investigation of a group is completed, the results, with full descriptions and illustrations, will be brought together in a monograph. This system has been carried out with the *Odontornithes*, and will be continued with the other groups. The investigation of several of these is now nearly completed, and the result will soon be ready for publication.

"The material is abundant for a series of monographs on the marvelous extinct vertebrates of this country, and the results already attained are full of promise for the future. A somewhat careful estimate makes the number of new species of extinct vertebrates, collected since 1868, and now in the Yale College Museum, about 1000. Nearly 300 of these have already been described by the writer, and some have been noticed or described by other authors, but at least one-half remain to be investigated.

"Among the new groups brought to light by these researches, and already made known by descriptions of their principal characters, are the following, which will be fully described in subsequent volumes of the present series.

"The first Pterodactyles or flying reptiles discovered in this country, were found by the writer in the same geological horizon with the *Odontornithes* described in the present memoir. These were of enormous size, some having a spread of wings of nearly twenty-five feet; but they were especially remarkable on account of having no teeth, and hence resembling recent birds. They form a new order, *Pteranodontia*, from the type genus *Pteranodon*. Of this group, remains of more than six hundred individuals are now in the Yale College Museum—ample material to illustrate every important point in their osteology.

"With these fossils were found also great numbers of Mosasauroid reptiles, a group which, although rare in Europe, attained an enormous development in this country, both in numbers and variety of forms. Remains of more than fourteen hundred individuals belonging to this order were secured during the explorations of the last ten years, and are now in the Museum of Yale College.

"The most interesting discoveries made in the Jurassic formation were the gigantic reptiles belonging to the new sub-order *Sauropoda*, including by far the largest land animals yet discovered. Another remarkable group of large reptiles found in the same formation were the *Stegosauria*. Other Dinosaurs from the same horizon, the 'Atlantosaurus beds,' show that this was the dominant form of vertebrate life in that age, and many hundred specimens of these reptiles are now in the Yale Museum. In a lower horizon of the same formation, the 'Sauranodon beds,' were found the remains of a peculiar new group of reptiles, the *Sauranodontia*, allied to *Ichthyosaurus*, but without teeth.

"In the Eocene deposits of the Rocky Mountains, the writer discovered a new order of huge mammals, the *Dinocerata*. Remains of several hundred individuals were secured, and a monograph on the group will follow the present memoir. In the same formation were found the remains of another new order of mammals, the *Tillodontia*, in many respects the most remarkable of any yet discovered. In the same Eocene deposits were secured the first remains of the fossil *Primates* known from North America as well as the first *Cheiroptera* and *Marsupialia*. Abundant material also was found in the same region to illustrate the genealogy of the Horse, and a memoir on this subject is in course of preparation.

CHOLESTEN.—This compound, $C_{26}H_{42}$, is a white amorphous powder, almost insoluble in alcohol, but soluble in ether. It resembles *c.* cholesterin in its physical and chemical properties.—W. E. Walitzky.

THE UNITY OF NATURE.

By the Duke of Argyll.

VI.

ON THE MORAL CHARACTER OF MAN, CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

The consciousness of unworthiness in respect to moral character is a fact as fundamental, and as universal in the human mind as the consciousness of limitation in respect to intellectual power. Both of them may exist in a form so rudimentary as to be hardly recognizable. The limits of our intelligence may be felt only in a dim sense of unsatisfied curiosity. The faultiness of our character may be recognized only in the vaguest emotions of occasional self-reproach. But as the knowledge of mankind extends, and as the cultivation of their moral faculties improves, both these great elements of consciousness become more and more prominent, and occupy a larger and larger place in the horizon of their thoughts. It is always the men who know most who feel most how limited their knowledge is. And so likewise it is always the loftiest spirits who are most conscious of the infirmities which beset them.

But although these two great facts in human consciousness are parallel facts, there is a profound difference between them; and to the nature and bearing of this difference very careful attention must be paid.

We have seen in regard to all living things what the relation is between the physical powers which they possess and the ability which they have to use them. It is a relation of close and perfect correspondence. Everything requisite to be done for the unfolding and upholding of their life they have impulses universally disposing them to do, and faculties fully enabling them to accomplish. We have seen that in the case of some animals this correspondence is already perfect from the infancy of the creature, and that even in the case of those which are born comparatively helpless, there is always given to them just so much of impulse and of power as is requisite for the attainment of their own maturity. It may be nothing more than the mere impulse and power of opening the mouth for food, as in the case of the chicks of many birds; or it may be the much more active impulse and the much more complicated power by which the young mammalia seek and secure their nourishment; or it may be such wonderful special instincts as that by which the newly hatched Cuckoo, although blind and otherwise helpless, is yet enabled to expel its rivals from the nest, and thus secure that undivided supply of food without which it could not survive. But whatever the impulse or the power may be, it is always just enough for the work which is to be done. We have seen, too, that the amount of prevision which is involved in those instinctive dispositions and actions of animals is often greatest in those which are low in the scale of life, so that the results for which they work, and which they do actually attain, must be completely out of sight to them. In the wonderful metamorphoses of insect life, the imperfect creature is guided with certainty to the choice and enjoyment of the conditions which are necessary to its own development; and when the time comes it selects the position, and constructs the cell, in which its mysterious transformations are accomplished.

All this is in conformity with an absolute and universal law in virtue of which there is established a perfect unity between these three things:—first, the physical powers and structure of all living creatures; secondly, those dispositions and instinctive appetites which are seated in that structure to impel and guide its powers; and thirdly, the external conditions in which the creature's life is passed, and in which its faculties find an appropriate field of exercise.

If Man has any place in the unity of Nature, this law must prevail with him. There must be the same correspondence between his powers and the instincts which incite and direct him in their use. Accordingly, it is in this law that we find the explanation and the meaning of his sense of ignorance. For without a sense of ignorance there could be no desire of knowledge, and without his desire of knowledge Man would not be Man. His whole place in Nature depends upon it. His curiosity, and his wonder, and his admiration, and his awe—these are all but the adjuncts and subsidiary allies of that supreme affection which incites him to inquire and know. Nor is this desire capable of being resolved into his tendency to seek for an increased command over the comforts and conveniences of life. It is wholly independent of that kind of value which consists in the physical utility of things. The application of knowledge comes after the acquisition of it, and is not the only, or even the most powerful, inducement to its pursuit. The real incitement is an innate appetite of the mind—conscious in various degrees of the mystery, and of the beauty, and of the majesty of the system in which it lives and moves; conscious, too, that its own relations to that system are but dimly seen and very imperfectly understood. In a former chapter we have seen that this appetite of knowledge is never satisfied, even by the highest and most successful exertion of those faculties which are, nevertheless, our only instruments of research. We have seen, too, what is the meaning and significance of that great Reserve of Power which must exist within us, seeing that it remains unexhausted and inexhaustible by the proudest successes of discovery. In this sense it is literally true that the eye is not satisfied with seeing, nor the ear filled with hearing. Every new advance has its new horizon. Every answered question brings into view another question unanswered, and perhaps unanswerable, lying close behind it. And so we come to see that this sense of ignorance is not only part of our nature, but one of its highest parts—necessary to its development, and indicative of those unknown and indefinite prospects of attainment which are at once the glory and the burden of humanity.

It is impossible to mistake, then, the place which is occupied among the unities of Nature by that sense of ignorance which is universal among men. It belongs to the number of those primary mental conditions which impel all living things to do that which it is their special work to do and in the doing of which the highest law of their being is fulfilled. In the case of the lower animals, this law, as to the part they have to play and the ends they have to serve in the economy of the world, is simple, definite, and always perfectly attained. No advance is with them possible, no capacity of improvement, no dormant or undeveloped powers leading up to wider and wider spheres of action. With Man, on the contrary, the law of his being is a law which demands progress, which endows him with faculties enabling him to make it, and fills him with aspirations which cause him to desire it. Among the lowest savages there is some curiosity and some sense of wonder, else even the rude inventions they have achieved would never have been made, and their degraded superstitions would not have kept their hold. Man's sense of ignorance is the greatest of his gifts, for it is the secret of his wish to know. The whole structure and the whole furniture of his mind is adapted to this condition. The highest law of his being is to advance in wisdom and knowledge: and his sense of the presence and of the power of things which he can only partially understand is an abiding witness of this law, and an abiding incentive to its fulfillment.

In all these aspects there is an absolute contrast between our sense of limitation in respect to intellectual power (or knowledge) and our sense of unworthiness in respect to moral character. It is not of ignorance, but of knowledge, that we are conscious here,—even the knowledge of the distinction between good and evil, and of that special sense which in our nature is associated with it, namely, the sense of moral obligation. Now it is a uni-

versal fact of consciousness as regards ourselves, and of observation in regard to others, that, knowing evil to be evil, men are nevertheless prone to do it, and that having this sense of moral obligation, they are nevertheless prone to disobey it. This fact is entirely independent of the particular standard by which men in different stages of society have judged certain things to be good and other things to be evil. It is entirely independent of the infinite variety of rules according to which they recognize the doing of particular acts, and the abstention from other acts, to be obligatory upon them. Under every variety of circumstance in regard to these rules, under every diversity of custom, of law, or of religion by which they are established, the general fact remains the same—that what men themselves recognize as duty they continually disobey, and what according to their own standard they acknowledge to be wrong they continually do.

There is unquestionably much difficulty in finding any place for this fact among the unities of Nature. It falls, therefore, in the way of this inquiry to investigate how this difficulty arises, and wherein it consists.

And here we at once encounter those old fundamental questions on the nature, the origin, and the authority of the Moral Sense which have exercised the human mind for more than two thousand years; and on which an eminent writer of our own time has said that no sensible progress has been made. This result may well suggest that the direction which inquiry has taken is a direction in which progress is impossible. If men will try to analyze something which is incapable of analysis, a perpetual consciousness of abortive effort will be their only and their inevitable reward.

For just as in the physical world there are bodies or substances which are (to us) elementary, so in the spiritual world there are perceptions, feelings, or emotions, which are equally elementary—that is to say, which resist all attempts to resolve them into a combination of other and similar affections of the mind. And of this kind is the idea, or the conception, or the sentiment of obligation. That which we mean when we say, "I ought," is a meaning which is incapable of reduction. It is a meaning which enters as an element into many other conceptions, and into the import of many other forms of expression, but it is itself uncompounded. All attempts to explain it do one or other of these two things—either they assume and include the idea of obligation in the very circumlocutions by which they profess to explain its origin; or else they build up a structure which, when completed, remains as destitute of the idea of obligation as the separate materials of which it is composed. In the one case, they first put in the gold, and then they think that by some alchemy they have made it; in the other case, they do not indeed first put in the gold, but neither in the end do they ever get it. No combinations of other things will give the idea of obligation, unless with and among these things there is some concealed or unconscious admission of itself. But in this, as in other cases with which we have already dealt, the ambiguities of language afford an easy means or an abundant source of self-deception. One common phrase is enough to serve the purpose—the "association of ideas." Under this vague and indefinite form of words all mental operations and all mental affections may be classed. Consequently those which are elementary may be included, without being expressly named. This is one way of putting in the gold and then of pretending to find it as a result. Take one of the simplest cases in which the idea of obligation arises, even in the rudest minds—namely, the case of gratitude to those who have done us good. Beyond all question, this simple form of the sense of obligation is one which involves the association of many ideas. It involves the idea of Self as a moral agent and the recipient of good. It involves the idea of other human beings as likewise moral agents, and as related to us by a common nature, as well as, perhaps, by still more special ties. It involves the idea of things good for them, and of our having power to confer these things upon them. All these ideas are "associated" in the sense of gratitude towards those who have conferred upon us any kind of favor. But the mere word "association" throws no light whatever upon the nature of the connection. "Association" means nothing but grouping or contiguity of any kind. It may be the grouping of mere accident—the associations of things which happen to lie together, but which have no other likeness, relation, or connection. But this, obviously, is not the kind of association which connects together the different ideas which are involved in the conception of gratitude to those who have done us good. What then is the associating tie? What is the link which binds them together, and constitutes the particular kind or principle of association? It is the sense of obligation. The associating or grouping power lies in this sense. It is the centre round which the other perceptions aggregate. It is the seat of that force which holds them together, which keeps them in a definite and fixed relation, and gives its mental character to the combination as a whole.

If we examine closely the language of those who have attempted to analyze the Moral Sense, or, in other words, the sense of obligation, we shall always detect the same fallacy—namely, the use of words so vague that under cover of them the idea of obligation is assumed as the explanation of itself. Sometimes this fallacy is so transparent in the very forms of expression which are used, that we wonder how men of even ordinary intelligence, far more men of the highest intellectual power, can have failed to see and feel the confusion of their thoughts. Thus, for example, we find Mr. Grote expressing himself as follows: "This idea of the judgment of others upon our conduct and feeling as agents, or the idea of our own judgment as spectators in concurrence with others upon our own conduct as agents, is the main basis of what is properly called Ethical sentiment."[1] In this passage the word "judgment" can only mean moral judgment, which is an exercise of the Moral Sense; and this exercise is gravely represented as the "basis" of itself.

Two things, however, ought to be carefully considered and remembered in respect to this elementary character of the Moral Sense. The first is, that we must clearly define to ourselves what the idea is of which, and of which alone, we can affirm that it is elementary; and secondly, that we must define ourselves as clearly, if it be possible to do so, in what sense it is that any faculty whatever of the mind can really be contemplated as separable from, or as uncombined with, others.

As regards the first of these two things to be defined, namely, the idea which we affirm to be simple or elementary, it must be clearly understood that this elementary character, this incapability of being reduced by analysis, belongs to the bare sense or feeling of obligation, and not at all, or not generally, to the processes of thought by which that feeling may be guided in its exercise. The distinction is immense and obvious. The sense of rightness and of wrongness is one thing; the way in which we come to attach the idea of right or wrong to the doing of certain acts, or to the abstention from certain other acts, is another and a very different thing. This is a distinction which applies equally to many other simple or elementary affections of the mind. The liking or disliking of certain tastes or affections of the palate is universal and elementary. But the particular tastes which are the objects of liking or of aversion are for the most part determined by habits and education. There may be tastes which all men are so coustituted as necessarily to feel disgusting; and in like manner there may be certain acts which all men everywhere must feel to be con-

[1] "Fragments on Ethical Subjects," pp. 9, 10.

trary to their sense of obligation. Indeed we shall see good reason to believe that this not only may bé so, but must be so. But this is a separate subject of inquiry. The distinction in principle is manifest between the sense itself and the laws by which its particular applications are determined.

The second of the two things to be defined—namely, the sense in which any faculty whatever of the mind can really be regarded singly, or as uncombined with others —is a matter so important that we must stop to consider it with greater care.

The analogy is not complete, but only partial, between the analysis of Mind and the analysis of Matter. In the analysis of Matter we reach elements which can be wholly separated from each other, so that each of them can exist and can be handled by itself. In the analysis of Mind we are dealing with one organic whole; and the operation by which we break it up into separate faculties or powers is an operation purely ideal, since there is not one of these faculties which can exist alone, or which can exert its special functions without the help of others. When we speak, therefore, of a Moral Sense or of Conscience, we do not speak of it as a separate entity any more than when we speak of Reason or of Imagination. Strictly speaking, no faculty of the mind is elementary in the same sense in which the elements of Matter are (supposed to be) absolutely simple or uncombined, Perhaps there is no faculty of the mind which presents itself so distinctly and is so easily separable from others as the faculty of Memory. And yet Memory cannot always reproduce its treasures without an effort of the Will, nor, sometimes , without many artificicial expedients of Reason to help it in retracing the old familiar lines. Neither is there any faculty more absolutely necessary than Memory to the to the working of every other. Without Memory there could not be any Reason, nor any Reflection nor any Conscience. In this respect all the higher faculties of the human mind are much more inseparably blended and united in their operation than those lower faculties which are connected with bodily sensation. These lower faculties are indeed also parts of one whole, are connected with a common centre, and can all be paralyzed when that centre is affected. But in their ordinary activities their spheres of action seem widely different, and each of them can be, and often is, seen in apparently solitary and independent action. Sight and taste and touch and hearing are very different from each other—so separate indeed that the language of the one can hardly be translated into the language of the other. But when from these lower faculties, which are connected with separate and visible organs of the body, and which we possess in common with the brutes, we ascend to the great central group of higher and more spiritual faculties which are peculiar to Man, we soon find that their unity is more absolute, and their interdependence more visibly complete. Ideally we can distinguish them, and we can range them in an ascending order. We can separate between different elements and different processes of thought and in accordance with these distinctions we can assign to each of them a separate faculty of the mind. We think of these separate faculties as being each specially apprehensive of one kind of idea, or specially conducting one kind of operation. Thus the reasoning faculty works out the process of logical sequence, and apprehends one truth as the necessary consequence of another. Thus the faculty of Reflection passes in review the previous apprehensions of the Intellect, or the fleeting suggestions of Memory and of Desire, looks at them in different aspects, and submits them now to the tests of reasoning, and now to the appreciations of the Moral Sense. Thus, again, the supreme faculty of Will determines the subject of investigation, or the direction of thought, or the course of conduct. But although all these faculties may be, and indeed must sometimes be, conceived and regarded as separate, they all more or less involve each other; and in the great hierarchy of powers, the highest and noblest seem always to be built upon the foundations of those which stand below. Memory is the indispensable servant of them all. Reflection is ever turning the mind inward on itself. The logical faculty is ever rushing to its own conclusions as necessary consequences of the elementary axioms from which it starts,and which are to it the objects of direct and intuitive apprehension. The Moral Sense is ever passing its judgments upon the conduct of others and of ourselves; whilst the Will is ever present to set each and all to their proper work. And the proper work of every faculty is to see some special kind of relation or some special quality in things which other faculties have not been formed to see. But although these qualities in things are in themselves separate and distinct, it does not at all follow that the separate organs of the mind, by which they are severally apprehended, can ever work without each other's help. The sense of logical necessity is clearly different from the sense of moral obligation. But yet as Reason cannot work without the help of Memory, so neither can the Moral Sense work without the help of Reason. And the elements which Reason has to work on in presenting different actions to the judgment of the Moral Sense may be, and often are, of very great variety. It is these elements, many and various in their character, and contributed through the help and concurrence of many different faculties of the mind that men are really distinguishing and dissecting when they think they are analyzing the Moral Sense itself. What they do analyze with more or less success is not the Moral Sense, but the conditions under which that Sense comes to attach its special judgments of approval or of condemnation to particular acts or to particular motives.

And this analysis of the conditions under which the Moral Sense performs its work, although it is not the kind of analysis which it often pretends to be, is nevertheless in the highest degree important, for although the sense of obligation, or, as it is usually called, the Moral Sense, may be in itself simple, elementary, and incapable of reduction, it is quite possible to reach conclusions of the most vital interest concerning its nature and its functions by examining the circumstances which do actually determine its exercise, especially those circumstances which are necessary and universal facts in the experience of mankind.

There is, in the first place, one question respecting the Moral Sense which meets us at the threshold of every inquiry respecting it, and to which a clear and definite answer can be given. This question is—What is the subject-matter of the Moral Sense? or, in other worda, what is the kind of thing of which alone it takes any cognizance, and in which alone it recognizes the qualities of right and wrong?

To this fundamental question one answer, and one answer only can be given. The things, and the only things of which the Moral Sense takes cognizance are the actions of men. It can take no cognizance of the actions of machines, nor of the actions of the inanimate forces of Nature, nor of the actions of beasts, except in so far as a few of these may be supposed to possess in a low and elementary degree some of the characteristic powers of Man. Human conduct is the only subject-matter in respect of which the perceptions of the Moral Sense arise. They are perceptions of the mind which have no relation to anything whatever except to the activities of another mind constituted like itself. For, as no moral judgment can be formed, and no moral perception can be felt, except by a moral agent, so neither can it be formed in respect to the conduct of any other agent which has not, or is assumed not to have a nature like our own—moral, rational and free.

And this last condition of freedom, which is an essential one to the very idea of an agency having any moral character, will carry us a long way on toward a farther definition of the subject-matter on which the Moral Sense is exercised. It is as we have seen, human conduct. But it

is not human conduct in its mere outward manifestations, for the only moral element in human conduct is its actuating motive. If any human action is determined not by any motive whatever, but simply by external or physical compulsion, then no moral element is present at all, and no perception of the Moral Sense can arise respecting it. Freedom, therefore, in the sense of exemption from such compulsion, must be assumed as a condition of human action absolutely essential to its possessing any moral character whatever. There can be no moral character in any action, so far as the individual actor is concerned, apart from the meaning and intention of the actor. The very same deed may be good, or, on the contrary devilishly bad, according to the inspiring motive of him who does it. The giving of a cup of cold water to assuage suffering, and the giving it to prolong life in order that greater suffering may be endured, are the same outward deeds, but are exactly opposite in moral character. In like manner, the killing of a man in battle and the killing of a man for robbery or revenge, are the same actions; but the one may be often right, whilst the other must be always wrong, because of the different motives which incite the deed. Illustrations of the same general truth might be given as infinite in variety as the varying circumstances and conditions of human conduct. It is a truth perfectly consistent with the doctrine of an Independent Morality, Every action of a voluntary agent has, and must have, its own moral character, and yet this character may be separate and apart from its relation to the responsibility of the individual man who does it. That is to say, every act must be either permitted, or forbidden, or enjoined, by legitimate authority, although the man who does it may be ignorant of the authority or of its commands. And the same proposition holds good if we look upon the ultimate standard of morality from the Utilitarian point of view. Every act must have its own relation to the future. Every act must be either innocent, or beneficent, or hurtful in its ultimate tendencies and results. Or, if we like to put it in another form, every act must be according to the harmony of Nature or at variance with that harmony, and therefore an element of disorder and disturbance. In all these senses, therefore, we speak, and we are right in speaking of actions as in themselves good or bad, because we so speak of them according to our own knowledge of the relation in which they stand to those great standards of morality, which are fact and not mere assumptions or even mere beliefs. But we are quite able to separate this judgment of the act from the judgment which can justly be applied to the individual agent. As regards him, the act is right or wrong, not according to our knowledge, but according to his own. And this great distinction is universally recognized in the language and (however unconsciously) in the thoughts of men. It is sanctioned, moreover, by Supreme Authority. The most solemn prayer ever uttered upon earth was a prayer for the forgiveness of an act of the most enormous wickedness, and the ground of the petition was specially declared to be that those who committed it "knew not what they did." The same principle which avails to diminish blame, avails also to diminish or extinguish merit. We may justly say of many actions that they are good in themselves, assuming, as we naturally do, that those who do such actions do them under the influence of the appropriate motive. But if this assumption fails in any particular case, we cannot and we do not, credit the actor with the goodness of his deed. If he has done a thing which in itself is good in order to compass an evil end, then, so far as he is concerned, the deed is not good, but bad, It may indeed be worse in moral character than many other kinds of evil deeds, and this just because of the goodness usually attaching to it. For this goodness may very probably involve the double guilt of some special treachery, or some special hypocrisy; and both treachery and hypocrisy are in the highest degree immoral. It is clear that no action, however apparently benevolent, if done from some selfish or cruel motive, can be a good or a moral action.

It may seem, however, as if the converse of this proposition cannot be laid down as broadly and as decidedly. There are deeds of cruelty in abundance which have been done, ostensibly at least, and sometimes, perhaps, really from motives comparatively good, and yet from which an enlightened Moral Sense can never detach the character of wickedness and wrong. These may seem to be cases in which the motive does not determine the moral character of the action, and in which our Moral Sense persists in condemning the thing done in spite of the motive. But if we examine closely the grounds on which we pass judgment in such cases, we hall not, I think, find them exceptions to the rule or law that the purpose or intention of a free and voluntary agent is the only thing in which any moral goodness can exist, or to which any moral judgment can be applied. In the first place, we may justly think that the actors in such deeds are to a large extent themselves responsible for the failure in knowledge, and for the defective Moral Sense which blinds them to the evil of their conduct, and which leads them to a wrong application of some motive which may in itself be good. And in the second place, we may have a just misgiving as to the singleness and purity of the alleged purpose which is good. We know that the motives of men are so various and so mixed, that they are not always themselves conscious of that motive which really prevails, and we may have often good reasons for our convictions that bad motives unavowed have really determined conduct for which good motives only have been alleged. Thus, in the case of religious persecution, we may be sure that the lust of power and the passion of resentment against those who resist its ungovernable desires, have very often been the impelling motive, where nothing but the love of truth has been acknowledged. And this at least may be said, that in the universal judgment of mankind, actions which they regard as wrong have not the whole of that wrongfulness charged against the doers of them, in proportion as we really believe the agents to have been guided purely and honestly by their own sense of moral obligation.

On the whole, then, we can determine or define with great clearness and precision the field within which the Moral Sense can alone find the possibilities of exercise—and that field is the conduct of men;—by which is meant not their actions only, but the purpose, motive, or intention by which the doing of these actions is determined. This conclusion, resting on the firm ground of observation and experience, is truthfully expressed in the well-known lines of Burns:—

> "The heart's aye the part aye
> Which makes us right or wrang."

And now it is possible to approach more closely to the great central question of all ethical inquiry:—Are there any motives which all men under all circumstances recognize as good? Are there any other motives which, on the contrary, all men under all circumstances recognize as evil? Are there any fundamental perceptions of the Moral Sense upon which the standard of right and wrong is planted at the first, and round which it gathers to itself, by the help of every faculty through which the mind can work, higher and higher conceptions of the course of duty?

(To be continued.)

Physiological Effects of Glycerin.—Chemically pure glycerine if injected under the skin of dogs proves fatal within twenty-four hours if the dose reaches 8 to 10 grms. per kilo. of the weight of the animal. The symptoms are comparable to those of acute alcoholism.—M M. Beaumetz and Audigé.

ATMOSPHERIC OZONE FOR JANUARY, 1881.

By L. P. Gratacap.

The memorable discovery of Ozone by Schönbein, in 1840, bequeathed to the scientific world one of its questiones vexatæ, about which opinions and experiments seem to have been equally at variance. As regards its constitution and essential nature there seems little reason to doubt it is a condensed form of oxygen, according to the views of Andrews and Tait, and that it displays the characteristic properties of that gas in an intensified degree. Its existence in the air can be hardly less questioned, but the extent and origin of its presence are involved in obscurity, and partly from the modifying influence of local circumstances and the identity of its reactions with other atmospheric bodies the conclusions of various experimenters are either equivocal or contradictory. The fact that chlorine, sulphurous fumes, the nitrogen oxides, affect the test papers in the same manner as ozone, and that humidity of the atmosphere, strong winds, bright sunshine, or local nuisances exaggerate or diminish the normal reaction, renders it difficult to eliminate the error introduced by their adventitious influence. The results here given were obtained with test papers of starch and iodide of calcium, prepared, presumably, like those of Dr. Moffat, from starch and iodide of potassium, and compared, after the test, with a scale of colors similar to Negretti and Zambra's.

After E. Schöne's recent condemnation of ozone tests made in this way, they may appear valueless, but it would hardly seem, admitting the justice of Schöne's strictures, that their comparative showing would be seriously impaired. The coloration obtained was in a great measure due to ozone, and its increase or decrease was due in the same proportion to an increase or decrease of this re-agent; the contemporaneous influence of nitrogen oxides may have deepened the tints, it certainly could not have neutralized them, and inasmuch as the papers were kept moist the effects of the varying humidity of the air were, in a measure, cancelled. Precautions against the disturbing influence of winds and that of strong sunshine were also taken. Duplicate observations were taken at 10 feet and at 40 feet from the ground, and their average (though in nearly all cases they proved identical one with the others) recorded, as the color-mark of the hours they were exposed.

Observations were taken every 12 hours, dividing the 24 between day and night, and notes kept of the weather. As a rule, the papers exposed at an elevation were more deeply colored than those near the ground, though this was probably due to a freer circulation of air. The papers at the periods of strongest ozonization were changed throughout; at other times they were marked in spots and near the edges, showing an unequal sensitiveness to the re-agents. In supplementary trials on the effect of the wind, it was found that those papers exposed to the wind were sometimes one-third deeper in tint than the protected ones, and reached their maximum much quicker. These contrasts were, of course, lessened with a diminished velocity of wind.

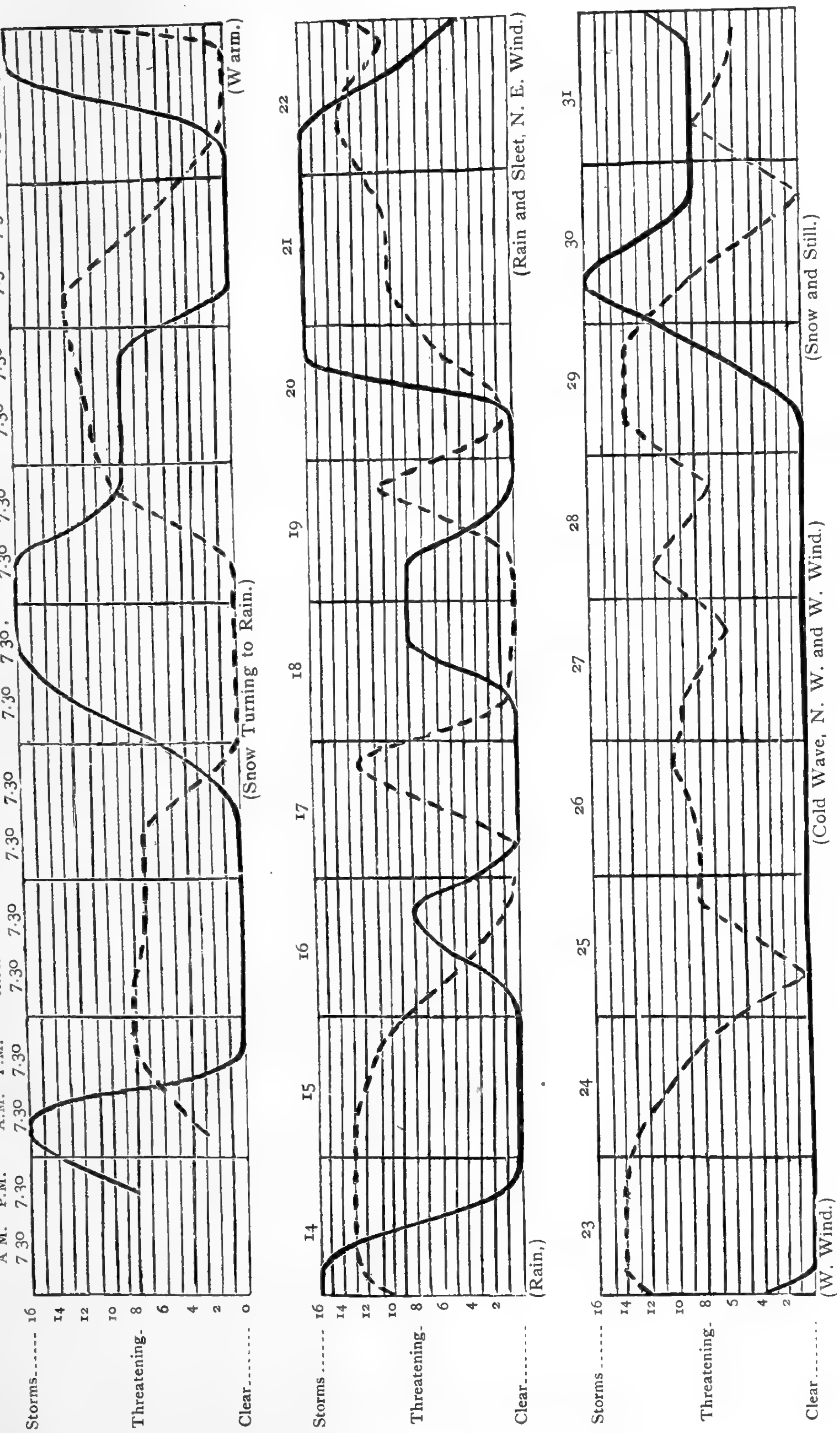
The manifestations of ozone followed, as a rule, lowered barometric pressure and rising temperature, in other words, they were coincident with change of weather. This is an interesting confirmation of Houzeau's experiments, and in the attempt I make below to give this a graphic demonstration this generalization appears, *i. e.*, that a wave of ozonization follows the storm wave, lagging somewhat behind it, and appreciably corresponding in duration and intensity to the force and continuance of the air wave which preceded it.* In this connection it will be noticed that threatening weather on the 16th and 18th was followed by a sudden projection of the ozone curve which as rapidly subsides, indicating either atmospheric disturbances responsive to an incipient but unfinished change of weather, or else undulations of ozonization coming from some neighboring storm centre or both. Further on the curve of ozonization rises somewhat before that of the weather, and I apprehend this may often or always happen when storms of unusual severity and violence are about to traverse a district. The thrill of ozonization recorded on the papers taken in on the morning of the 20th were prophetic of the fierce and extraordinary tempest which devastated New York and its vicinity upon the 21st.

The high readings from the 25th to 29th accompanied the advent of a cold wave in the Hudson River Valley on the night of the 24th, which sent the mercury down to 15° below 0° at Poughkeepsie and brought colder weather to New York and its vicinity, lasting four days, with strong N.-W. and W. winds. This appears analogous to the strong ozonization concurrent with storms, etc.; the atmospheric disturbance originating the cold wave propagated an ozone wave which appears simultaneously with the former. It is not probably due simply to an apparent increase of normal ozone from the rapid passage of air currents past the tester. This latter effect is doubtless efficient in heightening the entire result, but the wind appears to act as an ozone carrier, bringing into one area supplies of this gas formed in a different and removed one. Indeed it does not appear unwise to speculate upon the possibility of the wind acting as an ozone generator since the irruption of a volume of air at a high velocity of different temperature and density from that of the points over which its path sweeps, must comprise electrical changes, discharges and perturbations. Such effects would correspond in their intensity with the violence and character of the air blast, and we might find the neighboring areas to the track of a cyclone strongly ozonized. As a matter of observation the strong ozonization on the 29th succeeded the strong winds which ushered in the cold of the 27th and 28th. And in any case the deeper tints during wind indicate justly enough the increased prevalence of ozone in the areas swept over by the gale. That wind is not always efficient in changing the ozone papers was shown in Daremberg's experience at Mentone, where, although variations were caused by the wind, in some instances along the sea board the coloration did not at all respond to the strength of the former, and Houzeau is of the opinion that dry winds have slight influence upon the papers.

The cold wave was followed on the 30th by a still snow storm, the shower of pellets falling through an atmosphere unmoved by even a current of air. Threatening weather succeeded the cessation of the snowfall only to usher in the fierce storm of February 1st, when snow, wind, and a low temperature united to arrest life and motion upon the thoroughfares of land and water. The ozone curve responds but feebly to these meterological perturbations until February 1st, when it slowly rises, recalling Houzeau's conjectures as to storms which generated ozone and storms which did not.

It may seem superfluous, if not trivial, to record any observations upon atmospheric ozone when the whole subject is involved in a fog of scientific confusion, contempt and obloquy. It may be said that these observations presented no inconsistent, aberrant or contradictory results, and that to the general student of our local metereology they may in this graphic form exhibit some features of interest. The chart is simply suggestive and absolutely artificial; the numbers on the left of the lines indicate degrees of coloration and the weather line is determined by three points: clear, threatening and stormy. The readings were made at West Brighton, Staten Island, in New York Harbor, the maxima of colorations, and hence ozone, considered as coincident with the time at which the reading was taken, 7.30, night and morning, which must be at times barely approximative.

* As regards the sensible effects of the ozone following by many hours the opening of the storm on the 9th and 13th, the reactions appeared concurrently with a change in the weather from snow to rain. On the other hand, the storm of the 21st opened with rain.



OZONE AND WEATHER CURVE, JANUARY, 1881, STATEN ISLAND, N. Y.

——— WEATHER CURVE. - - - - - OZONE CURVE.

NOTE ON THE "POSTERIOR BRAIN" OF STEGOSAURUS.

In a paper before the National Academy of Sciences in November last, and more recently in an illustrated article in the *American Journal of Science*, February, 1881, Professor O. C. Marsh has described certain remarkable peculiarities of *Stegosaurus ungulatus,* one of the Dinosaurian Reptiles of the American Jurassic formation.

Judging from the figures, which are said to be reduced to one-sixteenth of the natural size, the arm of this species must have been about one meter in length, while the leg was about twice as long. "The great disproportion in size between the fore and hind limbs, as well as the structure of the principal joints in each, show plainly that *Stegosaurus* walked mainly as a biped. The massive posterior limbs, and the huge tail doubtless formed a tripod on which the animal rested at times, while the fore limbs were used for prehension or defense. The heavy dermal plates and powerful spines probably rendered the latter an easy task."

After recalling the statement which had been made by him in a previous article, that, proportionately, "this reptile had the smallest brain of any known land vertebrate," Professor Marsh describes with some detail "a very large chamber in the sacrum, formed by an enlargement of the spinal canal. This chamber was ovate in form, and strongly resembled the brain case in the skull, although very much larger, being at least ten times its size. . . . A perceptible swelling in the spinal cord of various recent animals has been observed in the pectoral and pelvic regions, where the nerves are given off for the anterior and posterior limbs; and in extinct forms some very noticeable cases are recorded, especially in Dinosaurs. . . . In some allied forms, *Camptonotus* for example, where the disproportion between the fore and hind limbs is nearly as marked, the sacral enlargement of the spinal cord is not one-fourth as great as in *Stegosaurus.* It is an interesting fact that in young individuals of *Stegosaurus* the sacral cavity is proportionately larger than in adults, which corresponds to a well-known law of brain growth. The physiological effects of a posterior nervous centre, so many times larger than the brain itself is a suggestive subject, which need not be here discussed. It is evident, however, that in an animal so endowed, the posterior part was dominant."

In the hope that Professor Marsh may continue his important observations and reflections upon this subject, attention is called to the following points:

1. It seems to be taken for granted that "the posterior nervous centre" was as large as the sacral cavity. This is hardly warranted, although it is certainly favored by the size of the sacral foramina. The cranial part of the elephant's skull is far larger than is required for the lodgment of its brain, on account of the surface needed for the attachment of the immense cervical muscles. With many fishes, especially some skates and the *Lophius,* the brain occupies but a small part of the capacious cranial cavity. May it not be, then, that the sacral cavity of *Stegosaurus* was enlarged, in part at least, in correlation with a general enlargement of the whole pelvis, in reference to the functions of the legs?

2. Unless such examinations have been made already, it would be well to ascertain the condition of the myelon in the kangaroos, and that of the sacral cavity in *Diprotodon* and *Megatherium,* all of which may be compared with *Stegosaurus* in respect to the size of the legs, or their employment in connection with a large tail.

It would be interesting to know the form and size of the entire myelonal canal of *Stegosaurus.* The paper leaves us in doubt as to whether the writer considers the "posterior nervous centre" as the homologue of the ordinary "lumbar enlargement" of the myelon in man and other vertebrates. If not, may it be the not yet wholly abbreviated representative of what the late Prof. Jeffries Wyman referred to in his paper "On Symmetry and Homology in Limbs"? In some adult fishes the spinal marrow ends in a ganglionic enlargement forming a kind of caudal brain. We have found such a ganglion quite conspicuous in the American *Lophius.*" In either case, it is probable that the remarkable condition of things in *Stegosaurus,* as described by Prof. Marsh, would have appeared to Prof. Wyman as an example of the law of organic polarity in the form of "fore-and hind symmetry," which has been advocated by him, by Dr. Coues, and by the writer of the present notice.

B. G. W.

THE ROMANCE OF ASTRONOMY.—Those interested in astronomy will be pleased to know that Professor R. Kalley Miller's delightful work "The Romance of Astronomy," has been published by the "Humbolt Library." Professor Miller is a professor at Cambridge, England, and has received aid from his distinguished colleagues, Sir William Thomson and Professor Tait. An appendix by Mr. R. A. Proctor is added. We remind our readers that the above work can be purchased for 15 cents, and that under the advantages now offered by club rates (to subscribers only), the 24 numbers issued annually can be secured for $2.25—an average of about 9 cents each.

ATMOSPHERIC DUST.

AT the period of the great debate on spontaneous generation between M. Pasteur and Pouchet, the latter was the first to draw attention to the fact, that some of the minute spherical granulations, discovered by the microscope in dust deposited from the air in various regions of the globe, were essentially composed of silica. That they had often been mistaken for eggs of infusoria or for micrococci was very evident; but when the dust was submitted to complete calcination in a platinum crucible the same grains were still visible, with the same forms and dimensions as before.

I have more than once repeated this experiment of Pouchet's, but I have also made the opposite one and examined the action of heat upon micrococci, diatoms and oscillariæ, which are supposed to contain large quantities of silica.

There is no doubt that the dust of the atmosphere reveals to the microscope, besides the larger mineral fragments mostly of an angular shape, exceedingly minute circular or spherical bodies, being often not more than 0.001 of a millimetre in diameter, and very similar in size and shape, which resist the action of a white heat in contact with the air, and that of strong hydrochloric acid. In some of my observations they were remarkably numerous. Both before and after the action of heat they are more or less transparent. What can be the origin of these singular objects?

The same experiments repeated with siliceous algæ, such as those belonging to the large family of the *diatomaceæ,* and with the *micrococci* of impure waters or vegetable infusions, showed me that they do not retain their forms after being subjected to the above treatment, and that in many instances they can be totally destroyed by heat on the object glass itself. On the other hand, the fossil diatoms resisted the action of heat and acids and retained their forms. I can draw only one conclusion from these observations: namely, that the minute siliceous bodies found in the atmosphere are also fossil—they are micrococci of another age.—Dr. T. L. PHIPSON, F.C.S.

SILVER BROMIDE.—The action of light upon this substance varies according as the bromide is in the state of emulsion in an indifferent medium, like collodian, or in an organic substance readily oxidisable, like gelatine. Temperature, moisture, and mechanical pressure do not appear to have any influence.

ASTRONOMICAL MEMORANDA.

THE CORDOBA OBSERVATORY.

In the fall of 1870, Dr. B. A. Gould, formerly director of the Dudley Observatory, Albany, arrived in Cordoba, for the purpose of establishing a National Argentine Observatory, and making the requisite observations for forming a complete catalogue of the principal fixed stars of the southern hemisphere. A long delay of about two years, in the receipt of the instrument necessary to make these observations, has been the cause of giving to the world the *Uranometria Argentina*, a work analagous to the Uranometry of Argelander, which rendered such signal service to astronomy more than forty years ago.

Cordoba is situated about five hundred miles northwest of Buenos Ayres, the Observatory occupying a height upon the outskirts of the town, in latitude 31° 25′ 15.″4 south ; longtitude oh., 51m., 27s., east from Washington. The equipment of the Observatory consists of a 12.5 inch equatorial (object glass by Fitz) used mainly for observing comets, etc.; a smaller equatorial of about 8 inches aperture, devoted to observations of variable stars ; a Zöllner photometer ; and various accessory instruments. But the most important instrument is a Repsolds' Meridian Circle of about 8 inches aperture, which was mounted and ready for use in September, 1872. With this instrument observations of zones from 23° south, to 80° south declination—760 zones embracing 106,000 observations—have been completed, and the reductions have been well advanced. To determine absolute positions of all stars included in this catalogue, the instrumental constants were determined before and after each zone by a series of observations "consisting of transits of two standard time stars, as well as of one circumpolar star above, and one below the pole, together with measurements of nadir, collimation and level."

Dr. Gould has established, though necessarily on a limited scale, a Signal Service which will doubtless develop rapidly, when meteorology receives more attention in South America than it does at present. Meanwhile data of inestimable value are being collected at very slight expense, by interesting many of the intelligent land owners, in making sucn observations of the barometer, thermometer, etc., as may be made with little outlay of time and trouble. A Time Signal is sent at noon once a week, over the available telegraph lines of the country.

A force of four observers and several copyists, mostly Americans, is engaged upon the work in the various departments, and Dr. Gould has taken with him, within the past few months, a photographer, in order to obtain exact representations of several very interesting star clusters, which can be compared directly with the appearance of the cluster at any future time, and thus afford a means of detecting any changes which may occur in the relative positions of the component stars.

It is to be hoped that the political party now in power, —under whose auspices these institutions have originated and have been maintained—will retain its influence in the government, and thus be enabled to promote the interests of science in the country.

DISCOVERY OF A NEW ASTEROID.

The Smithsonian Institution has received from Professor Foerster, of Berlin, the announcement of the discovery, by Palisa, of a planet of the tenth magnitude, in eleven hours thirty-nine minutes Right Ascension, eight degrees twenty-five minutes north declination, with a daily motion of one minute, north. This discovery brings the total number of asteroids up to two hundred and twenty, making the eighth discovered since February 6, 1880. The date of discovery is omitted.

IN a paper recently read before the Royal Astronomical Society, Mr. Stone has called attention to "some difficulties connected with the determination of the diameter of Mars." Upon examining the Greenwich observations of the diameter, since 1851, very marked personal equations have been noticed in the different observers, discrepancies which seem somewhat difficult to account for. Mr. Stone says, "it looks as if there were two different diameters of Mars observed,—one when Mars is comparatively near to us, and the other when it is at its greatest distance from us. The result is that as far as one can trace it, there is a distinct break of continuity between the smaller and the larger measures ; as if the observers had included the planet's atmosphere when Mars is distant."

SWEDEN has decided to take part in the international meteorological and magnetic observations in the Polar regions, and will establish two observatories, one at Masselbay in Spitzbergen, and one at Haparanda at the head of the Gulf of Boothia. Haparanda is to be well supplied with self-registering and printing meteorological apparatus, and with astronomical instruments to carry on a series of regular observations.

PROFESSOR PICKERING has called attention to the peculiar resemblance between the spectrum of the star Oeltzen 17681 and that of the three stars discovered by Wolf and Rayet in 1867, (*Comptes Rendus*, vol. lxv., p. 292). The relative brightness is found to be different in these spectra, and the subject promises to repay further investigation. W.C.W.

Washington, D. C , March 3, 1881.

THE DAVIDSON ASTRONOMICAL OBSERVATORY AT SAN FRANCISCO, CALIFORNIA.

Prof. George Davidson, of the United States Coast and Geodetic Survey, has established a private observatory in San Francisco, and mounted the six-and-a-half inch Equatorial which was exhibited at the Centennial, but which has now a Villarcean governor, spectrescope, and other improvements.

The geographical position of this observatory is :
Latitude = 37° 47′ 22″.3 north.
Longitude = 122° 24′ 39″.0 west of Greenwich.
In time = 8^h 09^m $3^s.60$ west of Greenwich.
This fixes it as the most western observatory in America.

To observe the total solar eclipse of January 11, 1880, Prof. Davidson transported the instrument and the observatory to the summit of Santa Lucia Mountain, about thirty-five miles southward of Monterey, and six thousand feet elevation above the Pacific ocean. In this undertaking everything had to be carried up four thousand feet over a very steep and rugged trail by pack mules ; and the party encountered one of the fiercest snow stor msof that coast, but successfully accomplished the object of the undertaking which was made under the directions of the Superintendent of the Coast Survey. Whenever opportunities offer for observing at not less than ten thousand feet elevation, he will transport it to these high stations. [It was intended to use it in 1879 at two of the coast survey stations occupied in the Sierra Nevada having elevations of 9800 to 10,600 feet, but unfortunately it was not received in season.]

Mr. Davidson has been engaged in regular coast survey duties upon the Pacific almost continuously since the Spring of 1850, and has had large experience in observing at great elevations.

AN international exhibition connected with electricity will open in Paris, August 1, 1881, and will close on the 15th of November following.

SCIENCE IN FRANCE.

PARIS, February 12, 1881.

There is so much at present which is both novel and important in the scientific world, that I fear, in my endeavors to do full justice to everything, I shall, by attempting too much, find myself in a position analogous with that of a certain unfortunate person mentioned in history, who, while striving to seat himself upon two chairs simultaneously, fell ignominiously to the ground.

But let us not waste time in odious comparisons! The news has probably long since reached you of a wonderful fossil forest which has recently been discovered in Hindoostan, and of a prehistoric grotto somewhere on the border line of France, containing various kinds of warlike weapons of an exceedingly primitive design, together with a single human tooth. It is not of these, however, that I intend to speak, as beyond the facts themselves nothing of particular interest remains to be told.

In the medical world a little instrument newly invented, is attracting considerable attention. It is called the *crayon feu* and is worthy of something more than a passing description.

That all intelligent physicians recommend instant cauterization when a person has been bitten by a mad dog, or indeed a dog of any sort, is a well-known fact. It is not, however, generally speaking, an easy matter to find an appropriate piece of iron and a lighted fire all ready for the operation, and consequently it usually happens that some time elapses before the remedy can be applied. Of course we all know that delay in such matters frequently proves fatal, and it was of this undoubtedly that Dr. Moser was thinking when he invented the tiny, portable apparatus which he calls the *crayon feu*, and which is so simply constructed that it can be used alike by physicians, travellers, hunters, or indeed any one who has either been bitten himself or who is required to treat another person.

This little instrument consists of a pencil made of some peculiar composition which ignites instantly when a match is applied to it and becomes red-hot while the patient's wound is being washed. The point of the pencil is then introduced directly into the wound, and the cauterization is performed in an instant. The patient merely experiences a slight sensation of being burned, as the operation is over before he is able to feel any definite pain. A little wooden or metal cover is placed over the pencil when it is not in use, and at the other end is a small receptacle for the particular kind of wax matches which are required to light it.

The *crayon feu* is indeed *multum in parvo* and can be carried in the vest pocket. Medical men, scientific societies, and all public administrations in Paris have given it a warm welcome—no pun is here intended—and their example has been followed by a host of others, while Dr. Moser himself, is looked upon as a veritable benefactor to humanity.

No less interesting are the curious experiments recently made by a Hungarian, M. Kerdig, by means of a combustible substance which is undoubtedly destined to be used at some future time for illuminating purposes.

M. Kerdig begins by placing upon a table a number of lamps filled with this fluid, which, indeed, gives forth a most brilliant light. He then announces to the interested spectators that it is in no danger whatever of catching fire or exploding, and in order to illustrate this fact so that even the veriest skeptic shall believe, he pours a quantity of the liquid upon his hat and calmly sets fire to it. A mass of lurid flame rises instantly almost to the ceiling, but M. Kerdig, in no wise disconcerted, places his hat coolly upon his head, and waits until the flame gradualty dies out. He then exhibits the hat triumphantly to the audience—it is uninjured. He next sets fire to the floor, then to his handkerchief, saturated with the substance, and finally goes so far as to pour some of it into the palm of his hand and light it; but the floor, the handkerchief and the hand are all alike unharmed.

Of course all this appears most extraordinary at first sight, but a little careful investigation is in this case, as in many others, capable of reducing mountains to mole hills. The vapor of M. Kerdig's mineral substance possesses considerable expansive force, so that in reality it is the vapor which burns and not the liquid. The latter being at a very low temperature, produces no sensation of heat upon the hand, notwithstanding the flame above.

Now, I suppose you would like to know of what this interesting product consists. M. Kerdig says that it is a very volatile essence of naptha, to which is added a compound of various evaporating substances. Other people, however, affirm that it is a product derived from natural oils recently discovered in Hungary, which, when properly distilled, results in a peculiar substance, very volatile, and, what is of still more importance, very cheap. A faint odor of petroleum pervades it, accompanied by a slight aromatic fragrance, and when spread upon the hand a sensation of cold is felt.

We have just received intelligence from the south of France of the terrible ravages made upon the olive crops this season by an insect designated by the entomologists as the *Dacusoleæ*. It is a little gray fly, with several legs, and long yellow antennæ. There are two generations of them every year, one appearing in July, the other in September. The eggs are deposited in the fruit, and the larva, which resembles a little yellowish-white maggot, consumes the pulp, intersecting it with tiny passages. The adult leaves the olive and makes its way to the ground, where it is transformed into a chrysalis and remains buried during the winter months.

Speaking upon agricultural topics reminds me of an unprecedented phenomenon which has just occurred in one of the districts of Jonzac. Upon the estate of a certain M. Delaume, who lives at Sèville, there is a grape vine which for five years has been infested with phylloxera, and the greater part of whose branches have borne nothing whatever—neither leaves nor fruit during that period. But a most unforeseen and extraordinary thing suddenly happened. From one branch, which has hitherto been looked upon as quite dead, has sprung a magnificent grape vine, well formed, and of a beautiful, dark green color. No one, as yet, has been able to explain this singular occurrence.

Still less can we account for another most remarkable event, a description of which I lately read in a Hungarian paper. A criminal, it seems, had been hanged, and the physician in attendance declared that life was quite extinct. An autopsy was subsequently made upon the body, and the latter subjected to the action of a strong galvanic current. Within the space of two hours, signs of life were distinctly observed. The dead man recovered his senses completely, but succumbed, on the second day following, to cerebral congestion.

If this account be true, we cannot too greatly encourage the use of electricity as a resuscitative and vital agent, nor can we fail to admit that the present age is one of unparalleled phenomena. COSMOS.

LAWS OF THE DISENGAGEMENT OF ELECTRICITY BY PRESSURE IN TOURMALINE.—The authors announce the following laws as resulting from their experiments:—The two ends of a tourmaline evolve quantities of electricity respectively equal, but of opposite signs. The quantity liberated by a certain increase of pressure is opposite in its sign, but equal to that produced by an equal decrease of pressure. This quantity is proportional to the variation of the pressure, independent of the length of the tourmaline, and for one and the same variation of pressure per unit of surface it is proportional to the surface.—M M. Jacques and Pierre Curie.

SCIENCE:

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INSANITY VERSUS CIVILIZATION.

It is interesting to note the steady progress made by Alienists in solving the many difficult problems which appear to underlie the practice of their profession, and we would give full credit to those who, in a purely scientific spirit, are building a foundation on which a system of treatment for mental diseases may be erected, which shall accord with modern anatomical discovery and the latest theories which have been developed by a careful study of insanity in all its forms.

The last number of the "*Journal of Nervous and Mental Diseases*" may be studied with advantage by those who would gather a few opinions expressed by those who "minister to a mind diseased." In the first place, we have the authority of Dr. J. S. Jewell for stating that insanity is on the increase, and must still increase with the advance of civilization. In this opinion he is confirmed by Professor W. Erb, of Leipzig, and others. Among the reasons advanced for alleging that the advance of civilization is favorable to an increase in nervous and mental diseases, it is stated, that the nervous systems of highly cultivated and refined individuals among civilized people are more complex and refined in structure and more delicate in susceptibility and action, at least in their higher parts, than the nervous systems of savages. As civilization advances, the occupations increase which imply a cultivation of the sensibilities, more especially those comprehended under the sense of beauty. A relatively large number of persons give themselves to the study and practice of art in its various forms, to polite literature, and to sedentary occupations. The more a part of the nervous system is used the more extended its development. In highly civilized communities there is a constant tendency to a loss of balance in nerve development, in which the *sensitive* side of the nervous system preponderates over the motor part of the same. Now, all disturbances of symmetry or balance in development tend toward disease; they do not constitute disease, but verge in that direction. This state of things is the result of advancing civilization, and involve a world of minor consequences, both for the weal and woe of the people.

Such is the substance of Dr. Jewell's views, who also charges the system of education in public schools with being the cause of increasing the number of cases of insanity, by breaking up the "*nerve health*" of youths. This remark applies equally to the course of study in Colleges and Universities, and the overworked student in hundreds of cases obtains his degree at the expense of loss of health, and retires with general nervous and brain exhaustion, and afflicted with melancholia, hysteria, vascular irregularities, cerebral congestion, neuralgias and other disorders of the same character.

Space will not permit us to describe the many forms in which the adult, in civilized life, courts the approach of the various forms of insanity; but they can be easily surmised, and are often referred to in articles treating on this subject.

We admit, with Dr. Jewell, that the higher developments of civilized life may produce a higher strain on the nervous system which may lead to more frequent cases of its derangement; but we think he draws too wide a parallel when he makes a distinction between our present modes of existence and actual savage life. When speaking of the increase of insanity, it is presumed reference is made to a period covering, perhaps, the last fifty years. Such being the case, we think Dr. Jewell has hardly done justice to the subject, by omitting the many mitigating circumstances attending an advanced civilization, which certainly alleviate many of the mental strains spoken of by him.

Within the last fifty years, the hours of labor have been curtailed both in manufactories and among the industrial classes in cities. Stores which at one time were open until midnight are now closed at 7 P. M. Means of recreation and amusements which until recently were monopolized by a few, are now enjoyed by the millions. Improved methods of transit now enable citizens to enjoy their evenings after the hours of labor, strolling upon grassy meadows or upon the shores of the ocean. Literature of an entertaining character is also now produced so cheaply as to make its use universal. The laws of hygiene are also at this present day better understood, and, by perfecting man's physical condition, instill fresh energy into his mental powers.

We thus find that, so far from all the conditions attending an advanced civilization being favorable to

insanity, many have a tendency to promote the most perfect mental and physical development.

If the Alienist would solve the problem attending the increase of cases of insanity, we would direct him to other sources of the evil than that of civilization; let him probe the open and hidden vices of great cities; let him calculate the effect of the indiscriminate use of alcoholic liquors and the pernicious abuse of potent drugs. We regard opium, tobacco, chloral and sewer gas as some of the offending agents which weaken and debilitate the mental powers, rather than the mild educational cause of our public schools or the attending circumstances of student-life.

Dr. Jewell himself admits the destructive effects of these agents upon the nervous system, but they are classed as due to the influence of civilization. This we think an error, as they are connected with vices of a debased life; and although insanity may be on the increase, we consider it is far from conclusive that to civilization we should attribute the primary cause.

SCIENTIFIC SOCIETIES IN WASHINGTON, D. C.

THE BIOLOGICAL SOCIETY.—Three papers were read at the last meeting, Friday, Feb. 25, as follows: A Description of Pronuba yuccasella, by Prof. C. V. Riley; The Hall Collection of Fossils from New York, by Prof. C. A. White; and Suctorial Prehension in the Animal Kingdom, by Mr. Smiley. Professor Riley's paper was a revision of his communications before the American Association at St. Louis and in other places, concerning a moth, the *Pronuba yuccasella*, which not only deposits its eggs in the capsules of the Yucca, but which is also indispensable to the fertilization of the ovaries of that plant. It was remarked by Mr. Lester F. Ward, in commenting upon the paper, that we have here the most wonderful example of commensualism. Professor White is in charge of the duplicate set from the Hall Collection of Fossils sent to the National Museum. His remarks were a brief description of them as they now appear. There are about 1500 entries, and they represent nearly all the types in the Collection of the American Museum. Mr. Smiley's paper was a description of suctorial organs in the various divisions of the animal kingdom. These organs have in different circumstances, three functions, locomotion, anchoring and the seizure of prey. The author has bestowed a great deal of care on his communication and brought together a valuable mass of material.

THE ANTHROPOLOGICAL SOCIETY.—The Society met in the main hall of the National Medical College, Major J. W. Powell in the chair. The following papers were read: Amphibious Aborigines of Alaska, by Ivan Petroff; The Evolution of Marriage Ceremonies and Their Import, by Dr. A. F. A. King. Mr. Petroff described his experience among the shore Innuit population of Alaska, from the lower peninsula north to the Yukon mouth. There is water and marsh, mud and swamp everywhere, and the heavens swell the mass by their contribution of fog, rain, snow and sleet. The natives are enveloped in this watery environment the year round and thrive upon it. They even drink enormous quantities of it, not excepting the salt water of the bays and fiords, in their long fishing journeys, Doctor King's paper was an argument to prove that the progress of civilization had the tendency to set aside the laws of sexual relations which exist in a state of nature, such as the survival of the fittest, the observance of natural periods, and sexual selection. The paper was discusssed by Major Powell and Mr. Ward.

ACTION OF AN INTERMITTENT BEAM OF RADIANT HEAT UPON GASEOUS MATTER.*

By JOHN TYNDALL, F. R. S.

The Royal Society has already done me the honor of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Professor Magnus and the late Professor Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question; the latest of whom,† while justly recognizing the extreme difficulty of the subject, and while verifying, so far as their experiments reach, what I had published regarding dry gases, find me to have fallen into what they consider grave errors in my treatment of vapors.

None of these investigators appear to me to have realized the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognize the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed, some time ago the threads of the inquiry. The results shall, in due time, be communicated to the Royal Society; but meanwhile, I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

After working for some time with the thermopile and galvanometer, it occurred to me several weeks ago that the results thus obtained might be checked by a more direct and simple form of experiment. Placing the gases and vapors in diathermanous bulbs, and exposing the bulbs to the action of radiant heat, the heat absorbed by different gases and vapors ought, I considered, to be rendered evident by ordinary expansion. I devised an apparatus with a view of testing this idea. But, at this point, and before my proposed gas thermometer was constructed, I became acquainted with the ingenious and original experiments of Mr. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies.

From the first, I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged upon by the beam. But if this be the case, and if gases and vapors really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds; while highly athermanous bodies would produce loud sounds; the strength of the sound being, in a sense, a measure of the absorption. The first experiment made, with a view of testing this idea, was executed in the presence of Mr. Graham Bell;‡ and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to em-

*Proceedings of the Royal Society.

† MM. Lecher and Pernter, "Philosophical Magazine," January, 1881. "Sitzb. der K. Akad. der Wissensch. in Wien," July, 1880.

‡On the 20th of November: see "Journal of the Society of Telegraph Engineers," December 8, 1880.

brace most of the gases and vapors employed in my former researches. My first source of rays was a Siemens' lamp connected with a dynamo-machine, worked by a gas engine. A glass lens was used to concentrate the rays, and afterwards two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about seven inches distant from the lens. A circle of sheet zinc provided first with radial slits and afterwards with teeth and interspaces, cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits produced the desired intermittence,* while a flask containing the gas or vapor to be examined received the shocks of the beam immediately behind the rotating disc. From the flask a tube of india-rubber, ending in a tapering one of ivory or box wood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. Graham Bell, the arrangement here described is rude; it is, however, effective.

With this arrangement the number of sounding gases and vapors was rapidly increased. But I was soon made aware that the glass lenses withdrew from the beam its effectual rays. The silvered mirrors employed in my previous researches were therefore invoked; and with them, acting sometimes singly and sometimes as conjugate mirrors, the curious and striking results which I have now the honor to submit to society were obtained.

Sulphuric ether, formic ether, and acetic ether being placed in bulbous flasks,† their vapors were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disc, so that the intermittent beam passed through the vapor, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapors which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapors. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapors of amylene, iodide of ethyl, iodide of methyl and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

It is the vapor, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapor-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rock-salt cell filled entirely with a volatile liquid, and subjected to the intermittent beam, produced no sound. This cell was circular and closed at the top. Once, while operating with a highly athermanous substance, a distinct musical note was heard. On examining the cell, however, a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled, the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disc and allowing its slits to act like the perforations of a syren.

Thus, as regards vapors, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam it yielded a musical note, but so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid, the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide, the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas, it gave birth to a musical note which, when the beam was in good condition, and the bulb well chosen, seemed as loud as that of an ordinary organ pipe.* We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorpti n and the intensity of the sound go hand in hand.

A soap bubble blown with nitrous oxide, or olefiant gas, and exposed to the intermittent beam produced no sound, no matter how its size might be varied. The pulses obviously expended themselves upon the flexible envelope, which transferred them to the air outside.

But a film thus impressionable to impulses on its interior surface, must prove at least equally sensible to sonorous waves impinging on it from without. Hence, I inferred, the eminent suitability of soap bubbles for sound lenses. Placing a "sensitive flame" some feet distant from a small sounding reed, the pressure was so arranged that the flame burnt tranquilly. A bubble of nitrous oxide (sp. gr. 1·527) was then blown, and placed in front of the reed. The flame immediately fell and roared, and continued agitated as long as the lens remained in position. A pendulous motion could be imparted to the bubble, so as to cause it to pass to and fro in front of the reed. The flame responded, by alternately roaring and becoming tranquil, to every swing of the bubble. Nitrous oxide is far better for this experiment than carbonic acid, which speedily ruins its envelope.

The pressure was altered so as to throw the flame, when the reed sounded, into violent agitation. A bubble blown with hydrogen (sp. gr. 0·069) being placed in front of the reed, the flame was immediately stilled. The ear answers instead of the flame.

In 1839, I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was therefore great. Placing a small quantity of liquid ammonia in one of the flasks, and warming the liquid slightly, the intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced. By the proper application of heat to a liquid the sounds may be always intensified. The ordinary temperature, however, suffices in all the cases thus far referred to.

In this relation the vapor of water was that which interested me most, and as I could not hope that at ordinary temperatures it existed in sufficient amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling-point. Placed in the intermitten beam, I heard—I avow with delight

* When the disc rotates the individual slits disappear, forming a hazy zone through which objects are visible. Throwing by the clean hand, or better still by white paper, the beam back upon the disc, it appears to stand still, the slips forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colors of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colors disappear. The flashes also produce a giddiness, sometimes intense enough to cause me to grasp the table to keep myself erect.

† I have employed flasks measuring from 8 inches to ¾ths of an inch in diameter. The smallest flask, which had a stem with a bore of about ⅛th of an inch in diameter, yielded better effects than the largest. Flasks from 2 to 3 inches in diameter yield good results. Ordinary test-tubes also answer well.

* With conjugate mirrors the sounds with olefiant gas are readily obtained at a distance of twenty yards from the lamp. I hope to be able to make a candle flame effective in these experiments.

—a powerful musical sound produced by the aqueous vapor.

Small wreaths of haze, produced by the partial condensation of the vapor in the upper and cooler air of the flask, were, however, visible in this experiment; and it was necessary to prove that this haze was not the cause of the sound. The flask was, therefore, heated by a spirit-flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam of light then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapor, however, the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10° C., fully expecting that the sound would vanish at this temperature; but not withstanding the tenuity of the vapor, the sound extracted from it was not only distinct but loud.

Three empty flasks, filled with ordinary air, were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit-lamp until all visible humidity has been removed, and afterwards urging dried air through them, on being placed in the intermittent beam the sotum in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the athermancy and diathermancy of gases and vapors, and it cannot be otherwise than satisfactory to me to find that particular vapor, whose alleged deportment towards radiant heat has been most strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapor, we are prepared for the fact that an exceedingly small percentage of any highly athermanous gas diffused in air suffices to exalt the sounds. An accidental observation will illustrate this point. A flask was filled with coal-gas and held bottom upwards in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal-gas. The flask was then placed upright, with its mouth open upon a table, and permitted to remain there for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.

Transferring a small flask or a test-tube from a cold place to the intermittent beam, it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporisation by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids andtheir vapors absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapor, would detach the effective rays, and thus quench the sounds. The experiment was made, and the conclusion verified. A layer of water, formic ether, sulphuric ether, or acetic ether, ⅛th of an inch in thickness, rendered the transmitted beam powerless to produce any musical sound. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapors—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that with those gases which yielded a feeble sound, the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds, the displacement was prompt and forcible.

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FURTHER EXPERIMENTS.

Since the handing in of the foregoing note, on the 3rd of January, the experiments have been pushed forward; augmented acquaintance with the subject serving only to confirm my estimate of its interest and importance.

All the results described in my first note have been obtained in a very energetic form with a battery of sixty Grove's cells.

On the 4th of January I chose for my source of rays a powerful lime-light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a nearer approach to the source of rays. Tested with this new reflector the stronger vapors rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those of the lime-light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapors.

Abandoning the mirror and bringing the candle close to the rotating disc, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the rotating disc produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds. Maintaining the flask in position behind the rotating disc, amusing alternations of sound and silence accompanied the alternate introduction and removal of the poker.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water.

As might be expected from the foregoing experiments, an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three, the sounds, though enfeebled, were still distinct.

My neglect of aqueous vapor had led me for a time astray in 1859, but before publishing my results I had discovered my error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks and permitted to remain in the laboratory from the 23d of December to the 4th of January. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due, not to dry air, but to traces of aqueous vapor.

An ordinary bottle, containing sulphuric acid for laboratory purposes, being connected with the ear and

placed in the intermittent beam, emitted a faint, but distinct, musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some days, was placed in the beam: the dry air above the liquid proved absolutely silent.

On the evening of January the 7th, professor Dewar handed me four flasks treated in the following manner. Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium; while the fourth contained a small quantity of phosphoric anhydride. They were closed with well fitting india-rubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last mentioned being the strongest. Tested again six hours later, the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with sulphuric acid always enfeebles, and sometimes destroys the sound.

A bulb, less than a cubic inch in volume, and containing a little water, lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit-lamp, the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb* is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapor. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapor.

The action of aqueous vapor being thus revealed, the necessity of thoroughly drying the flasks, when testing other substances, becomes obvious. The following plan has been found effective. Each flask is first heated in the flame of a spirit-lamp till every visible trace of internal moisture has disappeared, and it is afterwards raised to a temperature of about 400° C. While the flask is still hot, a glass tube is introduced into it and air freed from carbonic acid by caustic potash, and from aqueous vapor by sulphuric acid, is urged through the flask until it is cool. Connected with the ear-tube, and exposed immediately to the intermittent beam, the attention of the ear, if I may use the term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

In 1868 I determined the absorptions of a great number of liquids whose vapors I did not examine. My experiments having amply proved the parallelism of liquid and vaporous absorption, I held undoubtingly twelve years ago that the vapor of cyanide of ethyl and of acetic acid would prove powerfully absorbent. This conclusion is now easily tested. A small quantity of either of these substances, placed in a bulb a cubic inch in volume, warmed, and exposed to the intermittent beam, emits a sound of extraordinary power.

I also tried to extract sounds from perfumes, which I had proved in 1861 to be absorbers of radiant heat. I limit myself here to the vapors of patchouli and cassia, the former exercising a measured absorption of 30, and the latter an absorption of 109. Placed in dried flasks, and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than patchouli.

* In such bulbs even bisulphide of carbon vapor may be so nursed as to produce sounds of considerable strength.

Many years ago I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh-gas was of special interest. Professor Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask, and permitted to displace the air within it. Exposed to the intermittent beam, its sound was similar in power to that of marsh-gas.

The specific gravity of marsh gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This, however, is not the case. After a considerable interval, the film of this gas clinging to the interior surface of the flask was able to produce sounds of great power.

A small quantity of liquid bromine being poured into a well-dried flask, the brown vapor rapidly diffused itself in the air above the liquid. Placed in the intermittent beam, a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapor. But my former experiments on this vapor were conducted with obscure heat; whereas, in the present instance, I had to deal with the radiation from incandescent lime, whose heat is, in part, luminous. Now, the color of the bromine vapor proves it to be an energetic absorber of the luminous rays; and to them, when suddenly converted into thermometric heat in the body of the vapor, I thought the sounds might be due.

Between the flash containing the bromine and the rotating disc I therefore placed an empty glass cell: the sounds continued. I then filled the cell with transparent bisulphide of carbon: the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light, while allowing the rays of heat free transmission: the sounds were immediately stilled.

Iodine vaporised by heat in a small flask yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might indeed have been foreseen that the rays transmitted by the iodine as a liquid would also be transmitted by its vapor, and thus fail to be converted into sound.*

To complete the argument:—While the flask containing the bromine vapor was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disc. There was no sensible abatement of the sounds with either bromine or iodine vapor.

In these experiments the rays from the lime-light were converged to a point a little beyond the rotating disc. In the next experiment they were rendered parallel by the mirror, and afterwards rendered convergent by a lens of ice. At the focus of the ice lens the sounds were extracted from both bromine and iodine vapor. Sounds were also produced after the beam had been sent through the alum solution and the ice lens conjointly.

With a very rude arrangement I have been able to hear the sounds of the more active vapors at a distance of 100 feet from the source of rays.

Several vapors other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapors of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that

* I intentionally use this phraseology.

even the vapors of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

THE UNITY OF NATURE.

By the Duke of Argyll.

VI.

(*Continued from page* 103.)

ON THE MORAL CHARACTER OF MAN, CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

In dealing with this question, it is a comfort to remember that we are in possession of analogies deeply seated in the constitution and in the course of Nature. It is quite possible to assign to Intuition or to Instinct the place and rank which really belongs to it, and to assign also to what is called Experience the functions which are unquestionably its own. There is no sense or faculty of the mind which does not gain by education—not one which is independent of those processes of development which result from its contact with the external world. But neither is there any sense or faculty of the mind which starts unfurnished with some one or more of those intuitive perceptions with which all education and all development must begin. Just as every exercise of reason must be founded on certain axioms which are self-evident to the logical faculty, so all other exercises of the mind must start from the direct perception of some rudimentary truths. It would be strange indeed if the moral faculty were any exception to this fundamental law. This faculty in its higher conditions, such as we see it in the best men in the most highly civilized communities, may stand at an incalulable distance from its earliest and simplest condition, and still more from its lowest condition, such as we see it in the most degraded races of mankind. But this distance has been reached from some starting-point, and at that starting-point there must have been some simple acts or dispositions to which the sense of obligation was instinctively attached. And beyond all question this is the fact. All men do instinctively know what gives pleasure to themselves, and therefore also what gives pleasure to other men. Moreover, to a very large extent, the things which give them pleasure are the real needs of life, and the acquisition or enjoyment of these is not only useful but essential to the well-being or even to the very existence of the race. And as Man is a social animal by nature, with social instincts at least as innate as those of the Ant or the Beaver or the Bee, we may be sure that there were and are born with him all those intuitive perceptions and desires which are necessary to the growth and unfolding of his powers. And this we know to be the fact, not only as a doctrine founded on the unities of Nature, but as a matter of universal observation and experience. We know that without the Moral Sense Man could not fulfill the part which belongs to him in the world. It is as necessary in the earliest stages of the Family and of the Tribe, as it is in the latest developments of the State and of the Church. It is an element without which nothing can be done—without which no man could trust another, and, indeed, no man could trust himself. There is no bond of union among men—even the lowest and the worst—which does not involve and depend upon the sense of obligation. There is no kind of brotherhood or association for any purpose which could stand without it. As a matter of fact, therefore, and not at all as a matter of speculation, we know that the Moral Sense holds a high place as one of the necessary conditions in the development of Man's nature, in the improvement of his condition, and in the attainment of that place which may yet lie before him in the future of the world. There are other sentiments and desires, which, being as needful, are equally instinctive. Thus, the desire of communicating pleasure to others is one of the instincts which is as universal in Man as the desire of communicating knowledge. Both are indeed branches of the same stem—off-shoots from the same root. The acquisition of knowledge, to which we are stimulated by the instinctive affections of curiosity and of wonder, is one of the greatest of human pleasures, and the desire we have to communicate our knowledge to others is the great motive-force on which its progress and accumulation depend. The pleasure which all men take, when their dispositions are good, in sharing with others their own enjoyments, is another feature quite as marked and quite as innate in the character of Man. And if there is any course of action to which we do instinctively attach the sentiment of moral approbation, it is that course of action which assumes that our own desires, and our own estimates of good, and the standard by which we ought to judge of what is due to and is desired by others. The social instincts of our nature must, therefore, naturally and intuitively indicate benevolence as a virtuous, and malevolence as a vicious disposition; and, again, our knowledge of what is benevolent and what is malevolent is involved in our own instinctive sense of what to us is good, and of what to us is evil. It is quite true that this sense may be comparatively low or high, and consequently that the standard of obligation which is founded upon it may be elementary and nothing more. Those whose own desires are few and rude, and whose own estimates of good are very limited, must of course form an estimate correspondingly poor and scant of what is good for, and of what is desired by, others. But this exactly corresponds with the facts of human nature. This is precisely the variety of unity which its phenomena present. There are no men of sane mind in whom the Moral Sense does not exist; that is to say, there are no men who do not attach to some actions or other the sentiment of approval, and to some other actions the opposite sentiment of condemnation. On the other hand, the selection of the particular actions to which these different sentiments are severally attached is a selection immensely various; there being, however, this one common element in all—that the course of action to which men do by instinct attach the feeling of moral obligation, is that course of action which is animated by the feeling that their own desires and their own estimate of good is the standard by which they must judge of what is due by them to others, and by others to themselves.

And here we stand at the common point of departure from which diverge the two great antagonistic schools of ethical philosophy. On the other hand in the intuitive and elementary character which we have assigned to the sentiment of obligation, considered in itself, we have the fundamental position of that school which asserts an independent basis of morality; whilst, on the other hand, in the elementary truths which we have assigned to the Moral Sense as its self-evident apprehensions, we have a rule which corresponds, in one aspect at least, to the fundamental conception of the Utilitarian school. For the rule which connects the idea of obligation with conduct tending to the good of others, as tested by our own estimate of what is good for ourselves, is a rule which clearly brings the basis of morality into very close connection with the practical results of conduct. Accordingly, one of the ablest modern advocates of the Utilitarian system has declared that "in the golden rule of Jesus of Nazareth we read the complete spirit of the ethics of Utility. To do as you would be done by, and to love your neighbor as yourself, constitute the ideal perfection of Utilitarian morals."[2]

This may well seem a strange and almost a parodoxical result to those who have been accustomed to consider the Utilitarian theory not so much a low standard of morals, as an idea which is devoid altogether of that ele-

[2] J. S. Mill: "Utilitarianism," pp. 24, 25.

ment in which the very essence of morality consists. But it is a result due to these two causes—first, that under the fire of controversy Utilitarians have been obliged to import into the meaning of their words much that does not really belong to them; and secondly, to the fact, that when this essential alteration has been made, then the theory, or rather the portion of it which remains, does represent one very important aspect of a very complex truth.

It will be well to examine a little more closely the different ways in which these two causes operate.

In the first place, as regards the ambiguities of language, a moment's consideration will convince us that the word "utility" has, in its proper and primary signification, nothing whatever of the ethical meaning which is attached to it in the Utilitarian theory of morals. In its elementary signification the useful is simply the serviceable. It is curious to observe that this last word has no ethical savor about it. On the contrary, it is associated rather with the lower uses than with the higher of conduct. If this be objected to as preventing the two words from being really the equivalent of each other, then at least let it be recognized that utility must be divested of its ethical associations before it can be set up as an ethical test. If utility is first assumed to be the equivalent of goodness, it becomes of course a mere play on words to represent usefulness as the criterion of virtue. If we are to conduct our analysis correctly, we must expel from utility every adventitious element of meaning. The usefulness of a thing means nothing more than its conduciveness to some purpose. But it may be any purpose,—morally good, or morally bad, or morally indifferent. The boot-jack, the thumb-screw, and the rack are all useful machines for the purpose of producing torture on the victim, and for the purpose, too, of giving to the torturers that pleasure or satisfaction which wicked men find in tyranny or revenge. The words "good" and "bad" are themselves often used in a secondary and derivative sense, which, like "useful," may be destitute of any ethical meaning. A good thumb-screw would mean an implement well adapted to produce the most exquisite pain. A good torture may mean a torture well calculated to gratify the savage sentiment of revenge. In like manner, although not to the same extent, the words "right" and "wrong" are often used with no ethical element of meaning. The right way for a man who wishes to commit suicide would be the way to a precipice over which he desires to throw himself. But the same way is the wrong way for him, if he wishes to avoid the danger of falling. In this way we may speak of the right way of doing the most wicked things. One most eminent expounder of the Utilitarian theory has taken advantage of this common use of the words "good" and "bad," and of "right" and wrong," to represent utility and inutility to be the essential idea of all goodness and of all badness respectively.[3] Thus the unavoidable ambiguities of speech are employed to give a scientific aspect to the confounding and obliteration of the profoundest distinctions which exist in knowledge. By the double process of expelling from goodness the idea of virtue, and of inserting into utility the idea of beneficence, the fallacies of language become complete. Because subserviency to purpose of any kind is the meaning of "good," when applied equally to an instrument of torture and to an instrument for the relief of suffering, therefore, it is argued, the same meaning must be the essential one when we speak of a good man. And so indeed it may be, if we know or assume beforehand what the highest purpose is to which Man can be made subservient. There is a well-known Catechism of one of the Reformed churches which opens with the qnestion, "What is the chief end of Man?" The answer is perhaps one of the noblest in the whole compass of theology. "Man's chief end is to glorify God and to enjoy Him forever."[4] Given certain further beliefs as to the character of the Divine Being, and the methods of his Government, then indeed it would be true that this is a conception of the purpose of Man's existence which would erect mere serviceableness or utility into a perfect rule of conduct. Perhaps even a lower or less perfect conception of the great aim of Man's life would be almost enough. If virtue and beneficence are first assumed to be the highest purpose of his being, then subserviency to that purpose may be all that is meant by goodness. But, without this assumption as to the "chief end of Man," there would be no ethical meaning whatever in the phrase of "a good man." It might mean a good thief, or a good torturer, or a good murderer. Utility, that is to say, mere subserviency to any purpose, is undoubtedly a good thing in itself, and of this kind is the goodness of a machine which is invented for a bad or evil purpose. But this utility in the machine is, so far as the machine is concerned, destitute of any moral character whatever, and, so far as those who employ it are concerned, the utility is not virtuous, but, on the contrary, it is vicious. It is clear, therefore, that when the word "utility" is used as meaning moral or even physical good, and still more when it is identified with virtue, or when it is declared to be the standard of that which is right or virtuous in conduct, the word is used not in its own proper sense, but in a special or adventitious sense, in which it is confined to one special kind of usefulness, namely, that which conduces to good ends, and good aims, and good purposes. That is to say, the sense in which utility is spoken of as the test or standard of virtue is a sense which assumes that goodness and virtue are independently known, or, in other words, that they are determined and recognized by some other test and some other standard.

It is, however, clear that when by this other test and standard, whatever it may be, we have already felt or apprehended that it is right and virtuous to do good to others, then the usefulness of any action or of any course of conduct, in the production of such good, does become a real test and indication of that which we ought to do. It is a test or indication of the particular things which it is right to do, but not at all a test of the moral obligation which lies upon us to do them. This obligation must be assumed, and is assumed, in every argument on the moral utility of things. It is by confounding these two very distinct ideas that the Utilitarian theory of the ultimate basis of moral obligation has so long maintained a precarious existence, borrowing from the misuse of words a strength which is not its own. But the moment this distinction is clearly apprehended, then, although we set aside the bare idea of usefulness, apart from the good or bad purpose towards which that usefulness conduces, as affording any explanation whatever of the ultimate nature and source of duty, we may well, nevertheless, be ready to adopt all that the Utilitarian theory can show us of that inseparable unity which is established in the constitution of the world between the moral character and the ultimate results of conduct. As far as these results can be traced beforehand, and in proportion as they can be traced farther and farther in the light of expanding knowledge, they do indicate the path of duty. They do indicate the line of action which is obligatory on voluntary agents, to whom a very large amount of power is given in directing the course of things. Beyond all doubt there are a thousand acts and a thousand courses of conduct which are in accordance with the Moral Sense, because and only because of the known happiness of their effects. This is the fact, or rather the class of facts, which has in all ages recommended the Utilitarian theory of morals to so many powerful minds. For, indeed, if we understand by utility not the low or limited idea of mere usefulness for any purpose—not even the

[3] Herbert Spencer: "Data of Ethics," chap. iii.

[4] "The Shorter Catechism, presented by the Westminster Assembly of Divines to both Houses of Parliament, and by them approved."

mere idea of pleasure as an unquestionable good of its own kind, nor the mere idea of immediate profit or advantage—but the very different conception of the beneficence of ultimate results on the welfare of all men and of all creatures, then there may be, and probably there is, an universal and absolute coincidence between the things which it is wise and the things which it is right to do. Men may imagine, and they have imagined, that under this conception of utility they can devise a system of morality which is of such transcendental excellence that it is far too good for earth. Thus it has been laid down that evolution, in its most perfect conception, would be such that the development of every creature would be compatible with the equal development of every other. In such a system there would be no "struggle for existence—no harmful competition, no mutual devouring —no death."[5] The inspired imaginings of the Jewish prophets of some future time when the lion shall lie down with the lamb, and the ideas which have clustered round the Christian Heaven, are more probably the real origin of this conception than any theory of evolution founded on the facts and laws of Nature. But, for all practical purposes, such a system of ethics is as useless as the dreams of Plato's Republic or of More's Utopia. If, however, we have got from some independent source a right idea of that which will be most beneficent in its ultimate results, we may well be guided by this light in so far as we can see it. But inasmuch as these far-off results and tendencies of conduct cannot always be within sight, and are indeed very often wholly beyond the horizon visible to us, this admission, or rather this high doctrine that the right and the useful are always coincident, is a widely different doctrine from that which identifies the sense of obligation with the perception of utility. The mere perception that any act or course of conduct will certainly be beneficent in its results, would be of no avail without the separate feeling that it is right to strive for results which are beneficent.

And here it is well worthy of observation, that in direct proportion to the height and sublimity of the meaning artificially attached to the word "utility," it becomes less and less available as a test or as a rule of conduct. So long as the simple and natural meaning was put upon utility, and the good was identified with the pleasurable, the Utilitarian theory of morals did indicate at least some rule of life, however low that rule might be. But now that the apostles of that theory have been driven to put upon utility a transcendental meaning, and the pleasurable is interpreted to refer not merely to the immediate and visible effects of conduct on ourselves or others, but to its remotest effects upon all living beings, both now and for all future time, the Utilitarian theory in this very process of sublimation becomes lifted out of the sphere of human judgment. If it be true "that there can be no correct idea of a part without a correct idea of the correlative whole," and if human conduct in its tendencies and effects is only "a part of universal conduct,"[6]—that is to say, of the whole system of the universe in its past, its present, and its future—then, as this whole is beyond all our means of knowledge and comprehension, it follows that utility, in this sense, can be no guide to us. If indeed this system of the universe has over it or in it one Supreme Authority, and if we knew on that authority the things which do make, not only for our own everlasting peace, but for the perfect accomplishment of the highest purposes of creation to all living things, then indeed the rule of utility is resolved into the simple rule of obedience to legitimate Authority. And this is consistent with all we know of the Unity of Nature, and with all that we can conceive of the central and ultimate Authority on which its order rests. All intuitive perceptions come to us from that Authority. All the data of reason come to us from that Authority. All these in their own several spheres of operation may well guide us to what is right, and may give us also the conviction that what is right is also what is best, "at last, far off, at last to all."

Thus far a clear and consistent answer can be given to one of the greatest questions of ethical inquiry, namely, the nature of the relation between those elements in conduct which make it useful, and those elements in conduct which make it virtuous. The usefulness of conduct in promoting ends and purposes which are good is, in proportion to the nature and extent of that good, a test and an index of its virtue. But the usefulness of conduct in promoting ends and purposes which are not good is a mark and index, not of virtue, but of vice. It follows from this that utility in itself has no moral character whatever apart from the particular aim which it tends to accomplish, and that the moral goodness of that aim is presupposed when we speak or think of the utility of conduct as indicative of its virtue. But this character of goodness must be a matter of independent and instinctive recognition, because it is the one distinction between the kind of usefulness which is virtuous and the many kinds of usefulness which are vicious. Accordingly we find in the last resort that our recognition of goodness in the conduct of other men towards ourselves is inseparable from our own consciousness of the needs and wants of our own life, and of the tendency of that conduct to supply them. This estimate of goodness seated in the very nature of our bodies and of our minds, becomes necessarily, also, a standard of obligation as regards our conduct to others: for the unity of our nature with that of our kind and fellows is a fact seen and felt intuitively in the sound of every voice and in the glance of every eye around us.

But this great elementary truth of morals, that we ought to do to others as we know we should wish them to do to us, is not the only truth which is intuitively perceived by the Moral Sense. There is, at least, one other among the rudiments of duty which is quite as self-evident, quite as important, quite as far-reaching in its consequences, and quite as early recognized. Obedience to the will of legitimate Authority is necessarily the first of all motives with which the sense of obligation is inseparably associated; whilst its opposite, or rebellion against the commands of legitimate Authority, is the spirit and the motive upon which the Moral Sense pronounces its earliest sentence of disapproval and of condemnation. At first sight it may seem as if the legitimacy of any Authority is a previous question requiring itself to be determined by the Moral Sense, seeing that it is not until this character of legitimacy or rightfulness has been recognized as belonging to some particular Authority, that obedience to its commands comes in consequence to be recognized as wrong. A moment's consideration, however, will remind us that there is at least one Authority the rightfulness of which is not a question but a fact. All men are born of parents. All men, moreover, are born in a condition of utter helplessness and of absolute dependence. As a mattor of fact, therefore, and not at all as a matter of question or of doubt, our first conception of duty, or of moral obligation, is necessarily and universally attached to such acts as are in conformity with the injunctions of this last and most indisputable of all Authorities.

Standing, then, on this firm ground of universal and necessary experience, we are able to affirm with absolute conviction that our earliest conceptions of duty—our earliest exercises of the Moral Sense—are not determined by any considerations of utility, or by any conclusions of the judgment on the results or on the tendencies of conduct.

But the same reasoning, founded on the same principle of simply investigating and ascertaining facts, will carry us a great way farther on. As we grow up from infancy, we find that our parents are themselves also subject to Authority, owing and owning the duty of obedi-

Herbert Spencer: "Data of Ethics," chap. ii. pp. 18, 19.

Herbert Spencer: "Dara of Ethics," chap. i. pp. 1-6.

ence to other persons or to other powers. This higher Authority may be nothing but the rules and customs of a rude tribe; or it may be the will of an absolute sovereign; or it may be the accumulated and accepted traditions of a race; or it may be the laws of a great civilized community; or it may be the Authority, still higher, of that Power which is known or believed to be supreme in Nature. But in all and in each of these cases, the sense of obligation is inseparably attached to obedience to some Authority, the legitimacy or rightfulness of which is not itself a question but a fact.

It is true, indeed, that these rightful Authorities, which are enthroned in Nature, are fortified by power to enforce their commands, and to punish violations of the duty of obedienee. It is true, therefore, that from the first moments of our existence the sense of obligation is re-inforced by the fear of punishment. And yet we know, both as a matter of internal consciousness, and as a matter of familiar observation in others, that this sense of obligation is not only separable from the fear of punishment, but is even sharply contra-distinguished from it. Not only is the sense of obligation powerful in cases where the fear of punishment is impossible, but in direct proportion as the fear of punishment mixes or prevails, the moral character of an act otherwise good is diminished or destroyed. The fear of punishment and the hope of reward are, indeed, auxiliary forces which cannot be dispensed with in society. But we feel that complete goodness and perfect virtue would dispense with them altogether, or rather, perhaps, it would be more correct to say, that the hope of reward would be merged and lost as a separate motive in that highest condition of mind in which the performance of duty becomes its own reward, because of the satisfaction it gives to the Moral Sense, and because of the love borne to that Authority whom we feel it our duty to obey.

The place occupied by this instinctive sentiment in the equipment of our nature is as obvious as it is important. The helplessness of infancy and of childhood is not greater than would be the helplessness of the race if the disposition to accept and to obey Authority were wanting in us. It is implanted in our nature only because it is one of the first necessities of our life, and a fundamental condition of the development of our powers. All Nature breathes the spirit of authority, and is full of the exercise of command. "Thou shalt," or "Thou shalt not," are words continually on her lips, and all her injunctions and all her prohibitions are backed by the most tremendous sanctions. Moreover, the most tremendous of these sanctions are often those which are not audibly proclaimed, but those which come upon us most gradually, most imperceptibly, and after the longest lapse of time. Some of the most terrible diseases which afflict humanity are known to be the results of vice, and what has long been known of some of those diseases is more and more reasonably suspected of many others. The truth is, that we are born into a system of things in which every act carries with it, by indissoluble ties, a long train of consequences reaching to the most distant future, and which for the whole course of time affect our own condition, the condition of other men, and even the conditions of external nature. And yet we cannot see those consequences beyond the shortest way, and very often those which lie nearest are in the highest degree deceptive as an index to ultimate results. Neither pain nor pleasure can be accepted as a guide. With the lower animals, indeed, these, for the most part, tell the truth, the whole truth, and nothing but the truth. Appetite is all that the creature has, and in the gratification of it the highest law of the animal being is fulfilled. In Man, too, appetite has its own indispensable function to discharge. But it is a lower function, and amounts to nothing more than that of furnishing to Reason a few of the primary data on which it has to work—a few and a few only. Physical pain is indeed one of the threatenings of natural authority; and physical pleasures is one of its rewards. But neither the one nor the other forms more than a mere fraction of that awful and imperial code under which we live. It is the code of an everlasting Kingdom, and of a jurisprudence which endures throughout all generations. It is a code which continually imposes on Man the abandonment of pleasure, and the endurance of pain, whenever and wherever the higher purposes of its law demand of him the sacrifice. Nor has this spirit of Authority ever been without its witness in the human Spirit, or its response in the human Will. On the contrary, in all ages of the world, dark and distorted as have been his understandings of Authority, Man has been prone to acknowledge it, and to admit it as the basis of obligation and the rule of duty. This, at all events, is one side of his character, and it is universally recognized as the best.

There is no difficulty, then, in seeing the place which this instinct holds in the unity of Nature. It belongs to that class of gifts, universal in the world, which enable all living things to fulfill their part in the order of Nature, and to discharge the functions which belong to it. It is when we pass from a review of those instincts and powers with which Man has been endowed, to a review of their actual working and results, that we for the first time encounter facts which are wholly exceptional, and which it is, accordingly, most difficult to reconcile with the unities of Nature. This difficulty does not lie in the mere existence of a Being with powers which require for their perfection a long process of development. There is no singularity in this. On the contrary, it is according to the usual course and the universal analogy of Nature. Development in different forms, through a great variety of stages and at different rates of progress, is the most familiar of all facts in creation. In the case of some of the lower animals, and especially in the case of many among the lowest, the process of development is carried to an extent which may almost be said to make the work of creation visible. There are numberless creatures which pass through separate stages of existence having no likeness whatever to each other. In passing through these stages, the same organism differs from itself in form, in structure, in the food on which it subsists, and even in the very element in which it breathes and lives. Physiologists tell us that changes having a mysterious and obscure analogy with these pass over the embryo of all higher animals before their birth. But after birth the development of every individual among the higher orders of creation is limited to those changes which belong to growth, to maturity, and decay. Man shares in these changes, but in addition to those he undergoes a development which effects him not merely as an individual, but as a species and a race. This is purely a development of mind, of character, and of knowledge, giving by accumulation from generation to generation increased command over the resources of Nature, and a higher understanding of the enjoyments and of the aims of life.

It is true, indeed, that this is a kind of development which is itself exceptional—that is to say, it is a kind of development of which none of the lower animals are susceptible, and which therefore separates widely between them and Man. But although it is exceptional with reference to the lower orders of creation it is very important to observe that it constitutes no anomaly when it is regarded in connection with creation as a whole. On the contrary, it is the natural and necessary result of the gift of reason and of all those mental powers which are its servants or allies. But all Nature is full of these—so full, that every little bit and fragment of its vast domain overflows with matter of inexhaustible interest to that one only Being who has the impulse of inquiry and the desire to know. This power or capacity in every department of Nature of fixing the attention and of engrossing the interest of Man,

depends on the close correspondence between his own faculties and those which are reflected in creation, and on his power of recognizing that correspondence as the highest result of investigation. The lower animals do reasonable things without the gift of reason, and things, as we have seen, often involving a very distant foresight, without having themselves any knowledge of the future. They work for that which is to be, without seeing or feeling anything beyond that which is. They enjoy, but they cannot understand. Reason is, as it were, brooding over them and working through them, whilst at the same time it is wanting in them. Between the faculties they possess, therefore, and the governing principles of the system in which they live and under which they serve, there is, as it were, a vacant space. It is no anomaly that this space should be occupied by a Being with higher powers. On the contrary, it would be the greatest of all anomalies if it were really vacant. It would be strange indeed if there were no link connecting, more closely than any of the lower animals can connect, the Mind that is in creation with the mind that is in the creature. This is the place occupied by Man's Reason—Reason not outside of, but in the creature—working not only through him, but also in him—Reason conscious of itself, and conscious of the relation in which it stands to that measureless Intelligence of which the Universe is full. In occupying this place, Man fills up, in some measure at least, what would otherwise be wanting to the continuity of things; and in proportion as he is capable of development—in proportion as his faculties are expanded—he does fill up this place more and more.

There is nothing, then, really anomalous or at variance with the unity of Nature, either in the special elevation of the powers which belong to Man, or in the fact that they start from small beginnings and are capable of being developed to an extent which, though certainly not infinite is at least indefinite. That which is rarely exceptional, and indeed absolutely singular in Man, is the persistent tendency of his development to take a wrong direction. In all other creatures it is a process which follows a certain and determined law, going straight to a definite, consistent, and intelligible end. In Man alone it is a process which is prone to take a perverted course, tending not merely to arrest his progress, but to lead him back along descending paths to results of utter degradation and decay. I am not now affirming that this has been the actual course of Man as a species or as a race when that course is considered as a whole. But that it is often the course of individual men, and that it has been the course of particular races and generations of men in the history of the world, is a fact which cannot be denied. The general law may be a law of progress; but it is certain that this law is liable not only to arrest but to reversal. In truth it is never allowed to operate unopposed, or without heavy deductions from its work. For there is another law ever present, and ever working in the reverse direction. Running alongside, as it were, of the tendency to progress, there is the other tendency to retrogression. Between these two there is a war which never ceases,—sometimes the one, sometimes the other, seeming to prevail. And even when the better and higher tendency is in the ascendant, its victory is qualified and abated by its great opponent. For just as in physics the joint operation of two forces upon any moving body results in a departure from the course it would have taken if it had been subject to one alone, so in the moral world almost every step in the progress of mankind deviates more or less from the right direction. And every such deviation must and does increase, until much that had been gained is again lost, in new developments of corruption and of vice. The recognition of this fact does not depend on any particular theory as to the nature or origin of moral distinctions. It is equally clear, whether we judge according to the crudest standard of the Utilitarian scheme, or according to the higher estimates of an Independent Morality. Viewed under either system, the course of development in Man cannot be reconciled with the ordinary course of Nature, or with the general law under which all other creatures fulfill the conditions of their being.

It is no mere failure to realize aspirations which are vague and imaginary that constitutes this exceptional element in the history and in the actual condition of mankind. That which constitutes the terrible anomaly of his case admits of perfectly clear and specific definition. Man has been and still is a constant prey to appetites which are morbid—to opinions which are irrational, to imaginations which are horrible, and to practices which are destructive. The prevalence and the power of these in a great variety of forms and of degrees is a fact with which we are familiar—so familiar, indeed, that we fail to be duly impressed with the strangeness and the mystery which really belong to it. All savage races are bowed and bent under the yoke of their own perverted instincts—instincts which generally in their root and origin have an obvious utility, but which in their actual development are the source of miseries without number and without end. Some of the most horrible perversions which are prevalent among savages have no counterpart among any other created beings, and when judged by the barest standard of utility, place Man immeasurably below the level of the beasts. We are accustomed to say of many of the habits of savage life that they are "brutal." But this is entirely to misrepresent the place which they really occupy in the system of Nature. None of the brutes have any such perverted dispositions; none of them are ever subject to the destructive operation of such habits as are common among men. And this contrast is all the more remarkable when we consider that the very worst of these habits affect conditions of life which the lower animals share with us, and in which any departure from those natural laws which they universally obey, must necessarily produce, and do actually produce, consequences so destructive as to endanger the very existence of the race. Such are all those conditions of life affecting the relation of the sexes which are common to all creatures, and in which Man alone exhibits the widest and most hopeless divergence from the order of Nature.

It fell in the way of Malthus in his celebrated work on Population to search in the accounts of travelers for those causes which operate, in different countries of the world, to check the progress, and to limit the numbers of Mankind. Foremost among these is vice, and foremost among the vices is that most unnatural one, of the cruel treatment of women. "In every part of the world," says Malthus, "one of the most general characteristics of the savage is to despise and degrade the female sex. Among most of the tribes in America, their condition is so peculiarly grievous, that servitude is a name too mild to describe their wretched state. A wife is no better than a beast of burden. While the man passes his days in idleness or amusement, the woman is condemned to incessant toil. Tasks are imposed upon her without mercy, and services are received without complacence or gratitude, There are some districts in America where this state of degradation has been so severely felt that mothers have destroyed their female infants, to deliver them at once from a life in which they were doomed to such a miserable slavery."[7] It is impossible to find for this most vicious tendency any place among the unities of Nature. There is nothing like it among the beasts. With them the equality of the sexes, as regards all the enjoyments as well as all the work of life, is the universal rule. And among those of them in which social instincts have been specially implanted, and whose system of polity are like the most civilized polities of men, the females of the race are treated with a strange mixture of love, of loyalty, and of devotion. If, indeed, we consider

the necessary and inevitable results of the habit prevalent among savage men to maltreat and dagrade their women,—its effects upon the constitution, and character, and endurance of children, we cannot fail to see how grossly unnatural it is, how it must tend to the greater and greater degradation of the race, and how recovery from this downward path must become more and more difficult or impossible. But vicious, destructive, unnatural as this habit is, it is not the only one or the worst of similar character which prevail among savage men. A horrid catalogue comes to our remembrance when we think of them—polyandry, infanticide, cannibalism, deliberate cruelty, systematic slaughter connected with warlike passions or with religious customs. Nor are these vices, or the evils resulting from them, peculiar to the savage state. Some of them, indeed, more or less changed and modified in form, attain a rank luxuriance in civilized communities, corrupt the very bones and marrow of society, and have brought powerful nations to decay and death.

It is, indeed, impossible to look abroad either upon the past history or the existing condition of mankind, whether savage or civilized, without seeing that it presents phenomena which are strange and monstrous—incapable of being reduced within the harmony of things or reconciled with the unity of Nature. The contrasts which it presents to the general laws and course of Nature cannot be stated too broadly. There is nothing like it in the world. It is an element of confusion amidst universal order. Powers exceptionally high spending themselves in activities exceptionally base; the desire and the faculty of acquiring knowledge coupled with the desire and the faculty of turning it to the worst account; instincts immeasurably superior to those of other creatures, along side of conduct and of habits very much below the level of the beasts—such are the combinations with which we have to deal as unquestionable facts when we contemplate the actual condition of Mankind. And they are combinations in the highest degree unnatural; there is nothing to account for, or to explain them in any apparent natural necessity.

The question then arises, as one of the greatest of all mysteries—how it is and why it is that the higher gifts of Man's nature should not have been associated with corresponding dispositions to lead as straight and as unerringly to the crown and consummation of his course, as the dispositions of other creatures do lead them to the perfect development of their powers and the perfect discharge of their functions in the economy of Nature?

It is as if weapons had been placed in the hands of Man which he has not the strength, nor the knowledge, nor the rectitude of will to wield aright. It is in this contrast that he stands alone. In the light of this contrast we see that the corruption of human nature is not a mere dogma of theology, but a fact of science. The nature of man is seen to be corrupt not merely as compared with some imaginary standard which is supposed to have existed at some former time, but as compared with a standard which prevails in every other department of Nature at the present day. We see, too, that the analogies of creation are adverse to the supposition that this condition of things was original. It looks as if something exceptional must have happened. The rule throughout all the rest of Nature is, that every creature does handle the gifts which have been given to it with a skill as wonderful as it is complete, for the highest purposes of its being, and for the fulfillment of its part in the unity of creation. In Man alone we have a being in whom his adjustment is imperfect—in whom this faculty is so defective as often to miss its aim. Instead of unity of law with certainty and harmony of result, we have antagonism of laws, with results, at the best, of much shortcoming and often of hopeless failure. And the anomaly is all the greater when we consider that this failure affects chiefly that portion of Man's nature which has the direction of the rest—on which the whole result depends, as regards his conduct, his happiness, and his destiny. The general fact is this:—First, that Man is prone to set up and to invent standards of obligations which are low, false, mischievous, and even ruinous; and secondly, that when he has become possessed of standards of obligation which are high, and true, beneficient, he is prone first, to fall short in the observance of the , and next, to suffer them, through various processes of decay, to be obscured and lost.

[7] Malthus, 6th Edition, vol. i., p. 39.

ASTRONOMY.

THE LICK OBSERVATORY.

Work upon Mount Hamilton, the site of the new Lick Observatory, has been pushed forward as rapidly as could be expected, and it is probable that the building will be sufficiently finished to receive a portion of the instruments in the fall of this year. For instrumental equipment, a 12-inch Clark glass and tube, made for Dr. Draper, has been bought, and will be fitted to an equatorial mounting. A 4-inch transit, made on the same patterns as the 4-inch meridian circle of Princeton College, with a few changes introduced by Professors Newcomb and Holden, has been ordered from Fauth & Co., of Washington. It will be sent to California in October, and will probably be mounted by Prof. Holden, and used by him in connection with the 12-inch equatorial, to observe the transit of Mercury on November 7, 1881. A Repsolds meridian circle of six inches aperture will soon be ordered, as well as a small vertical circle. Alvan Clark & Sons, of Cambridge, have received the contract to make a glass three feet in diameter, at a cost of $50,000. The equatorial mounting for this immense objective (44 per cent. more powerful than that ordered for the Russian Government, with aperture of 30 inches, and 100 per cent. more powerful than the great Washington refractor) is not yet provided for. Proposals will be obtained from the principal instrument makers of Europe and this country, and the mechanical part will probably cost as much as the optical.

General plans for the buildings were prepared by Professors Newcomb and Holden, in August, 1880, and will govern the more detailed plans which are to be prepared by the architects. A dome for the 12-inch equatorial is already in process of construction.

The work done upon Mt. Hamilton by Mr. Burnham in the summer of 1879 shows how well suited the high situation is for astronomical observations, and much will be expected from an observatory so well provided with powerful instruments.

"THE 'ASTRONOMISCHE NACHRICHTEN.'—Contrary to what has been lately stated, it appears that this periodical will still be edited by Dr. C. F. W. Peters, who has for some time conducted it, and we are informed there is a probability that Prof. Kruger may set afloat a new astronomical journal under his own management."—*Nature.*

SITE FOR THE NEW NAVAL OBSERVATORY.—The Commission appointed by Congress to select a site for the proposed new Naval Observatory has purchased the Barbour estate, in Georgetown, at a cost of $63,000. A detailed description of the location will shortly appear.

W. C. W.

WASHINGTON, *March* 10, 1881.

We notice, in the last number of the *Chemical News*, that Mr. M. Benjamin, to whom we are indebted for notices of the American Chemical Society, was elected a Fellow of the Chemical Society, London.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

MICROSCOPY.

To the Editor of "SCIENCE:"

Dear Sir:—I am authorized by the President of the American Society of Microscopists to announce to its members, and to all others who may be interested, that the Executive Committee have decided, by an almost unanimous vote, to accept the invitation received from the Tyndall Association of Natural Science, of Columbus, Ohio, and to call the next meeting of the Society at that place on Tuesday, August 9, 1881, (the week previous to the meeting of the American Association for the Advancement of Science, at Cincinnati).

Permit me to add a word upon another matter. The proceedings of the American Society, which should have appeared two months ago, have been unavoidably delayed by circumstances which I shall explain to members at the time of issuing the volume. The latter is now in the press, and will be sent out before the end of the month. ALBERT H. TUTTLE, Sec'y.

COLUMBUS, Ohio, *March* 1, 1881.

BOOKS RECEIVED.

BACTERIA. BY DR. ANTOINE MAGNIN, General Secretary of the Botanical Society of Lyons, &c., &c. Translated by George M. Sternberg, M. D., U. S. A. Boston—Little, Brown, & Company. 1880. Price $2.50.

The present translation of Dr. Magnin's work by Dr. Sternberg will be welcome in all English speaking countries, and we trust its circulation may remove much of the ignorance which exists on this subject, among a large class of professional men, who would perhaps be ashamed to confess their want of knowledge.

Among physicians Dr. Magnin's work on the Bacteria should find a wide range of readers; to many it will read like a revelation, and may be the means of developing original ideas, which may give them a fresh impulse in their profession.

It has been a hard struggle with Nature, accompanied by the greatest difficulties, to solve the many problems involved in the phenomena attributed to Bacteria. One hundred and fifty years have passed since Leeuwenhoek, the Father of Microscopy, wrote the first paper on the subject, and Dr. Magnin occupies thirty-one pages of his work in recording a Bibliography of the works of those who have since contributed papers.

By the aid of this large amount of literature treating on Bacteria, supported by his own experience, Dr. Magnin has produced a work, a careful perusal of which will greatly reduce the difficulties of further investigations in solving the many problems still waiting for solution.

A full classification of the genera and species of Bacteria is given, with sufficient descriptions of their forms and characteristics to make their identification an easy task, and although this classification is merely provisional, its practical utility for student's work is not impaired.

We observe ten full-sized plates of engravings, each having from four to twenty-two illustrations of Bacterian forms.

No person possessing a Microscope should be without this book, and it should be closely studied by every physician.

The temptation is great to enter into a description of the varied contents of the work, but the subject is too intricate to be disposed of in a short paragraph and must be reserved for future treatment.

Bacteria are of all beings the most widely diffused; we meet with them everywhere, in the air, in the water, upon the surface of solid bodies, in the interior of plants and animals. They are the cause of disease, and the great agent in putrefaction, and yet the continuance of life on this globe would not be possible without them; they are so minute that some defy measurement with the highest powers of the microscope, but they become a mighty factor in the economy of creation by reason of their wonderful powers of reproduction, for in twenty-four hours the product of a single bacterium by division amounts to sixteen millions of individuals, and at this rate the ocean itself—calculating it equal to two-thirds of the terrestial surface, with a mean depth of one mile, equalling 920,000,000 cubic miles—would be filled with Bacteria in five days from a single germ, supposing the multiplication to be continued with the same conditions.

Fortunately researches of microscopists have brought to light facts regarding these organisms which enable man to control their prodigious reproductive powers, and our knowledge relating to Bacteria will probably at length be acknowledged as one of the greatest victories of modern science.

NOTES.

A PROCESS FOR THE TOTAL DESTRUCTION OF THE ORGANIC MATTERS IN THE DETECTION OF POISONOUS MINERAL SUBSTANCES.—From 100 to 500 grms. of the suspected matter are mixed in a large porcelain capsule with one-fourth its weight of the acid sulphate of potassa, and then with its own weight of fuming nitric acid. The action is very violent at first, and requires afterwards the aid of a slight heat. Here it is proper to stop if it is merely needful to search for arsenic or antimony. A large excess of pure concentrated sulphuric acid (1.845 sp. gr.) is then added, and the mixture is heated to near the boiling point of the acid. More acid is added from time to time till the mixture becomes pale and limpid. To complete the destruction of the last traces of organic matter it is well to let the liquid cool, add a few crystals of pure potassium nitrate, and heat again till abundant white vapors of sulphuric acid are evolved. The saline mass when cold is dissolved in boiling water, made up to 1 litre, and without previous filtration it is submitted to electrolysis by means of 4 Bunsen elements or a Clamond gas-battery. The negative platinum electrode becomes covered with a grey, blackish, or metallic coating. The action should be prolonged for twenty-four hours. If mercury is suspected a plate of gold should be used at the negative pole instead of platinum. If arsenic or antimony is sought for before the addition of the sulphuric acid, the carbonacious mass is cooled, powdered, and treated with boiling water. The solution thus obtained is examined as proposed by Dr. A. Gautier. (*Comptes Rendus*, August, 1875).—A. G. POUCHET.

DETERMINATION OF CARBONIC ACID IN THE AIR.—The authors, after referring to the discordant results obtained in the determination of atmospheric carbonic acid, describe their method. The carbonic acid is fixed by an absorbent body, from which it is afterwards set at liberty and measured by volume. As an absorbent they use pumice stone saturated with solution of potassa, and contained in a tube drawn out at both ends. The tubes are washed with sulphuric acid, filled with small fragments of pumice, calcined with sulphuric acid, and introduced while hot. The pumice is saturated with a given volume of potassa lye, operating in air deprived of carbonic acid. The lye is prepared by dissolving 1 kilo. potassa in 1.400 litres of water, and adding 200 grms. hydrated baryta to remove sulphates and carbonates. The tubes, prepared beforehand and sealed, are opened at the place of operation, and sealed again after 200 litres of air have been passed through.—A. MUNTZ and E. AUBIN.

RESIDUES FROM THE MANUFACTURE OF OILS FROM SCHISTS. The solid residues serve for the manufacture of alum, and may become an important source of lithia. The acid tarry matters contain sulphates of the bases of the pyridic series, especially of corindine, rubidine, and viridine. Aniline is not sensibly present. The insoluble portions and the alkaline tars contain peculiar phenols, thymols β and γ. There is no ordinary phenic acid, and very little thymol α.—GASTON BONG.

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SWINE PLAGUE.

The present discussion on the infectious disease existing among hogs in the United States, known as the "*Swine Plague*" will, we trust, be productive of some good in giving publicity to certain facts relating to this subject, which should be known and understood by all interested in the sale or consumption of pork.

It appears that a report was sent from the British Consulate at Philadelphia, to England, stating that 700,000 hogs had died of Swine Plague during the year 1880, in one of the Western States.

Those interested in the export trade have contested this statement, and with the very laudable motive of protecting an important American home interest, have endeavored to show that the action of the Consul was founded on erroneous information, and one journal in New York even accuses the British officials of "plotting a senseless scare."

It appears to be now officially admitted that 300,000 hogs died of this disease in one State alone in 1880; and, therefore, the real question now at issue, is not whether the disease exists, but merely how many hundred thousand hogs die in consequence of it annually in each State.

Without going outside of United States official documents the real facts of the case may be stated as follows:

The Swine Plague came into notice about 25 years ago, and on account of its excessive infectious nature, it steadily increased annually until the year 1878, when the Commissioner of Agriculture announced an annual death-rate of hogs for the United States, amounting to a money value of $20,000,000; as the victims are said to be chiefly among the smaller and leaner animals, probably $2 per head would be a fair average of value; in that case the number of deaths among hogs by the Swine Plague, actually taken by census, would be 10,000,000 for that year.

As this disease is no sudden epidemic, but has been progressing for a quarter of a century, it is not likely that, in the two years and a half which have passed since this report was made to the United States Government, the disease has much abated. The disease is at this date officially admitted to be raging, and the mere question of its destructive effects, is only one of degree.

Under these circumstances it would appear unjust to accuse foreign consuls of partial conduct in reporting these facts, and it is equally futile to attempt to suppress them.

The behavior of the New York Produce Exchange in this matter reminds us of the action of the ostrich when it buries its head in the sand at the approach of danger. We have one word of advice to those who would preserve the United States export trade in pork, and that is to admit the existence of Swine Plague, and the increasing contamination of pork by trichinæ. This done, it is not difficult to organize such a system of inspection as will satisfy foreign governments that the shipments of pork from this country are such as can be received with safety. At the date of our writing, a cable dispatch announces that the Austrian Government has interdicted the importation of American pork in any form, and unless our suggestion is accepted without delay, other foreign States will probably follow the example of Austria on this question.

Major J. W. Powell succeeds Clarence King as Director of the United States Geological Survey. This appointment appears to have given general satisfaction, and we consider it a fortunate circumstance that a gentleman of such high professional attainments has accepted this important position.

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA.

A course of practical instruction in Invertebrate Paleontology, to be given under the auspices of the Academy of Natural Sciences of Philadelphia, was inaugurated by Professor Angelo Heilprin, on Tuesday, March 8th, 1881, at 8 P. M., in the Hall of the Academy.

The lectures, twenty-five in number, will be continued on the successive Fridays and Tuesdays of each week, from 4 to 5 o'clock, P. M.

The plan of instruction will embrace the examination of the life-histories of the various geological formations, the discussion of the biological relations of past organic forms, and the practical determination of these forms for the purposes of paleontological inquiry. The demonstrations will be of an essentially practical nature, and will be based upon a careful study of the resources of the Academy's collections.

A course of practical instruction in Mineralogy was also inaugurated by Professor Henry Carvill Lewis, at the Academy of Natural Sciences of Philadelphia, on Tuesday, March 15th, at 8 P. M. The lectures will be continued on successive Mondays and Thursdays at 4 P. M., beginning March 21st.

The course will consist of ten lectures, and will be in great part practical, and confined to Determinative Mineralogy. Blowpipe analysis, and the application of simple chemical tests to the determination of minerals, will be especially dwelt upon. Students will be expected to devote at least half the time to the performance of practical work in this department.

The course will also embrace a reference to Physical and Crystallographic Mineralogy, and to Mineralogical classification.

Application for admission to the above courses may be made to Henry McCook, Chairman of the Committee on Instruction and Lectures.

A PARTIAL REVISION OF ANATOMICAL NOMENCLATURE, WITH ESPECIAL REFERENCE TO THAT OF THE BRAIN.*

By Burt G. Wilder, M. D., Professor of Comparative Anatomy, etc., in Cornell University, and of Physiology in the Medical School of Maine.

I.

INTRODUCTORY.

During the preparation of a paper "On the Gross Anatomy of the Brain of the Domestic Cat (*Felis domestica*)," I have been led to believe that some advantage may be gained by certain modifications of the current anatomical nomenclature. The present article contains suggestions, chiefly of a practical nature, which I wish to submit to other anatomists in the hope that, even if the changes here indicated do not meet their approval, they will be induced to take the general subject into consideration.

That the nomenclature of a science is worthy of attention is indicated by the care bestowed upon the language of modern chemistry and mathematics, and by the following expressions of opinion:

"Everything in science ought to be real, ingenuous and open; every expression that indicates duplicity, or equivocation, reservation, wavering or inconsistency, is a reproach to it."—Barclay, A., 89 †

"Questions of definition are of the very highest importance in philosophy, and they need to be watched accordingly." Duke of Argyll, 1.

"In all sciences, nomenclature is an object of importance; and each term should convey to the student a definite meaning." Dunglison, A, Preface.

"There is a necessity for perfect definiteness of language in all truly scientific work." P. G. Tait, 1.

"Technical terms are the tools of thought." ‡

"Only an inferior hand persists in toiling with a clumsy instrument, when a better one lies within his reach. . . . . . A single substantive term is a better instrument of thought than a paraphrase." Owen, A, I, Preface, pp. xii, xiv.

"As morphology deals with forms and relations of position, it demands a careful selection of terms and a methodical nomenclature." Goodsir, A, II, 83.

These remarks apply to the general subject of anatomical nomenclature. But the terms employed by anatomists form two divisions: those which indicate the *position* or *direction* of organs, and those by which the organs themselves are designated. Since, also, writers have usually treated of them separately, it will be convenient here to consider anatomical *toponomy* and *organonomy* under distinct headings.

TERMS OF POSITION AND DIRECTION—TOPONOMY.

Dr. Barclay's volume had especial reference to this division of the subject, and its key-note is struck in the following paragraph (A, 5):

"The vague ambiguity of such terms as superior, inferior, anterior, posterior, &c., must have been felt and acknowledged by every person the least versant with anatomical description."

Dunglison admits (A, 61) that "Great confusion has prevailed with anatomists in the use of the terms before, behind, &c." Dr. Spitzka has forcibly stated (1, 75, note 1) the objections to the use of anterior, &c., and their unsuitability is tacitly conceded in the employment of other terms by several writers who do not explicitly condemn the current toponomy: Gegenbaur (A, 491), Mivart (A, 69), Cleland (1, 170), Rolleston (B, 33, note), &c.

Finally, the need of a radical change of base has been proclaimed in one of the very strongholds of anthropotomy:

"Now that the more extended study of comparative anatomy and embryonic development is largely applied to the elucidation of the human structure, it is very desirable that descriptive terms should be sought which may, without ambiguity, indicate position and relation in the organism at once in man and animals. Such terms as cephalic and caudal, dorsal and ventral, &c., are of this kind, and ought, whenever this may be done consistently with sufficient clearness of description, to take the place of those which are only applicable to the peculiar attitude of the human body."—Quain, A, I, 6.

This is certainly explicit as to the principle involved, and it is to be hoped that later editions of this standard Human Anatomy may display its practical application to the body of the work.

How slender is the justification for retaining a toponomical vocabulary based upon the relations of organisms to the surface of the earth, appears more fully when we reflect that the assumed standard, for the higher vertebrates at least, is man in his natural erect attitude; yet that both man and animals are more often examined and compared when lying upon the *back*, this being an attitude truly characteristic of only that infrequent "subject," the sloth.

As a single illustration of the logical inconsistencies into which we are led by the use of the current toponomy, let us take the series of possible designations of the direction of some vertebral spinous process which projects toward the skin of the back at, or approximately at, a right angle with the myelon. With man the direction in which it points is *posterior*, but with a cat it is *superior*, while with an ape or a bird it is somewhere between the two; with all four, when on the dissecting table, it would be usually *inferior*. Finally, with a flounder the corresponding direction would be *horizontal* or *sidewise*.

In short, to designate the locations of organs by the relation of animals to the surface of the earth, which relation differs in nearly allied forms, and varies with the same individual according to circumstances, is as far from philosophical as it would be to define the place of a house or a tree by reference to the planet Jupiter, or to assume that mankind naturally face the rising sun, and hence to designate our right and left as the south and north sides of the body.

Some practical points respecting this division of the subject will be presented farther on.

DESIGNATION OF ORGANS,—ORGANONOMY.

There are probably few investigators or teachers of comparative anatomy who have not been impressed, in some degree, with the desirability of some modification of the prevailing nomenclature of organs,—the "bizarre nomenclature of anthropotomy," (Owen, A, II, 143)—based as it is upon the peculiar features of the human body, which has been fitly characterized, from a morphological point of view, as "not a model, but a monstrosity."

This impression may give rise to special papers, like those of Owen, (166), Maclise (1), and Pye-Smith (1, 16), or simply to more or less extended remarks upon the subject, with or without the use or presentation of new terms.

* This article is based upon two communications: the one, "A Partial Revision of the Nomenclature of the Brain," was read at the Boston meeting of the American Association for the Advancement of Science, August 28, 1880, and was reported, in part, in the *Boston Daily Advertiser*, of August 30, and in the New York Medical Record for September 18th, 1880; the other, "On some Points of Anatomical Nomenclature," was read at a meeting of the Cornell Philosophical Society, Ithaca, N. Y., January 15, 1881.

† In the List of Works and Papers at the end of this article, the names of the *authors* are placed in *alphabetical order*. The titles of *separate works* are designated by *letters*, and their order has no significance. The titles of *papers* are *numbered*. In the case of papers published between 1800 and 1873 the numbers correspond to those in the chronological "Catalogue of Scientific Papers published by the Royal Society of London." In other cases the numbers are only provisional, and are printed in italics.

The references are made as follows: the name of the author is given first, unless the author has been indicated already; then follows the letter or the number by which the title of the work or paper is designated upon the list; if a Roman numeral is given it denotes the number of the volume; and the last number is that of the page. This system of references was followed by me first in 1872, in the paper entitled Intermembral Homologies (10), and has been since adopted by others.

‡ I have mislaid the reference to the source of this aphorism. Perhaps some of my readers can supply it.

In the Preface to his "Anatomie du Chat" (A, pp. xiv—xvii), Straus-Durckheim devotes several pages to a discussion of anatomical nomenclature, and the body of the work contains many original names. Professor H. S. Williams calls attention (A, Preface), to the "crying need of a standard and uniform nomenclature of comparative anatomy."

In the preface to their recent account of the morphology of the skull (A), Parker and Bettany say: "It has been attempted to narrate the facts by means of a consistent terminology, amplifying what Prof. Huxley has so admirably developed." Several of Huxley's papers (as 70), contain new terms, most of which have been generally accepted, and in a greater or less degree the same is true of the elder Agassiz (A), Gegenbaur (59), Hæckel (A), Marsh (1), and others.

That my own consideration of the subject is not wholly of recent date may be seen from the papers numbered 10 and 2.

SCOPE AND METHODS OF THIS REVISION.

Most of the toponomical terms here discussed have a general application. But a revision of the organonomy of the entire body would extend this article beyond desirable limits.

As stated by Pye-Smith (1, 162), "the nomenclature of the brain stands more in need of revision than that of any other part," and on the present occasion I will simply endeavor to remove, in some degree, the deficiency implied in the following words of the French editors of "Huguenin" (A, Preface):

"That which is demanded of anatomy is an exact nomenclature and determination of the parts of the brain in their relative positions and contiguity, and if possible in their continuity."

Doubtless, for the entire comprehension of its functions, and even for the final determination of some of its homologies, the vertebrate brain should be fully understood in respect to the disposition of its cellular and fibrous elements,—that which the writers just mentioned term its *continuity*. But whoever is at all familiar with the literature of encephalic histology, or who has undertaken for himself the exhaustive study of even a very limited part of the brain will, if of sincere mind, admit the present impossibility of fairly discussing the microscopical terminology of the organ within the limits of a single article.

With the gross anatomy of the brain, the case is somewhat different. In the first place, some knowledge of it is requisite as a foundation for the histological enquiry, as well as for general work in human or comparative anatomy, physiology, and pathology. Secondly, the parts which are distinguishable by the naked eye are comparatively few, and while the numerous errors which may be found in even standard works sufficiently attest the difficulties of encephalotomy, its methods are comparatively simple. It is to be hoped, however, that the microscopical terminology and synonymy of the brain may shortly find due treatment.

A recent paper is entitled by its authors: "A *Reformed* System of Terminology, etc." Now the word *reform* is generally associated with questions of ethical improvement; whereas terminological reforms involve no other principle than that of expediency, taking into the account, however, the future as well as the present and the past. Such moral truisms as "do right because it is right" have no counterparts in considerations of scientific nomenclature, and he who, affected by the *cacoethes reformandi*, insists upon reform for the sake of an ideal perfection, is apt to appear as nothing better than a troublesome and useless pedant.

In the place, then, of what otherwise might be styled the principles of terminological reform, I will enumerate briefly the objects of the present revision, the considerations upon which it is based, and the methods which have been pursued :—

To facilitate the acquisition and communication of accurate anatomical knowledge, by rendering the vocabulary equally applicable to all vertebrates, and equally intelligible to all nations.

That the test of the accuracy and completeness of a description is, not that it may assist, but that it cannot mislead.

To include in this vocabulary, so far as practicable, only such terms as are brief, simple, significant, of classical origin, and capable of inflection.

To propose as few changes as possible, and to introduce new names only for parts apparently unknown or unnamed before (*e. g., crista fornicis*), or in the place of semi-descriptive appellations undesirably long or incapable of inflection, as *e.g., cimbia* for *tractus transversus pedunculi*, *porta* for *foramen Monroi*.

To consider *brevity* as an especially desirable characteristic of such names as are most frequently employed.

When a part is known by a descriptive phrase, to select therefrom some characteristic word as the technical designation; *e. g., iter* (*a tertio ad ventriculum quartum*).

When two or more parts are similar, or have similar relations, to distinguish them by joining to some common title already in use, prefixes indicative of their relative positions; *e.g., postgeniculatum, prægeniculatum*.

To shorten the names of several parts by omitting the word *corpus*, and using the neuter adjective as a substantive.

To keep modern usage, and the rules of classical etymology constantly in mind, but not to be hindered thereby from the employment or even the formation of terms which are eminently desirable from the practical standpoint.

To discard terms which indicate *size*, those which refer to the *natural attitude* of man or animals, most *vernacular* names, and all names of the reproductive organs which have been applied needlessly to other parts of the body.

With regard to the point last-named, while it may perhaps be urged in extenuation that the *patres anatomici* entertained a notion as to the representation of the entire organism in the brain, some of their words certainly indicate an entire freedom from apprehension that the mysteries of encephalic anatomy ever would be discussed by ordinary mortals, much less by women, or under circumstances requiring propriety of speech.

As has been stated, and as will be exemplified in the vocabulary, I have placed great stress upon *brevity* as a desirable characteristic of anatomical terms. So long as the study of anatomy was nearly confined to members of the medical profession, they being comparatively few in number, and, by ancient tradition at least, not wholly averse to clothing their discourse in a sesquipedalian garb impenetrable to the vulgar eye, it mattered little whether the statement of a given fact or idea required one minute or five. But now, thanks to the popular writings of Agassiz, Dana, Gray, Darwin, Hæckel, Huxley, Owen and others, in so far especially as they have aroused a personal interest in the problems of evolution, natural history instruction is given systematically in all schools and colleges, and the time seems to have come when, in the words of the naturalist first-named, "Scientific truth must cease to be the property of the few; it must be woven into the common life of the world." It is probable, indeed, that those who employ anatomical language to a greater or less extent at the present day are at least one hundred times as numerous as when Dr. Barclay's praiseworthy effort at reform was received with indifference or opposition.

It may be asked: In the face of this rapid popularization of anatomical knowledge is it worth while to introduce, or even to retain, any purely technical terms?

Apparently some German scientists have determined upon a negative reply to this inquiry, and their papers, even those of strictly scientific nature, teem with vernacular words, and with compounds thereof fearfully and wonderfully made.

If this kind of verbifaction be tolerable under any circumstances, it certainly would be justified by the extent and importance of the contributions to knowledge which appear first in the German scientific periodicals.

Upon this point, however, I can do no better than to quote the very recent judgment of one who is at the same time an investigator, a promoter of "the diffusion of knowledge," and an admirer of the methods and results of German science:

"Every art is full of conceptions which are peculiar to itself; and, as the use of language is to convey our conceptions to one another, language must supply signs for those conceptions. Either existing signs may be combined in loose and cumbrous paraphrases, or new signs, having a well-understood and definite signification, may be invented. Science is cosmopolitan, and the difficulties of the study of zoology would be prodigiously increased if zoologists of different nationalities used different technical terms for the same thing. They need a universal language; and it has been found convenient that the language shall be Latin in form, and Latin or Greek in derivation."—Huxley, C, 14.

Unless it can be shown that there is an essential distinction between the methods of designating entire organisms, and the parts thereof, the foregoing passages should silence the objections of those who would have us retain a vocabulary as vague as was that of chemistry in the days of lime, vitriol and copperas—a vocabulary which combines the ponderous stiffness of the cloister with the puerile vagueness of the nursery.

Tuberculum bigeminum anterius must give way to *lobi optici*, or some even shorter term; while *trachea* must take the place of *windpipe, weasand, luft-rohre* and *conduit ærien*. Life is too short to spend in digging for truth with a long-handled shovel when a trowel will serve the purpose; nor is it becoming that any nation, however wise and great, should ask all the rest to take their intellectual food with chop-sticks of its peculiar pattern.

That there is no inherent obstacle to the employment of technical terms of classical derivation is shown by the readiness with which such words as *petroleum* and *phylloxera* have become domesticated along with the objects which they represent. There are scores of animals, like the *Rhinoceros, Hippopotamus*, and *Ichneumon*, for which there are no English vernacular names; while the youngest student of botany accepts *Hepatica, Anemone*, and even *Rhododendron* without difficulty or hesitation. Homely as it sounds, *stomach* is a strictly classical word, and the use of *caul* for *omentum*, or *sweetbread* for *pancreas*, would surprise a class in elementary physiology.

Even the late Jeffries Wyman, who saw no objection to *forearm*, and used *near* rather than *proximal* for the first row of *carpalia*, accepted *intermembral* as "good," and freely employed, if indeed he did not originate, the adjective *pretibial*, which probably would have come into general use had not the bone in question proved to be the homologue of the *intermedium*.—(Morse, 18, 13).

THE LIMITS OF TERMINOLOGICAL CHANGE.

As has been stated already, the modifications here proposed are intended to provide for what seem to be actual necessities, irrespective of purely theoretical considerations, and of any desire for a perfectly uniform and consistent terminology. It may be well, however, to specify certain general limitations to changes of anatomical nomenclature.

Priority is practically of little moment in respect to the names of organs, since it is usually difficult to ascertain when and by whom they were first applied. An example of this is afforded by the phrase *foramen of Monro*, (Wilder, 3). Nor, indeed, has priority always been held sacred in systematic zoology. Owen's "Deinosaurians" was proposed nine years later than von Meyer's "Pachypoda;" yet, as stated by Huxley (108, 33), it has been retained, notwithstanding the small size of some members of the group.

Etymological appropriateness is sometimes disregarded, as in the case just mentioned, and in the more familiar names *Reptiles, Vertebrates, Edentates*, &c. Prof. Huxley has recently expressed the common sense view of the matter as follows:

"If well understood terms which have acquired a definite scientific connotation are to be changed whenever advancing knowledge renders them etymologically inappropriate, the nomenclature of taxonomy will before long become hopelessly burdened." (B, 751.)

So, too, the names of organs have sometimes been given in reference to some variable or unessential character, or have even represented an erroneous idea; yet no one now thinks of discarding either *rectum, arteria*, or *carotid*.

Sometimes even brevity and etymological accuracy yield to established usage. The word *cubitum*, proposed by me in 1872 (10, 21) as the technical equivalent of *forearm*, is both shorter than *antebrachium*, and more in accordance with its classical employment; but the latter word seems to be more generally preferred, and I am ready to accept it.

In another case, even though a new term has not yet come into general use, a special vitality may be imparted to it by the authority of those who may have adopted it. No marked or persistent disfavor is likely to be shown to terms which, like *myelon*, can claim Prof. Owen as father, and find a god-father in Prof. Huxley.

MESON, ITS DERIVATIVES AND CORRELATIVES.

The present tendency of accurate anatomical description is to refer the position or direction of all parts and organs to an imaginary plane dividing the body into approximately equal right and left halves; hence it is desirable to designate this middle plane, or any line contained therein, by a word which is at once significant, short, and capable of inflection. Dr. Barclay proposed *mesion*, and *mesial* has been generally used; but would it not be better to adopt the very term employed by the Greeks to signify the middle, *meson*, τό μέσον, equivalent to the more ponderous Latin *meditullium*? The corresponding adjective is *mesal*, and the adverb *mesad*, while in combination it becomes *meso*.

The following general terms were also proposed by Barclay, and have been more or less systematically employed by Owen, Huxley and others: *Dorsal, ventral, dextral sinistral, lateral*, with the corresponding adverbial forms *dorsad*, etc. Should the alleged correspondence of the ventral region of the vertebrate with the tergal region of the arthropod prove to be one of true homology, it may be desirable in time to discard *dorsal* and *ventral* for more suitable terms, but for the present, if on practical grounds alone, it seems well to retain them.

CEPHALIC AND CAUDAL.

Barclay proposed *atlantal* and *sacral* for the designation of the position of parts lying toward the head or the tail in reference to an imaginary plane dividing the trunk at the middle of its length. But these terms were not applicable to parts beyond the atlas and the sacrum, so that new words were applied to the regions of the head. Perhaps this needless complication has hindered the general adoption of Barclay's nomenclature notwithstanding its many admirable features. At any rate, *cephalic* and *caudal* are much more acceptable terms, and are practically unobjectionable, although certain theoretical difficulties readily suggest themselves.

Proximal and *distal, central* and *peripheral* are in common use, and the general employment of their inflections and derivatives is only a question of time.

Ental, and *ectal* are here first proposed as substitutes for the more or less ambiguous words *inner* and *outer, interior* and *exterior, deep* and *superficial, profound* and *sublime*. Derived respectively from ἐντός and ἐκτός their significance is obvious, while their brevity and capacity for inflection will probably commend them to accurate working anatomists.

DESIGNATION OF THE REGIONS OF THE LIMBS.

Barclay's terms *ulnar, radial, tibial* and *fibular* refer to only two of the four aspects of each limb. Prof. Huxley has made the very important suggestion that, for comparison, all vertebrate limbs be regarded as placed in a *uniform normal position;* they are then extended laterad at right angles with the meson, with the convexities of the knee and elbow directed dorsad. Each limb then presents not only a proximal and a distal portion, but four general aspects, *dorsal, ventral, cephalic*, and *caudal*. Hence there appears to be no need for the introduction of the new terms employed to some extent by Huxley and other English anatomists, *epaxial, hypaxial, preaxial*, and *postaxial*. These words are also liable to misconception because *axial* has been used already in reference to not only the axis vertebra, but also the entire skeleton of the trunk as contradistinguished from that of the limbs.

DESIGNATION OF CURVATURES.

Ordinary descriptions of the directions of curvatures are apt to be ambiguous, and Huxley resorts to the phrase "arcuated outwards" to indicate the form of the mandibular rami of the Balænoidea. Since the Latins designated the two malformations of the legs, "knock-knee" and "bow-legs," by the words *varus* and *valgus* respectively, we may find it convenient to speak of parts whose convexities look mesiad as *varate*, and of those whose convexities look laterad as *valgate?* In other cases, however, and perhaps even in these, so long as there is any opportunity for misapprehension, it will be well to describe curvatures as *presenting a convexity* in one or another direction. For instance, the mandibular rami of the Balænoidea present a *laterad convexity*, while those of the Physeteridæ are convex toward the *meson*.

HYPOCAMPA.

This is employed by Vicq D'Azyr in the descriptions of the plates of his Traité D'Anatomie, published in 1786. The more common form *hippocampus* occurs in the list of anatomical terms in the same volume, but this may have been compiled partly by others, while the descriptions are obviously the work of the anatomist himself. Vicq D'Azyr does not discuss the etymology of the term, but says the "grande hypocampe" was first mentioned by Arantius and Varolius, whose works are not now accessible to me. Even Hyrtl does not seem aware of the use of the word by Vicq D'Azyr, and all other writers, so far as I know, make it *hippocampus*.

If the original orthography cannot be ascertained, *hypocampa* is to be preferred on etymological grounds; the ridges known as *hippocampus major* and *h. minor* bear no obvious resemblance to the fish known to the ancients as ἱππόκαμπος and *hippocampus*, but the larger of the two, which probably first received the name, does certainly present a most notable *downward curvature*, such as the Greeks might have designated by ὑποκαμπή.

DESIGNATION OF THE ENCEPHALIC CAVITIES.

As based upon the condition of things in man the current nomenclature of the ventricles had some slight foundation. But, in the light of better methods and more accurate knowledge, it appears incongruous and needlessly perplexing.

Let the learned anatomist lay aside his familiar acquaintance with the parts and their names, and put himself in the place of the beginner who, after gaining a general idea of the arrangement of the vertebrate brain from a frog or menobranchus, is trying to master the complexities of the mammalian organ from the brain of the cat, dog or sheep.

Leaving the myelon, he finds the *canalis centralis* expanding into a cavity which, although the first of the series, is called the *fourth* ventricle. The more or less distinct cavities corresponding to the cerebellum and the *lobi optici* are not called ventricles at all, and the *third* is between the thalami. The two "lateral" ventricles are rarely mentioned as the *first* and *second*, but since the numbers must be understood in order to account for the *third* and *fourth*, the student desires, in vain, to know which is the first and which the second. In point of fact, if the enumeration is begun at the cephalic end of the series, the lateral ventricles are the third and fourth, since there are well-developed ventricles in the *lobi olfactorii*. Finally, a "*fifth* ventricle" is mentioned, which is not only at the greatest distance from the fourth, but has no normal connection with the other ventricles, and is, in fact, no part of the series.

In view of all this, the task of describing to students the highways and by-ways of the brain,—which should be most attractive because therein is most clearly manifested the ideal arrangement of the organ,—is one from which I shrink as from any other kind of solemn nonsense. To my mind, indeed, rather than go on as we have been going, it would be at once more philosophical and more intelligible to adopt the simple vocal device employed by Straus-Durckheim for the designation of the metatarsalia—"padion, pedion, pidion, podion, pudion"—and to re-christen the ventricles by, for instance, the names *pran, pren, prin, pron*, and *prun*.

Fortunately, however, another alternative is presented. Whatever objections may be urged against them on theoretical grounds, a real practical advantage is gained by the use of the terms *rhinencephalon, prosencephalon, diencephalon, mesencephalon, epencephalon*, and *metencephalon*, and their German or English equivalents are likewise often employed for the designation of the general regions of the brain. Assuming that these terms are to be retained, and that they are to be learned by successive generations of students, why should we not transfer the distinctive prefixes to the Greek word for ventricle, *cœlia*, κοιλία? This would give us *rhinocœlia, procœlia, dicœlia, mesocœlia, epicœlia*, and *metacœlia*.

These terms are capable of inflection, and the longest of them is no longer than the Latin *ventriculus*, which requires a prefix or qualifying word. Lastly, but by no means of least importance, they correspond with the names of the encephalic segments. As will be seen in the list of names of the parts of the brain, these prefixes are employed for the designation of the membraneous roofs of the "third" and "fourth" ventricles, and the plexuses of these and the lateral ventricles. After a somewhat prolonged consideration of the matter, it seems to me that the practical usefulness and logical consistency of these new terms outweigh any objections that may be urged, and that these latter are less numerous and serious than could be brought against any other substitutes for the present heterogeneous and ill-applied nomenclature.

Two or more ventricles may be spoken of as *cœliæ*, while the "fifth" may be called *pseudo cœlia*. I hope, before long, to justify more fully the proposition already made* to consider the cephalic portion of the "third" between the *portæ* (foramina Monroi), as a morphologically independent cavity under the name of *aula*.

RHINEN, ETC.

May not *rhinen., prosen., dien., mesen.* and *epen.* be written, for the sake of brevity, for the full titles of the general divisions of the brain, *rhinencephalon, prosencephalon*, etc?

* Proceedings of the Am. Assoc. for Adv. of Science, Aug. 25, 1880 reported in "New York Medical Record."

The following abbreviations are printed in Webster's Dictionary without the period: *etym(on)*, *demirep(utation)*, *grog(ram)*, *hyp*,* and *hypo(chondria)*, *noncon(tent)*, *hyper(critic)*, *navvy* for *navigator*; but the abbreviations above suggested should probably be followed by the period.

PRÆCOMMISSURA, ETC.

The single words ***præcommissura***, ***medicommisura***, and ***postcommissura*** are proposed as substitutes for the compound terms *commissura anterior*, *medius*, and *posterior*, and for their English equivalents. A similar change is desirable in the case of the three cerebellar peduncles, which may be more conveniently termed ***præ- meso-*** and ***postpedunculus***. So, too, the *corpora geniculata* (*external and internal*) may be called ***prægeniculatum*** and ***postgeniculatum***; the *brachia* of the mesencephalon become ***præbrachium*** and ***postbrachium***, and the two "perforated spaces," ***præperforatus*** and ***postperforatus***. The "anterior pyramids" have been called by Owen "prepyramids," but more exact designations of these and of the "posterior pyramids" would be ***ventripyramides*** and ***dorsipyramides***.

The prefixes are usually employed when the object referred to lies before, between, or behind other objects of a different kind; *e. g.* ***præcordia***, ***mediterraneus***, and ***posterganeus***. The use here proposed is as if three dogs in line were designated by ***præcanis***, ***medicanis*** and ***postcanis***. If the terms are objectionable, what can be substituted for them? They are certainly as legitimate as are the well-established terms ***prosencephalon***, ***mesencephalon*** and ***metencephalon***. Do not the English words ***preposition*** and ***postposition*** offer some analogy?

The following points are mainly etymological and orthographical rather than anatomical.

THE CONNECTING VOWEL.

With derivative words the connecting vowel is commonly ***i***; ***e.g. alipes, claviger, fatifer, fidicen, fluctigena, decimanus, neurilemma***, and ***xiphisternum***. But classical exceptions are ***mulomedicus, quadrupedus, noctuvigilus***, and ***decumanus***. In common English and scientific terms of Latin or Greek origin the *o* is common; *e. g.* ***ambodexter, burgomaster, gastrotomy, termonology, ventroinguinal, lateroflexion, mucopurolent, vasomotor, curvograph, neuroglia, oculospinal, pleuroperitoneal, xiphosura, septopyra, hemoglobin, cephalotribe***, etc. Rarely is it ***e*** as in *venesection*.

Should the *i* or the *o* be used in the following terms: ***Dorsimeson, ventrimeson, dorsicumbent, latericumbent, dextriflexion, sinistriversion, cephaloduction, caudiduction***, etc.? Both analogy and euphony lead one to use the ***i*** when the first part of the word is of Latin origin, and the ***o*** with the Greek.

Should any of these terms be written as compound words?

COMPOUND WORDS.

The two Latin compounds known to me are *veneri-vagus* and *vesti-contubernium*. The following common or technical English compound words are selected from Webster's English Dictionary, or the Medical Dictionaries of Dunglison, or Littré et Robin, or from the writings of Barclay, Humphrey, and Straus-Durckheim: *Anglo-Saxon, concavo-convex, dextro-gyrate, ventro-appendicular, costo-vertebral, costo-alaris, caudo-pedal, osseo-cutaneous, occipito-scapularis, dorso-lateral, sterno-clavicular, clavo-cucullaire, clavi-sternal, clavio-humeralis*. By analogy with the foregoing, compound terms of direction should read *dorso-ventral, caudo-cephalic, meso-lateral, sinistro-cephalic*, etc.

HYBRID WORDS.

Some of the terms already mentioned are formed by the union of Latin with Greek words; *e. g.*, *dorsimeson, meso-lateral*, and *caudo-cephalic*; several others are likely to be employed; *e. g. clavo-mastoideus*, and *felitomy*.

Beyond the occasional intimation, in the dictionaries, that a term is hybrid, the subject seems to be ignored, and it might fairly be inferred that literary authorities entertain one or the other of two opposite convictions: either mongrel words are verbal monstrosities which will be shunned instinctively by all well-regulated minds, or there is no more serious objection to their use, or even their creation, than to the employment, or even the production, of mules, or the mixed varieties of grapes and roses.

However this may be, the fact is that the Latin and the Greek tongues have united to form the following nine hybrids which may be found in Latin writings: ***anticato, biclinium, cryptoporticus, dentarpaga, epitogium, monosolis, monoloris, pseudo-flavus***, and ***pseudo-urbanus***. Of these, the third only occurs with any degree of frequency.

Whoever will spend the time to look through an unabridged dictionary of the English language—and the interest as well as the instructiveness of such a search can hardly be realized by those who use the volume only for occasional reference—will find that, after excluding the twenty-five or more words ending with ***meter***, which may perhaps be derived directly from the Latin form ***metrum***, there are more than ***one hundred hybrid*** words, many of them in good standing. Many more are to be gleaned from the dictionaries of medicine and the other arts and sciences.

Nevertheless, it is probable that a due regard for the feelings of the classical purists in whose eyes language was not made for man, but rather man for language, will lead scientists to refrain from the introduction of mongrel terms when others will serve the purpose, and the present writer will be pleased to receive suggestions leading to the substitution of wholly unobjectionable words for any of the hybrids which have been mentioned.

(*To be continued in our next.*)

ON CHICKEN CHOLERA: STUDY OF THE CONDITIONS OF NON-RECIDIVATION AND OF SOME OTHER CHARACTERISTICS OF THIS DISEASE.*

By M. L. Pasteur.

II.

Concerning the properties of the extracts of the artificial cultivation of the germ of chicken cholera, an inquiry presents itself. We have shown that these extracts contain no substances capable of preventing the cultivation of the germs of this disease. They might, however, contain elements adapted to the vaccination of chickens. To investigate this point I have prepared cultivations where volume was not less than 120 c.c. After filtration and evaporation at a low temperature, while infinite care has been taken that its purity should not be affected, this liquid has given a dry extract, which was re-dissolved in 2 c.c. of water, and the totality of this was injected under the skin of a chicken which had never had chicken cholera. A few days afterwards the chicken, after being inoculated with a virus of the greatest virulence, died with the usual symptoms of ***unvaccinated*** chickens.

This class of experiments led to the following observation, which is of the greatest importance in physiology. When the extract from the cultivation of the germ of this disease, corresponding to an abundant development of the parasite, is injected under the skin of a fresh chicken in perfect health, the following phenomena take place: At first the chicken seems to suffer from a nervous dis-

*Translated from the *Comptes Rendus de l'Academie de Sciences*, of May 3d, 1880, by P. Casamajor. The translation of the second paper of this series appeared in the *Chemical News*, vol. xlii., page 321 (December 31, 1880).

order, which is indicated by panting breath and alternately opening and closing its beak; afterwards it becomes motionless, assumes the shape of a ball, refuses food, and seems overcome with drowsiness, as is the case with chickens affected with the disease. There is this difference, however, that the chicken wakes up at the least noise. This sleep lasts about four hours, after which the chicken wakes up, looks as well as usual, eats and cackles as if nothing had happened to it.

I have repeated this experiment several times and have always observed the same facts. Before injecting the extract above mentioned, I took, in every case, the precaution of injecting an extract of the pure chicken broth, which does not cause analogous phenomena. I have, by this means, acquired the conviction that, during the life of the parasite, a narcotic is formed, and that it is this narcotic which causes the morbid symptom of sleep so characteristic of the disease which we are studying.

By the acts of its nutrition the germ of the disease causes grave disorders and brings on death. The germ, being aerobian, absorbs during its life large quantities of oxygen, and burns up many of the elements of its medium of cultivation. This may be seen by comparing the extract of the broth, before the development of the germ, with the extract of the liquid in which the development has taken place. Everything seems to show that it is from the globules of the blood that the oxygen necessary to its existence is derived by absorption through the tissues. While the chickens are still alive, even when death is still far off, their combs assume a violet tinge at a time when the germ of disease is so little diffused through the blood that it escapes microscopical examination. This species of asphyxia is one of the most curious traits of the disease we are studying. Death is caused by the grave disorders brought about by the development of the parasite in its body, by pericarditis, by serous extravasations, by alterations of its internal organs, by asphyxia, but the sleep characteristic of the disease is caused by a product formed during the life of the germ, which acts on the nervous centres. The independence of these two effects in the symptoms of this disease is further established by the fact that the extract from a filtered cultivation of the germ acts as a narcotic on chickens which have been submitted to the *maximum* degree of vaccination.*

These facts will, doubtless, be found worthy of the meditations of pathologists.

Although I have taken already much of its time with this subject, the Academy will allow me to call its attention to some other characteristics of the disease called *chicken cholera*. We know that this disease is rapidly fatal, particularly if caused by a direct inoculation of its germ. It must then appear extraordinary that it sometimes presents itself in the chronic state, as in the case of inoculated chickens; which, after being severely ill, do not die, but seem to get relatively better. They eat, however, very little; they become anæmic, as shown in the discoloration of their combs; they continue to lose flesh, and finally die, after lingering for weeks or months. This fact would not be of primary importance if, at the death of the chicken, the germ of the disease was not, in most cases, found in its body, which conclusively proves that the parasite has been present since the last inoculation, always active, although in a mild form, for it brings on death slowly. Doubtless, the germ was placed in some *vaccinated* portion unfavorable to its cultivation. Vaccinnated chickens are most apt to present this form of disease, which is of very rare occurrence. We might be led to believe that, in this case, the virulent virus is changed into the attenuated, but this would be an error. In cases of this kind the virulence of the germ of the disease seems, on the contrary, to be aggravated. This may be easily seen by cultivating it artificially, so as to separate it from the blood, and inoculating it on fresh chickens.

Facts of this kind help us to understand the possibility of those long incubations of virus, such as that of rabies, for instance, which, after existing a long time in the body in a state which may be called latent, suddenly manifest their presence by the most marked virulence and by death. Do not these facts also throw light on human pathology?

Alas! how often we see virulent diseases, such as scarlatina, measles, typhoid fever, followed by serious disorders of long duration, which are frequently incurable? The facts to which I have called attention are of the same nature, only here we can put our finger on their true cause.

I will conclude by pointing out another peculiarity, which is not less worthy of the attention of the medical profession.

In chickens in perfect health which have been thoroughly vaccinated there often occurs an abscess full of pus on some portion of the body, which does not seem to have any injurious effect on the health of the animal. It is a remarkable circumstance that this abscess is due to the germ of chicken cholera, which remains in it as in a closed vessel, and it cannot propagate, doubtless, because the chicken has been vaccinated. This germ may be withdrawn by artificial cultivation, or it may be directly inoculated on fresh chickens, which it kills in the usual manner after an abundant development. These facts recall the abcesses on guinea-pigs, which I have mentioned in the first communication on this subject, and they furnish a rational explation of what happens in these abcesses. In all likelihood the muscles of the guinea-pig cultivate the germ more slowly and with greater difficulty than those of chickens; the disease is limited to an abscess, and recovery becomes possible.

I will now conclude this statement, as I have no wish to wear out the patience of this Academy. This subject is, however, so vast and so fruitful that I will ask its permission to bring the subject before it again. I have other observations to present than these. I will add those which will present themselves in the investigations I am now making.

"We would give nothing to the public," said Lavoisier, "if we waited until we reach the end of our researches, as these become broader and more extended the farther we advance."

* I should, however, try to isolate the narcotic, and see whether a sufficient quantity could cause death, and whether, in this case, the internal disorders would be the same as those of the disease itself.

THE NEW CHEMISTRY.

Lieut.-Col. W. A. Ross, who has done so much to advance our knowledge of blowpipe analysis, and whose original chemical investigations are of the greatest interest, in speaking of Prof. Cooke's "New Chemistry," indicates as follows, that much more radical changes, at all events as regards anhydrates, will shortly demand the attention of philosophical chemists, in consequence of the following facts:

FACT 1. 5mgrs. of pure caustic lime are carefully fused into a bead of pure boric acid before the blowpipe, the bead boiled in distilled water, and the transparent calcium borate ball thus extracted, weighed. The weight will invariably be exactly 20mgrs.

FACT 2.—The above-mentioned ball is now fused into a second bead of boric acid, the transparency of which it does not in the least degree affect, and when again boiled out it has the same weight—viz., 20mgrs.

FACT 3. —5mgrs. of calcium hydrate are now fused into a boric acid bead similarly to (1), when it is observed that the borate ball formed is at first opaque white; then, as it becomes transparent B B, that an enormous amount of opalescent matter is emitted from the ball into the bead; and finally, that the extracted ball weighs only 15mgrs.

FACT 4.—The calcium borate ball (2) is now held as a bead *per se* on platinum wire, and 2.5mgrs. of pure silica, or of rock crystal, dried at red heat, dissolved in it B B: after which the silicious ball is weighed, and added B B to a boric acid bead, which it NOW *renders opaque with opalescent matter;* finally, the extracted ball, when weighed, showed, in an average of three assays, an increase in weight of 42 per cent.

FACT 5.—5mgrs. of pure "anhydrous" silica (SiO_2) are carefully taken up on a bead of pure boric acid, and observed to be absolutely unalterable there, B B. A weighed ball of anhydrous calcium-borate is now added B B to this bead, when the silica is gradually decomposed —the weight of the ball being unaltered—not into silicon and oxygen, but into really anhydrous silica (*which possesses extraordinary electrical properties*), and some compound of hydrogen, which makes the bead opalescent. After boiling, only 2mgrs. of residue are obtained.

Now these five facts, and more especially the immense increase in weight of the silicious calcium-borate ball (4), notwithstanding the great loss of matter causing opalescence, show that there is an enormous percentage, nearly half, of SOME COMPOUND OF HYDROGEN, not eliminable as gas, existing in what has been hitherto supposed to be an anhydrous substance, which has escaped even the closeness of modern chemical analysis, for the simple reason that the water solutions of acids and alkalies used to analyse, themselves contain this very compound of hydrogen.

Many confirmatory proofs of this startling truth have been afforded, but cannot be detailed here, because the details form part of the subject of a competitory essay, and cannot yet be published.

Hydrogen, however, in this solid form, can now be proved to be an almost omnipresent component—of all so-called "anhydrous" minerals, of most artificial as well as natural inorganic productions, of many so-called "elements," and, to my mind, of the galvanic "currents" themselves.

Thus it is seen that the beautiful and immaculate theory of combining proportions, first enunciated in 1777 by the illustrious Wenzel in his "Lehre Von der Verwandschaft der Körper," relates entirely to hydrates, and that a new chemistry, the chemistry of anhydrates, now requires to be studied.

Let us hope that some future Wenzel and Dalton will apply proportional and atomic theories to this anhydrate chemistry, and now that the first dawning of the truth has at last been published in Germany and America as well as here, we cannot doubt that this will soon be done.

It remains, now, an unpleasant part of my duty to point out that, although I supposed, by the discovery of the above mentioned facts, I had laid the first foundation of what must, sooner or later, be adopted as a new and essential study by everyone who aspires to the title of a philosophical chemist, I found I had been anticipated in my most important deductions by no less a man than Joseph Priestly.

That unfortunate genius—in repeating one of whose experiments with a more powerful electric battery, Sir Humphry Davy discovered potassium—has been so utterly misrepresented by the modern school of chemists, which has elevated Lavoisier in his place as the founder of the chemistry of hydrates, that it would take more time than you and I can afford, to adduce in proof, a quarter of their misrepresentations.

I will give just one instance. Prof. Cooke, in the book called "The New Chemistry," says (p. 98): "Iron, in rusting, gains in weight, 'Hence,' said Lavoisier, 'it has combined with some material.' 'No,' said such men as Cavendish, Priestley, and Scheele, 'it has only lost phlogiston, which differs from your gross forms of matter in that it is specifically light, and, when taken from a body, increases its weight.' We smile at this idea," etc.

Now what does Priestley himself say?—See p. 249, Vol. I., "Experiments and Observations," sect. IV., "Inflammable Air."—"It was even asserted by some that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before, on which account they chose to call it the principle of levity!" Priestley says here, that he "discovered phlogiston to be hydrogen by direct experiments."

Then follow those celebrated experiments—so much neglected and concealed by modern chemists—in which Priestley converted a certain quantity of lead oxide into a certain quantity of lead "by throwing the focus of a burning-lens upon it through a glass receiver filled with a certain quantity of "inflammable air"—or hydrogen.

It may be fashionable now to "smile at the ideas of such men as Cavendish, Priestley and Scheele"; but it seems to me much more reasonable to smile at the ideas of Lavoisier and his disciples, who did not seem able to understand the possibility of a compound losing (by means of heat or other factor in the operation) an extremely light constituent, and taking up, instead of it, another surrounding constituent sixteen times as heavy, whereby the aggregate weight of the compound would, of course, be increased by the coefficient fifteen.

In precisely the same way I have proved, by my humble experiments, that a ball of calcium-borate, having silica (for instance) dissolved in it, increases enormously in weight by treatment in boric acid B B, although it obviously loses a large quantity of hydrogenous matter, which renders the whole bead opaque white; simply because the compound acquires, instead, a much heavier constituent—viz., boric acid.

We invite those who are interested in the blow-pipe analysis who desire any information on the subject to address a letter to "SCIENCE," as Col. Ross is one of our subscribers, and appears always ready to aid those who require instruction. A letter to "SCIENCE" will doubtless receive prompt attention.

THE AMERICAN CHEMICAL SOCIETY.

The March meeting of the American Chemical Society was held on Monday evening, the 7th inst., Vice-president Squibb in the chair. The resignations of the following gentlemen were read and accepted. Messrs. Elihu Root, H. G. Smith, F. Alexander, J. T. O'Connor, and also, in consequence of its interference with his business, the Recording Secretary Dr. A. H. Gallatin, tendered his resignation from office. Mr. Theodore Tonnelé and Mr. J. G. Mattison were nominated for membership. The reading of papers followed, the first of which "A New Specific Gravity Bottle" by William H. Gregg, was read by Dr. A. Behr. The essential difference between the ordinary bottle and the one devised by Mr. Gregg consists in that the latter has an expansion or bulb just above where the stopper is, in the regular form. A thermometer serves as a stopper passing through the bulb sealing it at both extremities. The advantage of this improvement is that the liquid cannot run over or volatilize (in the case of essential oils, etc.,) for it will be retained in the bulb which is stoppered at each end by the thermometer.

The second paper was by Dr. J. H. Tucker, "On the solvent action of carbonic anhydride in solution upon various bodies under different conditions as to temperature and pressure." The methods of manipulation were first detailed, after which the effect upon the "various bodies," these being chiefly mineral, was described. Mr. Casamajor followed with some observations upon the difficulty that he had experienced in obtaining hydrogen sulphide from impure iron sulphide. After some experimenting he found that upon adding a little zinc amalgan, alopians evolution of the gas ensued. By this method he was successful in obtaining excellent results with galena, orilcopyrite and pyrite.

Mr. J. H. Stebbins, Jr., called the attention of the society to several new coloring materials that he had discovered among the di-amido compounds. They were yellow in color and suitable for silk, woolen and cotton dyeing, but especially desirable for the latter.

Dr. A. R. Leeds gave a short description of some new experiments on the action of hydrogen peroxide with ammonium hydrate.

A committee consisting of Mr. Casamajor and Dr. Alsberg were appointed to make arrangements for the annual dinner. M. B.

NEW YORK, *March* 9, 1881.

IMPROVED PORTABLE EQUATORIAL STANDS.

By JAMES H. GARDINER.

The stand I use, and those which I have seen, have no levels and no means by which the telescope can be moved in azimuth without moving the whole stand. It seems to me that, with a very little trouble, these stands could be made not only a great deal more accurate, but also much more useful for amateur work by the following additions: Instead of having the equatorial mounting screwed firmly to the lower plate to which the legs are attached so the telescope cannot be moved in azimuth without moving the whole stand, a plate could be ground to touch, say, only ¼ of an inch, and revolved on the lower plate. This would give a steadier and easier motion, with less friction than if the two plates were ground to touch all over. A thread is to be cut in the side of this upper plate, so that with a tangent screw it can be moved in azimuth. On this upper plate that revolves on the lower plate, and to which the tangent screw is attached, should be placed two levels at right angles to each other, and then on this upper plate that revolves the usual equatorial mounting is to be firmly fastened. It will be seen that the above stand only differs from the usual stands on tripods, in having levels and means to move the telescope in azimuth without moving the whole stand. Such a stand would be of great use to amateurs, who have a poor horizon, and are obliged to move their stands about to command all parts of the heavens; or for those who may have a good horizon, but cannot afford the luxury of a fixed pillar and dome. The use of such a stand will appear from the following illustration; Suppose the observer has such a stand, and that he is at Washington, and on the 1st of March, 8 P. M., he desires to put his telescope in the meridian. He carefully levels the stand, and turns his telescope on *a Polaris* to come into the centre of the field. If it does not happen to come exactly in the centre of the field, he can raise or lower his polar axis, or move the telescope in azimuth by aid of the tangent screw. Here it is to be noted that with the old stands he would have to twist the whole stand around and throw it out of level, and by repeated trials get *a Polaris* in the centre of the field, and when he again levelled the stand *a Polaris* might not be in the centre of the field. Thus every movement of the old stand would throw it out of level. All these tedious trials are obviated by the new stand with azimuth motion. When once levelled it would stay so, and the telescope could be moved to the east or west without having to be continually bothered with levelling it. Thus in a few moments he would have *a Polaris* in the centre of the field, and the telescope approximately in the meridian. He now reads his R. A. circle, and turns his telescope on some well known star, as *a Leonis* or *Regulus*, for example, and then reads his R. A. circle again. Supposing the difference of these two readings of the R. A. circle to be 3h. 25m. 13s., this is the observed hour-angle of *Regulus*. The true hour-angle of *Regulus* is equal to the difference of the Sidereal time and the R. A. of *Regulus*, or 3h. 22m. 13s. This shows that the object-end of the telescope must be moved 3m. to the west to make the observed hour-angle agree with the true hour-angle. This can be done nicely by the targent screw that moves the telescope in azimuth without throwing it out of level, but with the old kind of stand it would be thrown out of level, and it would be a very tedious job, requiring time and patience to accomplish. Having got the telescope very nearly in the meridian, the declination circle can now be set to the δ of the star. With such a sand the careful amateur can put it near enough in the meridian to pick up a comet or any other object by its R. A and δ. The accuracy of the adjustments depends upon the levelling, the collimation, and an exact value of the local time. The levelling would generally be accurate enough, and most stands have screws in the saddle that carries the telescope for correcting the collimation. But the amateur should try to get the exact value of his local time, as this would probably introduce the greatest error. This can be done by equal altitudes of the sun or star. Or where the latitude of the place is well known the local time may be found by an altitude of the sun. With such a stand as has been described, if it should be necessary to move it to another place, it could easily be put in the meridian again, as described. Besides, many have stands with good circles which they seldom use, because they cannot afford a fixed pillar and dome, and do not care to put it in the meridian, as they are obliged each night to bring the telescope into the house. But if it could be put in the meridian easily, I am sure many would be pleased to use their circles.

ASTRONOMICAL MEMORANDA.

[Approximately computed for Washington, D. C., Monday, March 21, 1881.]

Sidereal time of mean noon, 23^h, 57^m, 24^s. Equation of time, 7^m, 8^s. Mean noon *preceding* apparent noon.

On the morning of March 20th, the sun crosses the equator and enters the constellation Aries, thus indicating the commencement of Spring. The violent actions upon the sun's surface have continued throughout the past month.

The *moon* reaches its last quarter on March 22, and is new again on the 29th. On March 21st, she crosses the meridian at 4 A. M. The moon will be in conjunction with Mercury on the 27th, and with Jupiter and Saturn on the morning of the 31st.

Mercury is morning star, crossing the meridian about an hour before the sun, nearly 6 degrees farther south. Mercury was in inferior conjunction with the sun on the 11th and is travelling towards the west.

Venus has been moving westward since her greatest eastern elongation on the 20th of February, and will continue to increase in brilliancy till March 27th. She crosses the meridian at about 2.40 P. M., about 20 degrees farther north than the sun.

Mars, crossing the meridian nearly 3 hours in advance of the sun, is coming towards us, and gradually increasing in brilliancy.

Jupiter crosses the meridian at about 1.15 P. M., and *Saturn* 15 minutes later. They are both becoming very unfavorably situated for observation, and must be looked for immediately after sun-set.

Uranus is in right ascension 10^h, 50^m, 47^s; declination 8° 14′ north, and was in opposition on March 1st.

Neptune, right ascension 2^h, 47^m, 17^s; declination 13° 56′ north. Neptune and Venus are in conjunction on the 23rd.

THE following is a list of the officers and council of the Royal Astronomical Society, elected February 11, 1881:— President: J. R. Hind; Vice-Presidents: Prof. Cayley, E. Dunkin, W. Huggins, E. J. Stone; Treasurer: F. Barrow; Secretaries: W. H. M. Christie, J. W. Glaisher; Foreign Secretary: the Earl of Crawford; Council: Prof. Adams,

Sir G. B. Airy, J. Campbell, A. A. Common, G. H. Darwin, Major J. Herschel, E. B. Knobel, G. Knott, A. Marth, E. Neison, A. C. Ranyard, Prof. H. J. S. Smith.

THE gold medal of the Royal Astronomical Society has been presented to Prof. Axel Möller, Director of the Observatory at Lund, in Sweden, for his investigations on the motion of Faye's comet. W. C. W.

WASHINGTON, March 18, 1881.

MICROSCOPY.

On looking over the Transactions of the New Zealand Institute for 1878, we notice that a Mr. A. Hamilton speaks of having discovered *Melicerta ringens.* It was found in great profusion,on the finely-divided leaves of the *Myriophyllum.* This adds another locality to the wide geographical distribution of this interesting Rotifer.

Mr. Hamilton states that after examining a number of specimens he found the description given by Gosse correct, except that the formation of the pellets was at a much slower rate than that stated by him.

In the same locality were also found organisms which Mr. Hamilton thought to be *Plumatella repens;* they were growing on dead thistles in a swamp in only a few inches of water.

The American Monthly Microscopical Journal for March editorially announces the immediate publication of Mr. F. Habirshaw's Catalogue of the Diatomaceæ, also by the editor, a small book based on Professor J. Leidy's "*Freshwater Rhizopods of North America.*" The editor's handbook on Adulteration is withdrawn.

In the same number Dr. F. S. Billings gives a long resumé of what is known about "*Trichenæ,*" but seems to offer no new facts; the illustration he offers of "*Fresh trichinous invasion*" (after Heller) is a wretched misrepresentation of free trichinæ.

Any reader desirous of examining living specimens of trichinæ in this condition can obtain them on calling at our office.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

The development of a peculiar non-nervous tissue in connection with the rhomboid sinus of the lumbo-sacral intumescence in birds, and which is especially well marked at the embryonic period, is I think of some bearing on the recently agitated question of a so-called lumbar brain in the extinct sauranodon. In all amniote embryos that I have studied myself, and of which I can find illustrations in accessible works, it is remarkable that there is a distinct posterior enlargement before the cephalic enlargement is well marked, or the brachial intumescence is even indicated in the medullary tube. This fact may point to the potent influence of some, at one time, deeply engrafted ancestral trait. It is not, I think, necessary or warrantable to go beyond this fact, and the established one of the existence of a non-nervous enlargement at the same region in allied sauropsidæ in endeavoring to account for the peculiarity found in the spinal canal of an extinct saurian genus. The supposition of the existence of anything meriting the designation of a brain elsewhere than in the cranial cavity in any amniote animal would be so fundamentally out of harmony with what we have learned to consider as the normal type of structure, that much stronger evidence than the size of a bony receptacle must be adduced before it can even be taken into consideration. That the size of a cavity and that of the contained organ are not neecssarily in close correspondence, has been alluded to by another correspondent under the initials of B. G. W. I have been struck, in this connection, with the discrepancy between the size of the brain cavity and the brain itself in a two year old hippopotamus, though they corresponded in a young elephant.

Respectfully,

E. C. SPITZKA, M. D.

N. Y. 130 E. 50th Street.

To the Editor of SCIENCE:

PARIS, March 5th, 1881.

In bringing before your notice various points which are both novel and interesting, it seems to be my fate constantly to struggle with an *embarras de richesse* represented by a vast combination of phenomena which is forever appearing upon the scientific horizon.

Condensing therefore as much as possible the matter at my command, I will begin with a very trite and commonplace observation; petroleum is a most excellent thing in its way. It is inexpensive and it gives forth a beautiful light. But these advantages, as many know to their sorrow, are more than counterbalanced by the disagreeable habit it sometimes has of exploding. Accidents thus occasioned, frequently prove fatal, as the violence and intensity of the explosion prevent, in most cases, speedy relief being administered to the victims. Besides this, the methods employed are inefficient and usually unsatisfactory.

M. Ichlumberger, whose mind for some time has been occupied with this subject, finally proposes a mode of extinction which is exceedingly simple, and at the same time instantaneous. So confident is he of the efficacy of his plan, that he would like to make a law compelling every one to adopt it who has petroleum in any quantity.

This is his method; Upon every keg or barrel of petroleum, place a moderately large bottle filled with aqua ammoniæ. Should an explosion occur, the shock will shatter the bottle, spread the fumes of the ammonia in the atmosphere, and produce an automatic and infallible extinction of the flames.

This plan can well be recommended to those who make use of petroleum, or who are obliged to superintend the distillation of the liquid. It is only necessary to have within easy access one or several bottles of aqua ammoniæ, whose contents should instantly be scattered upon the petroleum in case it catches fire.

M. Ichlumberger also thinks that this mode of extinction could be effectively utilized in mines where fire-damp is imminent. The ammonia should be put in reservoirs, and so placed that it will be overturned immediately when the explosion occurs. This agent would undoubtedly be more powerful than water, and M. Ichlumberger's idea is worthy of serious attention.

A very peculiar case of poisoning occurred a short time ago at *Puy l'Evêque*, an account of which was sent to the *Académie de Médicine* by Dr. Demeaux. It seems that a family composed of five persons was taken violently ill after having eaten some mushrooms. One of the mushrooms left from dinner was sent by Dr. Demeaux to the *Académie* as a specimen, and upon being examined by M. Chatin, was found to belong to one of the numerous varieties of the *oronge-cigué* species called the *Amanita bulbosa.* Nine-tenths of the mushroom poisoning we hear about is due to this *Amanita* which, on account of its white color is frequently mistaken by the inexperienced and unsuspecting for the harmless mushroom. It is certainly the height of folly for people to run about the woods and fields mushroom hunting, unless they are perfectly familiar with the different species.

Science, it seems, is able to reap some benefit from everything, however trivial! Fancy the ignoble art of Tattooing being elevated to a philanthropic institution! And yet, this is indeed the case. Up to the present day, the artists whose business it was thus to decorate the human skin, confined themselves to tracing merely a warlike emblem in indigo or vermillion upon the arms of our troopers, with the number of the regiment added sometimes. However, their ambition led them to execute a more intricate and ornamental design, such as a flaming heart pierced by an arrow, accompanied by the inscription, "To Mary," or something equally effective. Henceforth, be it understood, these dermographic artists will be looked upon as valuable auxiliaries to surgery.

"Why is it," asks Dr. le Comte, who is physician to a regiment of dragoons, "why is it that such quantities of soldiers die upon the battle field?" And then he replies confidently: "Simply because of the difficulty which arises in regard to arresting hemorrhages,"

The compression of an artery being the best mode of stopping profuse bleeding, Dr. le Comte proposes to teach each soldier first where these vessels are situated, so that he may assist himself while waiting for the surgeon. Therefore, he tattoos an image of some kind upon every portion of the soldier's body where there is an artery. Think of it! Has ever a more ridiculous and absurd idea been put into practice? How infinitely preferable it would be to furnish each soldier with a tourniquet, or at least compel him to attend six lectures upon anatomy, even though such a course might spoil a good soldier to make a bad doctor.

I believe some news has already reached you of Balmain's luminous painting, which attracted public attention some months ago and was first practically applied at the establishment of Messrs. Thlee and Horm. The ceilings of their different offices were covered with a layer of the composition, dissolved in water, and the effect produced is that of a diffused light which is sufficient to enable one to distinguish the various objects in the room.

M. Balmain's idea is excellent, and it would be most advantageous to paint the ceilings of rooms, passages, halls, etc., with his composition, should the use of lamps be dangerous or not absolutely necessary. A simple border of the painting is sufficient in narrow passage ways and stair-cases, and costs a mere nothing.

When dissolved in water, the composition can be applied like whitewash or kalsomine and is useful in more ways than one. Large slabs of glass have been covered with it and employed on board of English marine vessels, also in the Waltham powder factory and in Mr. Young's refinery to illumine places where it is impossible to carry a light. This painting has likewise taken the place of lamps upon several railroads in England, particularly those lines where tunnels are so frequent as to necessitate constant light in the carriages.

Now, a word about meteorology. Nearly every book that has been written on the subject, tells us unhesitatingly that the *Aurora Borealis* is a very rare occurrence except in the polar regions. It appears, however, that this is by no means the case, and that it can be observed with equal frequency in countries occupying a much lower latitude.

M. Sophus Tromholt, of Bergen, Norway, has just published an interesting account of some observations made at his request during the winter of 1878-1879, at one hundred and thirty-two stations extending throughout Sweden, Norway and Denmark. Many extraordinary facts concerning the *Aurora Borealis* can be gathered from this work. To give you an example, it was found that scarcely an evening passed that the phenomenon was not witnessed in one of these countries. M. Tromholt thinks the Aurora is often a local phenomenon, situated but a short distance above the surface of the earth. To strengthen his opinion he quotes many cases in which the Aurora was seen at one or more of the stations without being visible at Bergen, the headquarters, so to speak, where observations were carefully made both day and night. The phenomenon was only seen three times simultaneously by all the stations comprised in 71 and 55 degrees. And even then, who knows but that it was the same Aurora that each saw?

I cannot close my letter without mentioning why the inauguration of the Berlin electric railroad has been so long delayed. As was feared, electricity escaped from the middle rail, and a copper conductor supported by means of stakes has been substituted. Although this is a great improvement, it is doubtful whether the railway can be used in rainy weather, and this fact justifiesan article recently published in *L'Electricité*, which affirms that electric railroads can only be properly employed in tunnels such as, for instance, those of the future Metropolitan in Paris. COSMOS.

VANILLIN.—Meissner's new process for the manufacture of the aromatic principle of vanilla consists in producing it from eugenol $C_{10}H_{12}O_2$ by first forming aceteugenol; then oxidizing the said product with a certain proportion of permanganate of potash in a neutral solution; and, finally, further oxidizing the product with bichromate of potash in a neutral solution. The aceteugenol is obtained by digesting the dry eugenol with an excess of acetychloride C_2H_3OCl. The excess of acetychloride is distilled off, and the remainder used for the production of vanillin. (1). The crude aceteugenol is oxidized in a neutral solution, 47 to 50 parts of permanganate being used to 20 parts of aceteugenol. The product obtained is separated by a filter press from the binoxide of manganese formed during the oxidization, and after the decomposition of the small quantity of the carbonate of potash by sulphuric acid, the clear liquid is evaporated in a vacuum at 50 deg. C., to about 1-15th of the original volume. The acetvanillin is extracted from the lye thus obtained by repeated agitation with ether. (2). The lye freed from the acetvanillin is heated to 100 deg. C. to remove all the ether, and after being neutralized is mixed with neutral chromate of potash. The mixture is heated until the chromate is decomposed, and the product filtered off from the oxide of chromium, and shaken up with ether to remove the acetvanillin formed during the oxidizing process. This operation is repeated on the lye several times. After the evaporation of the ether, the acetvanillin is boiled with soda, by which operation crude vanillin is obtained, which is purified by being dissolved in ether, and shaken up in a warm saturated solution of bisulphite of soda, and set aside to crystallize. The crystals are washed in bisulphite of soda solution and strong alcohol, and finally decomposed by sulphuric acid when the vanillin separates as a colorless oil, and can be finally recrystallized in water.

IN speaking recently of the Washington telescope we inadvertently referred to it as a 32-inch equatorial. This instrument is well-known to have an object glass of 26 inches diameter. The objectives for the Russian Government ordered by Struve is 30 inches, and the Lick equatorial will have a 36-inch objective.

THE CAUSE OF SPONTANEOUS DECOMPOSITION OF RAW CANE SUGAR.—Organisms contained in these sugars multiply and produce an inversive ferment.—U. GAYON.

PERSISTENT VITALITY OF CARBUNCULAR GERMS, AND THEIR PRESERVATION IN CULTIVATED SOILS.—At a farm near Senlis, cattle which have died of carbuncular fever twelve years ago have been buried at a certain spot in a walled garden. Samples of the soil were lixiviated and concentrated, and guinea-pigs inoculated with the matter died quickly with well-marked symptoms of carbuncle. Of seven sheep allowed experimentally to pass a few hours daily on this spot, two died of the same disease in the course of six weeks, whilst the rest of the flock from which the seven had been taken remained healthy.—M. PASTEUR.

BOOKS RECEIVED.

SIR WILLIAM HERSCHEL: HIS LIFE AND WORKS. By EDWARD S. HOLDEN, of the United States Naval Observatory, Washington. Charles Scribner's Sons, 743 Broadway, New York. 1881.

There is a charm which attends the memory of some representative men, and which endears even their history to posterity.

Foremost among such men we recall the name of Herschel, and we could hardly select a more pleasing task than to touch lightly on a few salient points in his eventful career.

In our opinion the great feature in Herchel's history was, that he succeeded in reaching eminence as a scientific man, notwithstanding the apparently insurmountable difficulties that stood in his path to success.

Consider for a moment the position of Herschel when he made his first effort to become an Astronomer. He was 34 years of age, residing in a foreign country where he was unknown, and earning a bare existence as a musician, with a younger brother on his hands, and a sister who was not even acquainted with the language of the country (England) in which they then resided.

They were too poor to hire a servant, and what with out-door performances and giving instruction at home, there was little time for recreation, for even leisure moments were occupied by copying music. So that it was only at night, when he would retire wearily to bed, with a basin of milk, and Smith's *Optics* and Ferguson's *Astronomy*, that he could devote the first thoughts to a science which hereafter must ever be associated with his name.

He would then rise in the morning with thoughts intent on seeing for himself the celestial objects of which he had been reading over night.

To purchase an instrument was out of the question, but with the indomitable energy of will which stamped his career thereafter, he at once determined to make a telescope with his own hands, and not content with striving to see what other observers had observed, he began to contrive a telescope eighteen or twenty feet long.

But to earn an existence by music now occupied every moment, day and night and it was many months before a telescope could be commenced; but finally in 1744, when he was 36 years of age, he completed a Gregorian telescope, and began to view the heavens under circumstances that must have been depressing to a less ardent mind; for he had to contrive a few spare moments as best he could, even running home between the acts at the theatre to make a short observation, and then rushing back to take his position in the band.

And so, with mind divided between the oratorios of the *Messiah*, *Judas Maccabeus*, &c., and the variable star *Mira Ceti*, along with the music went the Astronomy, until on the 13th of March, 1781, Herchel, this amateur astronomical observer of Bath, made one of the most striking discoveries since the invention of the telescope, for in examining the small stars in the neighborhood of H *Geminorum* perceived one which appeared visibly larger than the rest, and this object proved to be the major planet, now calledUranus.

Naturally, this was the turning point of Herschel's life, and his a ter career was a rapid rise to the highest eminence as a scientific man and one of the most accomplished astronomers.

The story of Herchel's life is now presented by Professor Edward S. Holden, in a charming little book which may be read at a single sitting, and yet complete and ample in all the details necessary to convey to the reader a vivid picture of the great Astronomer.

We admire Professor Holden's book for its simplicity of diction; not a superfluous word is given, and most of the more interesting events are given in the very words of his sister as recorded by her.

We desire to see this interesting work in the hands of the youth of this country, for if a noble example of a successful career will stimulate a young man to exalted aspirations for a useful and honorable life, the perusal of the present memoir should have such an inspiring effect.

We acknowledge the receipt of the following important works from the Government of New Zealand, being part of a series prepared by the Colonial Museum and Geological Survey Department, of which James Hector, M. D., C. M. G., F. R. S., is Director in Chief:

A MANUAL OF THE NEW ZEALAND MOLLUSCA.—A systematic and descriptive catalogue of the marine and land shells, and of the soft Mollusks and Polyzoa of New Zealand and the adjacent islands, by Frederick W. Hutton, F. G. S., C. M. Z. C., Professor of Biology, Canterbury College, New Zealand University, Wellington, 1880.

A MANUAL OF THE NEW ZEALAND COLEOPTERA, by Captain Thomas Brown, Wellington, 1880.

This Catalogue occupies 650 pages and contains 1050 species. It is a complete description of all the New Zealand Coleoptera known to Science, classified according to the views of Lacordaire. This valuable work is spoken of as a monument to the zeal and industry of an ardent naturalist.

PALÆONTOLOGY OF NEW ZEALAND.—Part IV.—Corals and Bryozoa of the Neozoic period in New Zealand; by the Rev. J. E. Tenison-woods, F. G. S., F. L. S. Wellington, 1880.

The author has a high reputation for his minute acquaintance with the Marine Invertebrata of the tropical and temperate parts of Australia, and during the last twenty years has published many works on the subject, so that the inferences drawn in this work may be received with much confidence.

MANUAL OF THE INDIGENOUS GRASSES OF NEW ZEALAND, by John Buchanan, F. L. S., Land-Botanist and Draughtsman of the Geological Survey. Wellington, 1880.

The general system of classification employed by the author is that adopted from Sir Joseph Hooker's standard works on the New Zealand Flora, but the methods upon which the general and specific characters have been arranged is from a more recent work on the British Flora by the same distinguished botanist. Sixty full-page illustrations are given of specimens, nature-printed, each having, in addition, from 10 to 25 drawings showing the anatomical character of the inflorescence in each species, from original microscopic dissections made by the author, whose excellent botanical knowledge, combined with his skill as a draughtsman, peculiarly fitted him for the work.

TRANSACTIONS AND PROCEEDINGS OF THE NEW ZEALAND INSTITUTE, 1879.,Vol.VII., edited by James Hector, C. M. G., M. D., F. R. S.; issued May, 1880. Wellington.

In this volume is a valuable series of papers, many of them well illustrated, and we congratulate the colony on the valuable scientific work accomplished and in progress. We find many of the papers in this volume of the highest interest, and we shall shortly present our readers with selections.

Any of our readers residing in New York who desire to examine these works can do so by calling at our office, and it may be convenient to know that the Colonial Government has arranged a scale of moderate charges, at which any of these publications can be purchased.

SCIENCE:

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JOHN MICHELS, Editor.

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ANATOMICAL NOMENCLATURE.

In this and in the preceding number considerable space is devoted to a somewhat elaborate discussion of the general subject of Anatomical Nomenclature, accompanied by practical suggestions with regard to the brain.

When we consider that, as stated by Professor Wilder, the brain presents about 150 parts or regions which are visible to the unaided eye, that these parts are more and more frequently mentioned in connection with the progressive sciences of Anatomy, Zoology, Physiology and Psychology, and yet that many of them have received from two to a dozen, more or less, ponderous names, there would seem to be no question as to the desirability of some improvement upon the existing terminology.

The author of this article has undertaken to amend the matter by selecting the shortest or otherwise most appropriate one of the several names by which some parts are known, or by abbreviating descriptive phrases either by discarding all but the most significant word, or converting qualifying adjectives into prefixes, or, in a few cases, mostly of parts observed by himself, by proposing new terms altogether.

The fact is, as every original investigator is aware, all scientific nomenclature is more or less provisional, and must be constantly modified to suit the additions to knowledge and the clearing-up of ideas. The author has given a few instances of the employment of new terms by modern writers, and many more might have been adduced. Marsh uses "postpubis," Huxley "epipubes, pylangium, synangium, intraovular;" Foster employs—if he did not originate—"hemisection and aspychical;" "orad" is used by Thacher in place of *cephalad*, while "dorsad" occurs in recent writings of Mivart, and in Huxley's latest utterance, the paper on "Evolution," parts of which were reprinted in this journal.

Among all the arguments in favor of some modification of the existing nomenclature, the strongest—to the mind of the unprejudiced layman—is, perhaps, the very one which will least commend itself to the professional anatomist: namely, that the ease and comfort of those now living should be held of little moment as compared with any advantage which the change may confer upon the "vastly more numerous anatomical workers of the future."

Those who object to the strictly technical construction of the proposed vocabulary should try to realize what would be the outcome of a total disuse of all technical terms, and the substitution therefor of the vernacular words which are current among the people of the various countries in which anatomy is cultivated. Ancient Babylon would have a parallel in modern Science, and there would result confusion, misunderstanding, contention, and finally apathy and ignorance. Professor Wilder has evidently prepared his article in the hope of eliciting criticism from the working-anatomists of all parts of the world, and not with a view to the hasty praise or dissent of English-speakers alone.

The pages of "SCIENCE" are open to the fullest and freest discussion of the whole subject.

A PARTIAL REVISION OF ANATOMICAL NOMENCLATURE, WITH ESPECIAL REFERENCE TO THAT OF THE BRAIN.*

By BURT G. WILDER, M.D.,

Professor of Comparative Anatomy, etc., in Cornell University, and of Physiology in the Medical School of Maine.

II.

GENERAL NAMES OF ORGANS, AND THEIR ABBREVIATIONS.

For ease of reference these words are arranged in the alphabetical order of their abbreviations.

A.—Area. Ar.—Arteria. Ath.—Arthron, joint, articulation. B.—Bulbus. C.—Cœlia; ventricle of the brain. Cd.—Condylus. Co.—Columna. Cn.—Canalis. Cp.—Corpus. Crn.—Corona. Cr.—Crus. Cs.—Commissura. Ctl.—Cartilago. Dg.—Digitus, finger or thumb. Dm.—Dimidium; half. Dt.—Dactylus; toe, digitus pedis. Dv.—Divisio. F.—Fissura. Fm.—Foramen. Fs.—Fossa. Fsc.—Fascia. Gl.—Glandula. G.—Gyrus; convolution. L.—Lobus. Lc.—Locus. Lg.—Ligamentum. Ll.—Lobulus. Ln.—Linea. M.—Musculus. Mb.—Membrana. Math.—Mesarthron; segment. N.—Nervus. O.—Os. P.—Portio. Pl.—Plexus. R.—Recessus. Rg.—Regio. Rm.—Ramus. Rx.—Radix; root. S.—Sinus. Sb.—Substantia. Sl.—Sulcus. Sp.—Spina. Spt.—Septum. T.—Tuber. Tu.—Tuberositas. Tbl.—Tuberculum. Tr.—Tractus. V.—Vena.

LIST OF NAMES OF PARTS OR FEATURES OF THE BRAIN.

This list includes between 150 and 160 names. Unless otherwise stated they apply to the brains of Man and the Domestic Cat. Most of the names refer to more

* Continued from No. 38, page 126, March 19, 1881.

or less distinct parts, but a few indicate general regions, or areas which are distinguishable by color or elevation. No purely histological features are referred to. Some parts of the cerebellum and medulla are omitted altogether. The names of the fissures of the cat's cerebrum have been discussed in a previous paper, 8.

In each case, the name first given is regarded as preferable; but occasionally I have indicated the desirability of a better one. So much of each name as is printed in small capitals is regarded as a sufficient designation of the part under ordinary circumstances; sometimes it may be desirable to add the words in parenthesis. Most of the names are those in common use, with the omission of superfluous elements like *corpus*, and the genitives of the names of more comprehensive parts. Most of the apparently new names will be found to be old acquaintances under such thin disguises as *translation, transposition, abridgement*, and the *substitution of prefixes* for qualifying words. In a few cases the old names are wholly discarded for briefer new ones. Most of the new names, however, refer to parts apparently unobserved hitherto (*e. g., crista, carina, delta,*) or to parts which—although probably observed—seem not to have been regarded as needing a special designation, (*e. g., aula, quadrans, corpus præpontile.*)

Let me express here my desire to be favored with the fullest and freest criticism, both as to the general questions involved in this revision, and as to the special terms here proposed.

ALBICANS, (Corpus).—*abn.*—C. *candicans, c. mammillare*, etc. Unable to ascertain which of its many titles has priority, I select that which indicates its most obvious feature on the fresh brain.

AMYGDALA, (cerebelli).—*ag. cbl.*

ARACHNOIDEA, (Membrana).—*Ach.*—The arachnoid layer.

AREA CRURALIS.—*Ar. cr.*—The general region of the base of the brain between the pons and the chiasma. The middle region, or region of the isthmus.

AREA ELLIPTICA.—*A. el.*—An area, in the cat, just laterad of the ventripyramis. Perhaps it represents the "inferior olive."

AREA INTERCRURALIS.—*Ar. icr.*—The interpeduncular space. The mesal part of the *Area cruralis.*

AREA POSTPONTILIS.—*Ar. ppn.*—The ventral aspect of the metencephalon, (medulla). The caudal one of the three general regions into which the base of the brain may be conveniently divided for description. It is more extensive, relatively, in the cat than in man.

It will be noted that the adjective *pontilis* follows the analogy of *gentilis* rather than *montanus* or *fontinalis*. The form *pontal*, however, has been used by Owen. (A, III).

AREA PRÆCHIASMATICA.—*Ar. prch.*—The cephalic one of the three areas of the base of the brain. The space cephalad of the chiasma.

ARBOR VITÆ (cerebelli).—*Arb.*

AULA.—*a.*—The cephalic portion of the third ventricle; the prethalamic part of the "third ventricle," between the two portæ, or *foramina Monroi*; 'aula,' Wilder, 3 and 5." "The here common ventricular cavity," in *Menobranchus*, Spitzka, 6, 31. This represents the cavity of the "unpaired hemisphere vesicle," formed by a protrusion from, or constriction of, the "anterior primary encephalic vesicle," the aula is relatively larger in some of the lower vertebrates.

AULIPLEXUS.—*apx.*—The plexus of the *aula*. The free border of the fold of *pia*, known as the *velum*, forms a vascular plexus in the *aula*, in each *porta*, and in the *medicornu* of the *procœlia*. In place of compound terms, like *plexus aulæ*, etc., I suggest that single terms be formed, *auliplexus, portiplexus*, and *proplexus*. For the plexuses of the diacœlia and metacœlia—the "third" and "fourth ventricles"—we may use *diplexus* and *metaplexus.*

BASICOMMISSURA.—*bcs.* "The basilar commissure of the thalami," Spitzka, 2, 14. The ventral continuity of the two thalami.

BIVENTER (cerebelli).—*bv.*—The biventral lobe of the cerebellum.

BULBUS OLFACTORIUS.—*B. ol.* The olfactory bulb. The more or less expanded cephalic part of each lateral half of the rhinencephalon, consisting of the *pes* and *pero*. Often called *olfactory lobe.*

CALAMUS (scriptorius).—*clm.*

CALCAR (avis).—*clc.* *Hypocampa*, or *hippocampus minor.*

CALLOSUM, (corpus).—*cl.*—*Commissura cerebri maxima, trabs medullaris*, etc.

CANALIS CENTRALIS (myelonis).—*Cn.ce.*—The central canal of the spinal cord.

CARINA (fornicis).—*ca.*—The mesal ridge of the caudo-ventral surface of the *fornix*, dorso-caudad of the *crista*. I am not sure of its existence in man.

CAUDA STRIATI.—*cd. s.*—"Surcingle," Dalton (1, 13); the slender continuation of the *striatum* caudo-ventrad. If a new name is required for this longer "tail," which was described by Cuvier (B. 111, 51), as forming, with the striatum proper, a "horse-shoe," Prof. Dalton's "surcingle" may be technically rendered "cingulum." I have not yet looked for the *cauda* in the cat.

CEREBELLUM.—*cbl.*—Several of the external features of the *cerebellum* are omitted from this paper.

CEREBRUM.—*cb.*—The *prosencephalon*, less the *striata*. The *hemisphæræ.*

CHIASMA (opticum, or nervorum opticorum).—*ch.*—The optic chiasma or commissure.

CIMBIA.—*cmb.*—"*Tractus transversus pedunculi*," Gudden, as quoted by Meynert (A, 737). A slender white band across the ventral surface of the *crus cerebri*. It is a distinct ridge in the cat. The word is used in architecture to denote a *band* or *fillet* about a pillar, and is here proposed as a fitting substitute for Gudden's descriptive name.

CINEREA, (substantia).—*c.*—The gray matter of the nervous organs.

CLAUSTRUM.—*cls.*—The "*claustrum*," (Burdach); "*nucleus tæniæformis*," (Arnold), as stated by Quain, A. II, 564.

COLUMNA (fornicis).—*Co. f.*—The anterior pillar of the fornix, assuming that there is one upon each side. It would be convenient to have a single short name.

CŒLIA.—*C.*—A ventricle of the *encephalon*. For a brief statement of the reasons for substituting this for the word *ventriculus*, see elsewhere in this article.

COMMISSURA FORNICIS.—*Cs. f.*—In the cat, a distinct band across the caudal aspect of the *fornix* just ventrad of the *crista*, and apparently uniting the two columnæ more closely.

COMMISSURA HABENARUM.—*cs. h.*—A white band connecting the caudal ends of the habenæ, and forming the dorsal border of the *Fm. conarii.*

CONARIUM.—*cn.*—The *glandula pinealis. Epiphysis cerebri. Penis cerebri.*

CORONA RADIATA.—*Cn. r.*—*C. radians.*

CORPUS PRÆPONTILE.—*Cp. prp.*—A slight white longitudinal ridge of the *postperforatus*, near the meson. It is distinct in the cat. When more fully known, perhaps a better name may be found.

CORTEX (cerebri, or cerebelli).—*ctx.*—The ectal layer of gray and white substance at the surface of the cerebrum and cerebellum.

CRENA (calami).—*crn.*—The caudal end or notch of the metacœlia.

CRISTA (fornicis).—*crs.*—A small but, in the cat, very distinct ovoid mesal elevation of the caudal surface of the *fornix*, ventrad of the *carina*, and dorsad of the *commissura fornicis*, and the *recessus aulæ*. It is also present in the human brain. Wilder, 7.

CRUS CEREBRI.—*Cr. cb.*—Pedunculus cerebri.

CRUS OLFACTORIUM.—*Cr. ol.*—The isthmus by which the *bulbus olf.* is connected with the *prosen.*

CRUSTA (cruris cerebri). *cst.*

DECUSSATIO PINIFORMIS.—*dc. pnf.*—"Piniform decussation," Spitzka.

DECUSSATIO VENTRIPYRAMIDUM.—*dc. vpy.*—The "decussation of the anterior pyramids."

DELTA (fornicis).—*d.*—A subtriangular area of the ventro-caudal surface of the fornix of the cat. The lateral angles are at the *portæ*, and the apex points dorso-caudad. It is bounded by the lines of reflection of the *endyma*, and represents the entocœlian surface of the *fornix*. Wilder, 5. It probably exists in man.

DENTATUM, (corpus cerebelli).—*dnt.*

DISTELA.—*dtl.*—The *tela vasculosa* forming the membraneous roof of the *diccelia* or "third ventricle."

DIENCEPHALON. — *den.* — The *thalamencephalon, deutencephalon, inter-brain*, enclosing the *diccelia*. Whether it should include also the *aula* and its walls is to be determined by reference to the condition of the parts in some of the lower vertebrates.

DORSIPYRAMIS.—*dpy.*—The *posterior pyramid* of the *metencephalon.*

DICŒLIA.—*dc.*—The "third ventricle," or "*ventriculus tertius*," less the *aula*. The interthalamic space, reduced in mammals by the *medicommissura.*

DIPLEXUS.—*dpl.*—The plexus of the "third ventricle."

ENCEPHALON.—*en.*—The brain, including the *medulla* or *metencephalon.*

ENDYMA.—*end.*—*Ependyma.* Lining membrane of the ventricles.

EPENCEPHALON.—*epen.*—The hind-brain, or *cerebellum* with the *pons* and its peduncles, and the corresponding part of the *medulla*. It is difficult, perhaps impossible, to define exactly the limits of the epen. and the metencephalon, and of their respective cavities.

EPICŒLIA.—*epc.*—The division of the ventricular cavity corresponding with the cerebellum. Perfectly disinct in the cat, and even in man, but relatively more extensive in many of the lower vertebrates.

FASCIOLA.—*fscl.*—May not this single word take the place of *fasciola cinerea* and *fascia dentata?* The parts are continuous, and the latter is not *dentate* in the cat.

FILUM TERMINALE (myelonis).—*fl. t.*

FIMBRIA.—*fmb.*—*Corpus fimbriatum. Tænia hippocampi.* "*Fimbria*," Meyn. A, 667.

FLOCCULUS.—*flc.*—*Lobulus pneumogastricus.* The flocks. This seems to be a different part from the *lobulus appendicularis* of the carnivora, with which it has been sometimes confounded.

FORAMEN CÆCUM.—*Fm. c.*—"*Fossa cæca*," Spitzka, 3, 6. *Foramen cæcum* is used by Dunglison and Vicq D'Azyr (A, pl. xviii., "48"), and should be retained, notwithstanding the somewhat unusual application of the word *foramen.*

FORAMEN INFUNDIBULI.—*Fm. inf.*—The orifice in the *tuber cinereum* left after the removal of the *hypophysis* and *infundibulum.*

FORAMEN MAGENDIE.—*Fm. mg.*—The communication of the *metacœlia* with the "subarachnoid space." Not having satisfied myself as to the nature of this communication, I prefer to quote from Quain, A, ii., 513.

FORNIX.—*f.*—*Camara. Testudo cerebri*, &c.

GENU.—*g.*—*Genu callosi.*

HABENA. — *h.* — *Habenula. Pedunculus pinealis.* There seems to be no need of using the longer word. According to my observations, the *habenæ* have a distinct morphical significance as nearly corresponding with the lines along which the *endyma* is reflected toward the opposite side; 5 and 7.

HYPOPHYSIS.—*hy.*—Pituitary body.

HYPOCAMPA.—*hym.*—*Hyppocampus major.* The reasons for preferring the form employed by Vicq D'Azyr are presented elsewhere in this article.

ITER.—*i.*—*Iter a tertio ad ventriculum quartum. Aquæductus Sylvii.* A convenient name for the contracted mesocœlia of man and most mammals.

INSULA.—*ins.*—Island of Reil. *Lobus centralis. Insula cerebri. Gyri operti.*

INFUNDIBULUM.—*inf.*—*Infundibulum cerebri*, &c.

INTEROPTICUS.—(*lobus*).—*iop.*—The interoptic lobe; Spitzka, 4, 98; 5. In some reptiles.

LEMNISCUS INFERIOR.—*lmn. i.*—Spitzka, 4, 95 and 100.

LEMNISCUS SUPERIOR.—*lmn. s.*—I have not been able to identify these parts in the cat.

LIGULA.—*lg.*—"Ponticulus." Ligula, Quain, A, 11, 506.

LIMES ALBA.—*lm. a.*—*Limes alba radicis lateralis rhinencephali.* The white stripe of the lateral root of the rhinencephalon. Perfectly distinct in the fresh brain of the cat.

LIMES CINEREA.—*lm. c.*—The gray stripe of the radix lateralis.

LIQUOR VENTRICULI.—*lq. vn.*—This term is used by Mihalk. A, 163. Is a better one to be found?

LOBULUS APPENDICULARIS (cerebelli). *Ll. ap.* The appendicular lobule of the *cerebellum* of many carnivora, and perhaps other mammals. It seems to have been confounded in some cases with the human *flocculus*, but more probably represents the lateral lobes of the cerebellum. Its relations should be studied in a series of related forms. See my paper, 11, 217.

LOBULUS OLFACTORIUS.—*L. ol.* — The olfactory lobe of the hemisphere. A part of the hemisphere said to be in more direct connection with the rhinencephalon.

LOBUS OLFACTORIUS.—*L. ol.*—A general name for either half of the rhinencephalon, including the crus and the bulbus.

LOCUS NIGER.—*lc. n.*—The *locus niger* of the *crus cerebri*, between the *tegmentum* and the *crusta.*

MEDICOMMISSURA.—*mcs.*—*Commissura mollis.* Middle commissure. "Thalamic fusion," Spitzka.

MEDICORNU (procœliæ).—*mcu.*—*Cornu temporale.* The middle or descending horn of the "lateral ventricle."

MEDIPEDUNCULUS (cerebelli).—*mpd.*—*Crus ad pontem.* Middle peduncle of the cerebellum.

MESENCEPHALON.—*men.*—The mid-brain. The *lobi optici, postoptici and interoptici*, with the corresponding *crura cerebri.*

MESOCŒLIA.—*msc.* The ventricular division corresponding with the *mesencephalon*. In man and most mammals it is usually reduced and known as *iter*, or *aquæductus Sylvii.*

METATELA.—*mtl.*—The membraneous roof of the *metacœlia*, or "fourth ventricle."

METACŒLIA.—*mtc.*—The "fourth ventricle," *ventriculus quartus.* Ventricle of the metencephalon.

METAPLEXUS.—*mtpl.*—The *plexus choroideus* of the *metacœlia.*

MONTICULUS (cerebri).—*mnt.*—The ventral prominence of the *lobus temporalis.* Natiform protuberance. *Alveus. Subiculum.*

MYELENCEPHALON.—*myen.*—The cerebro-spinal axis. The term was proposed by Owen.

MYELON.—*my.*—The spinal cord. Owen. Huxley.

NERVUS OLFACTORIUS.—*N. ol.*—Olfactory nerve.

NUCLEUS LENTICULARIS.—*nc.ln.*—*Nucleus lentiformis.* Meynert.

OBEX.—I have not identified this part.

OLIVA.—*o.*—*corpus olivarium.* Olivary body. Olive. The "inferior olive." Spitzka.

OPTICUS, (lobus). *Natis cerebri.* An optic lobe, excluding the *postopticus and interopticus.*

PERO (olfactorius).—*po.*—The softer cap, or shoe-like covering of the rhinencephalic lobe, from which the *nervi olfactorii* directly spring. In the cat this may be accurate-

ly removed from the *pes ol.* The Latin *pero* denoted a sort of boot made of raw hide.

PES OLFACTORIUS.—*ps. ol.*—The firmer ental portion of each rhinencephalic lobe. As it is the termination of the crus, and has, in the cat, a somewhat foot-like shape, I suggest the above name for it.

PIA (mater).—*pi*— In the cat's brain there are indications of at least two layers of the *pia.*

PONS (Varolii).—*pn.*—*Tuber annulare,* etc. There seems to be no need of the qualifying genitive.

PONTIBRACHIUM.—*pnbr.*—"*brachium pontis;*" Spitzka, 4, 100.

PORTIO DEPRESSA (præperforati).—*Pt. d.*—In the cat the (*locus*) *præperforatus* is distinctly divided into two portions, the caudal of which is depressed, while the cephalic is elevated, and sometimes furrowed. Briefer names are desirable.

PORTIO PROMINENS (præperforati).—*Pt. p.*

POSTBRACHIUM (mesen.).—*pbr.*—*Brachium posterius.*

PRÆBRACHIUM (mesen.).—*prbr.*—*Brachium anterius.* I have not identified these parts.

PORTIPLEXUS,—*ppl.*—The small portion of the free border of the *velum* which hangs in the *porta.*

POSTCOMMISSURA.—*pcs.*—*Commissura posterior cerebri.* The posterior commissure.

PRÆCOMMISSURA.—*prcs.*—*Commissura anterior.*

POSTGENICULATUM, (corpus).—*pgn.*—*Corpus geniculatum internum.*

PRÆGENICULATUM, (corpus).—*prgn.*—*corpus geniculatum externum.*

POSTOPTICUS, (lobus).—*pop.*—*Testis cerebri.* The caudal eminence of the "*corpus quadrigeminum.*" "Postoptic lobe," Spitzka, 4, 100, and 103.

POSTPEDUNCULUS (cerebelli).—*ppd.*—*Crus cerebelli ad medullam.* Inferior peduncle.

PRÆPEDUNCULUS.—*prpd.*—*Crus seu processus ad corpus quadrigeminum.* Superior peduncle of cerebellum.

POSTPERFORATUS, (locus).—*ppf.*—*Locus perforatus posticus.* Posterior perforated space. *Pons Tarini.*

PRÆPERFORATUS.—*prpf.* *Locus perf. anticus.*

PROCŒLIA.—*prc.*—Ventricle of the prosencephalon, "Lateral ventricle."

PROPLEXUS.—*prp.*—The plexus of the *medicornu* of the *procœlia.* It is the long free border of the *velum,* and, still covered by the *endyma,* enters by the rima. It is continuous with the *portiplexus,* and extends to near the tip of the *medicornu.*

PROSENCEPHALON.—*pren.*—The cerebral hemispheres ; *cerebrum* less the *striatum* ; the fore-brain.

PROTERMA.—*prtr.*—The primitive *lamina terminalis* or *l. cinerea.* *Terma embryonis.* My reason for suggesting different terms for the adult and embryonic terminal plate, is that, as now understood, the latter includes not only the *lamina cinerea* of anthropotomy, but also the parts afterward differentiated to form the *columnæ fornicis,* and the *præcommissura,* with perhaps some other parts of the *fornix.*

PSEUDOCŒLIA.—*psc.*—*Ventriculus septi pellucidi.* "Duncan's höhle," Lœwe, A, 13. Fifth ventricle. This is not a true member of the cœlian series. If it ever presented an opening into the *aula,* it is because of some injury which has torn the brain. This point was urged by me in the unpublished paper No. 4.

PULVINAR.—*plv.*—*Pulvinar thalami.* The posterior tubercle of the human *thalamus.*

QUADRANS, (cruris cerebri).—*q.*—In the cat, a depressed area approximately equal to the fourth of a circle, upon the ventral surface of the *crus,* in its meso-cephalic angle.

RADIX INTERMEDIA (rhinencephali).—*Rx.* I.—The middle root of the *rhinencephalon.* In anthropotomy, the *middle* root of the olfactory nerve. In the cat it is little more than a sub-triangular interval between the RR. *lateralis* and *mesalis.*

RADIX MESALIS.—*Rx. m.*—The mesal root of the *rhinencephalon.* The "internal root of the olf. nerve." In the cat, it turns pretty sharply from the ventral to the mesal aspect of the brain.

RADIX LATERALIS.—*Rx. l.*—The lateral root of the *rhinen.* The "external root of the olf. nerve." In the cat it presents a gray and a white stripe—*limes cinerea* and *l. alba.*

RECESSUS AULÆ.—*R. a.*—A small depression between the two *columnæ fornicis,* and ventrad of the *crista.* The aulic recess.

RECESSUS CONARII.—*R. cn.*—"*Recessus pinealis,*" Reich. A, Taf. ix, *rp.*

RECESSUS OPTICUS.—*R. op.*—This is a pyramidal recess, just dorsad of the *chiasma,* the apex pointing laterad. The term is used by Mihalkovics, A, 79.

RECESSUS PRÆPONTILIS.—*R. prpn.*—The mesal depression which is overhung by the cephalic border of the *pons.* Its floor is formed by the caudal part of the *postperforatus.*

REGIO AULICA.—*Rg. a.*—It may be convenient sometimes to employ this term as a designation for the general region, of which the *aula* is the center. Within a short distance of the *aula* are many parts of great morphical importance ; the whole brain seems to converge thereto. Whoever understands the aulic region will find no serious difficulty with the gross anatomy of other parts.

RESTIFORME, (corpus).—*Rf.*—The restiform body of the *metencephalon.*

RHINENCEPHALON.—*rhen.*—The division of the brain, which is united with the cephalic end of the base of the *prosencephalon,* and connected by the *nervi olfactorii* with the *nares.* Each lateral *lobus* includes a *crus* with its *radices,* and the *bulbus olfactorius,* consisting of the *pes* and *pero.*

RHINOCŒLIA.—*rhc.*—The cavity or ventricle of each lateral part of the *rhinencephalon,* and connected with the procœlia.

RIMA (cerebri). *r.* The interruption of nervous tissue between the *fimbria* and the *tænia,* by which the fold of *pia*—still covered by the *endyma*—enters the *procœlia* to form the *proplexus.*

It extends from the dorsal border of the corresponding *porta* to near the tip of the *medicornu.* In a general way it coincides with a lateral half of the "fissure of Bichat," or "great transverse fissure." That, in the cat, the borders of this *rima* are closely united by the intruded *pia,* and that the *thalamus* is *wholly excluded from the procœlia,* was demonstrated by me on the 25th of November, 187-, in the presence of my assistant, Prof. S. H. Gage, who recorded it at the time. It was affirmed in my lectures on Physiology at the Medical School of Maine in the Spring of 1877, and in subsequent courses there and at Cornell University ; and was one of the points made in a paper (4) read at the meeting of the Am. Assoc. Adv. of Sci. in 1879. While affirming this of the cat, I stated that the material at my disposal had not enabled me to demonstrate it upon the human brain, but there was no doubt that the same condition would be ascertained when a human brain could be prepared and examined with sufficient care with reference to that feature. In the Spring of 1880, Dr. Spitzka informed me that Hadlich had denied lately the appearance of the *thalamus* in the lateral ventricle, presumably of man. The fact is, whoever begins his studies of encephalic anatomy with the brains of the lower vertebrates will soon perceive that—excepting for some rupture of the parts—the *thalamus* can no more form a part of the floor of the "lateral ventricle" than can the *cerebellum* or any other part of the brain.

RIPA (deltæ).—*rp.*—The border of the *delta* formed by the reflection of the *endyma* upon the intruded *auliplexus.* Probably also in man.

ROSTRUM (callosi).—*rm.*—The rostrum of the *callosum ;* much shorter in the cat than in man.

SEPTUM LUCIDUM,—*spt. l.*—This term is not only compound, but based upon two misconceptions: that it

is always or even usually *translucent* in mammals, and that it forms a partition between the two *procœliæ* in the ordinary sense. A new term is desirable, which may refer to either of the two lateral halves of the septum, in connection with the *procœlia*, or the rest of the wall of the hemisphere.

SPLENIUM (callosi).—*sp* —The splenium.

STRIATUM, (corpus).—*s.*—The intraventricular, or entocœlian, portion of what is sometimes called the *corpus striatum*. The *nucleus caudatus*. The caudate lobe.

SULCUS HABENÆ.—*Sl. h.*—The slight furrow along the dorsal border of the *habena*.

SULCUS INTERCRURALIS LATERALIS.—*Sl. ic. l.*—In the cat, a distinct lateral furrow in the *area intercruralis*.

SULCUS INTERCRURALIS MESALIS.—*Sl. ic. m.*—A mesal furrow in the *area intercruralis of the cat*.

SULCUS LIMITANS.—*Sl. li.*—The furrow between the *thalamus* and *striatum*, in which lies the free border of the *fimbria* in contact with the *tænia*. The qualifying word is given in reference to the fact that this furrow is the line of separation between the entocœlian surface of the *striatum* and the ectocœlian surface of the *thalamus*. A shorter and more significant term is desirable.

SULCUS MONROI.—*Sl. Mn.*—The term is employed by Reichert, (A, 65, Taf. 11), to designate a part of the *dicœlia* of man ventrad of the *medicommissura*.

TÆNIA (semicircularis).—*tn.*—There seems to be no reason why this single word may not replace the numerous compounds by which the part is known.

TEGMENTUM.—*tg.*—The more dorsal layer of fibers of the *crus cerebri*, separated from the *crusta* by the *locus niger*.

TELA.—*tl.*—A general name for the membraneous roofs of the *dicœlia* and *metacœlia*. "*Tela vasculosa*" is employed by Huxley, *I.*

TERMA.—*tr.*—*Lamina cinerea*. The adult *lamina terminalis*.

THALAMUS.—*th.*—*Thalamus opticus* seu *nervorum opticorum*. As has been well remarked by Spitzka (2), this single word is to be preferred upon all grounds to the compounds which have been applied to this part.

TRACTUS OPTICUS.—*tr. op.*—The optic tract.

TRAPEZIUM.—*tz.*—The *trapezium* of the *metencephalon*. Exposed in the carnivora, but in man concealed by the caudal margin of the *pons*.

TUBER CINEREUM.—*T. cn.*—The elevation just caudad of the *chiasma*, to which is attached the *hypophysis* by the *infundibulum*.

TUBERCULUM ROLANDO.—*tbl. R.* The tubercle or tuber of Rolando, Huguenin, A, 83.

VALVULA (cerebelli).—*vv.*—The valve of Vieussens.

VELUM (interpositum).—*vl.*—The ectocœlian portion of the fold of *pia*, the entocœlian free border of which forms the plexuses of the aula, portæ, and procœliæ.

VENA CHOROIDEA.—*v. ch.*—*Vena Galeni.*

VENTRIPYRAMIS.—*vpy.*—The anterior pyramid. The "prepyramid," Owen, A.

VERMIS (cerebelli).—*vm.*—The median lobe of the cerebellum. This and the other external features of the cerebellum are not here presented with any fullness.

If I venture to hope that a few of the changes proposed in this paper may escape disapprobation, and that all my readers may not be hostile critics, it is because the times have changed, and such an undertaking is now more likely to be viewed in its true light. I have endeavored simply to define more clearly the necessity for terminological improvement which has been admitted, in some cases unconsciously perhaps, by all who have, for example, substituted *ventral* for *anterior*, *ectoglutæus* for *glutæus maximus*, *hypophysis* for *pituitary gland*, *corpus callosum* for *commissura cerebri maxima*, *adrenals* for *suprarenal capsules*, and *basioccipital* for *basilar portion of the occipital bone.*

In evidence that the suggestions here made are not impracticable, it may be proper to state that most of the terms enumerated, particularly those of toponomy, have been used in the anatomical laboratory of Cornell University for from one to three years; that the freest criticism has been asked from the score or two of students working at practical anatomy and making their own descriptions under the immediate direction of Professor Gage; and that, so far from there having been any inconvenience, the wish has been expressed that a similar terminology might be adopted elsewhere.

On what may be called experimental grounds, therefore, it seems to me that, whatever may befall the particular terms here presented, as biological knowledge is more widely diffused, and the demand for it correspondingly increased, considerable changes in nomenclature must be effected unless anatomical teachers are willing to be styled professors of the art of needless mystification.

There is, however, little danger of the too rapid progress of terminological reform; for, whatever may be the general pressure of students and the public, definite innovations are rarely made without the sanction, or at least the toleration, of those who are most inconvenienced by any departures from custom.

The beginner can learn the new terms even more easily than the old, and at any rate he has nothing to forget. But the trained anatomist shrinks from an unfamiliar word as from an unworn boot; the trials of his own pupilage are but vaguely remembered; each day there seems more to be done, and less time in which to do it; nor is it to be expected that he will be attracted spontaneously toward the consideration that his own personal convenience and preferences, and even those of all his distinguished contemporaries, should be held of little moment as compared with the advantages which reform may insure to the vastly more numerous anatomical workers of the future.

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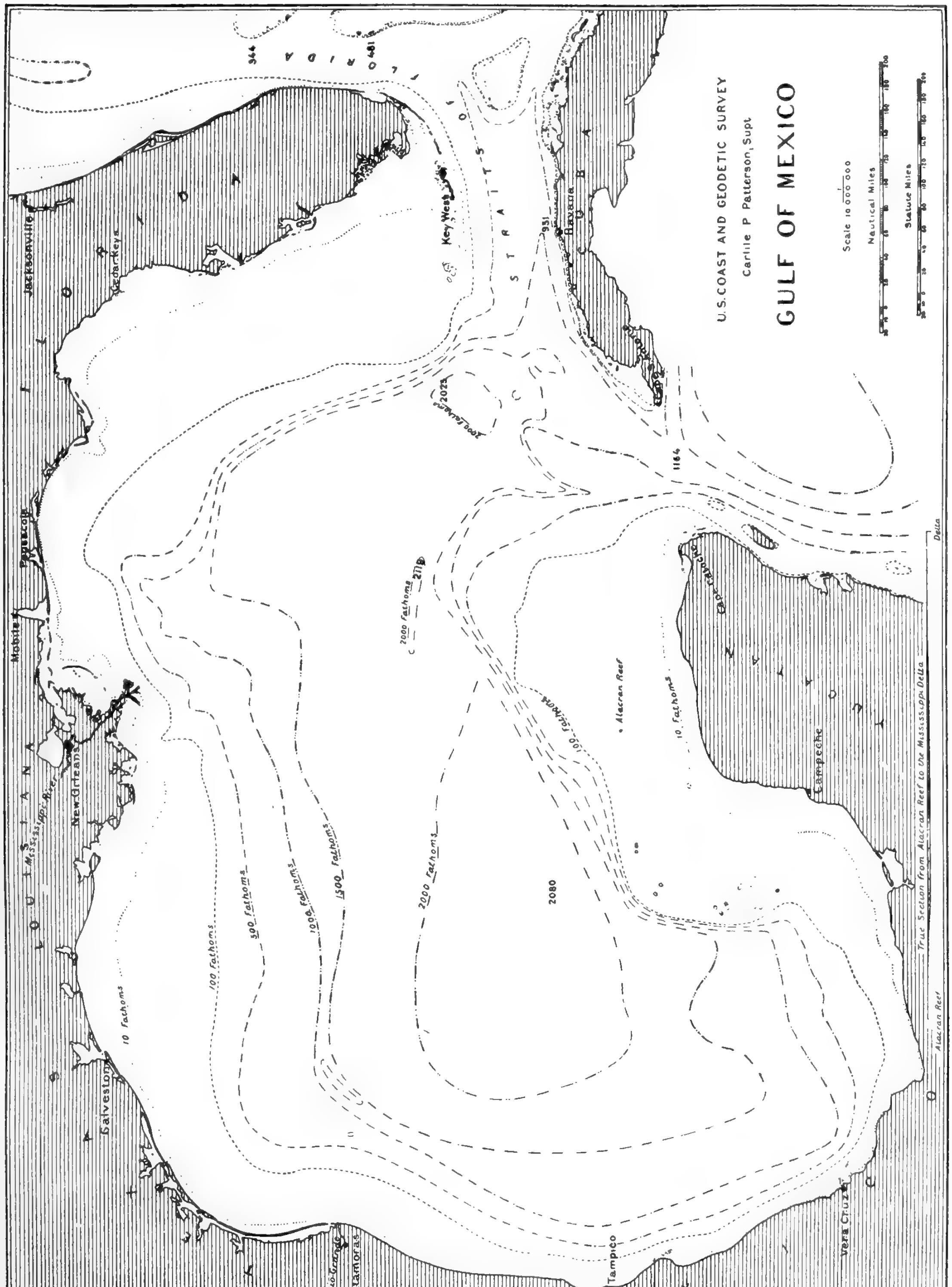
THE BASIN OF THE GULF OF MEXICO.

By J. E. HILGARD, M. N. A. S.

A COMMUNICATION TO THE NATIONAL ACADEMY OF SCIENCES MADE NOV. 18, 1880, BY AUTHORITY OF C. P. PATTERSON, SUPT. U. S COAST AND GEODETIC SURVEY.

At the meeting of the National Academy of Sciences in New York, Nov. 18th, 1880, Mr. J. E. Hilgard presented, on the part of Hon. C. P. Patterson, Superintendent of the U. S. Coast and Geodetic Survey, a model of the Gulf of Mexico constructed from numerous soundings taken in the progress of that work. The accompanying plate is a reduced plan of the model, the full size of which is 24×32 inches, being on a horizontal scale of 1 : 2,400,000, and on a vertical scale of 1 inch : 1000 fathoms; making the proportion of horizontal to vertical scales 1 : 33. The plan shows the horizontal curves of every 500 fathoms of depth, as well as the curves of 100 and 10 fathoms. The same curves are delineated on the model, the forms of which, however, are shaped in conformity with all the detail obtained from the soundings, those inside of 100 fathoms being quite numerous, varying according to the configuration and importance of the locality, while beyond the 100 fathom line, where the work pertains rather to physical geography than to navi-


U.S. COAST AND GEODETIC SURVEY
Carlile P Patterson, Supt
GULF OF MEXICO
Scale 1/10 000 000
Nautical Miles
Statute Miles
FLORIDA
STRAITS OF FLORIDA
CUBA
Havana
Key West
Jacksonville
Cedar Keys
Pensacola
Mobile
New Orleans
Mississippi River
LOUISIANA
Galveston
TEXAS
Rio Grande
Matamoras
MEXICO
Tampico
Vera Cruz
Campeche
YUCATAN
Alacran Reef
10 fathoms
100 fathoms
500 fathoms
1000 fathoms
1500 fathoms
2000 fathoms
2080
2119
2022
1164
1951
364
481
True Section from Alacran Reef to the Mississippi Delta
Delta

gation, 1055 soundings were obtained, of which 355 are in depths greater than 1000 fathoms.

The object of the communication being merely to give a general description of the structural features of the basin of this great inland sea—the American Mediterranean—it is only necessary to mention here, that in connection with the soundings, temperatures were taken at various depths, and the organic life was explored by means of dredges. Everywhere below the depth of about 800 fathoms, the temperature was found to be between 39° and 40° F. The method of sounding was by the use of fine steel wire, indicated by Sir Wm. Thomson, with the mechanical appliances perfected by Commanders Belknap and Sigsbee of the U. S. Navy.

The exploration of the Gulf of Mexico was begun by the U. S. Coast Survey as long ago as 1846, when surveys of the shores were made, and soundings of the approacnes were obtained under the Superintendency of Prof. A. D. Bache. These investigations continued until the outbreak of the civil war, Prof. Bache having in view from the earliest date of his work, the exploration of the Gulf Stream and its attendant phenomena, in addition to the surveys requisite for navigation. When after the close of the war the Coast Survey resumed its former activity, under the administration of Prof. Benjamin Peirce, the physical and biological investigations were continued; but it was not until the present Superintendent of the U. S. Coast Survey, (C. P. Patterson, LL.D.) organized a systematic Exploration of the whole Gulf, that its character became rightly understood. These explorations, begun in 1872 by Commander Howell, U. S. N., on the west coast of Florida in comparatively shallow water, were continued and brought to a successful conclusion by Commander Sigsbee, U. S. N., (1875–78) in the steamer "Blake," accompanied by Prof. A. Agassiz in charge of biological investigations. The methods of obtaining temperatures at great depths as well as of dredging have been described in the Coast Survey Reports for several years past, and more especially in a treatise by Commander Sigsbee recently published by the Coast Survey.

Turning now to our model or map, we perceive that the basin of the Gulf of Mexico is an oval connected with the general ocean-circulation by two outlets, the Yucatan Channel and the Florida Straits. The area of the entire Gulf, cutting it off by a line from Cape Florida to Havana, is 595,000 square miles. Supposing the depth of the Gulf to be reduced by 100 fathoms, a surface would be laid bare amounting to 208,000 square miles, or rather more than one third of the whole area, The distance of the 100 fathom line from the coast is about 6 miles, near Cape Florida; 120 miles along the west coast of Florida; at the South Pass of the Mississippi, it is only 10 miles; opposite the Louisiana and Texas boundary, it increases to 130 miles; at Vera Cruz it is 15 miles, and the Yucatan banks have about the same width as the Florida banks.

The following table shows the area covered by the trough of the Gulf of Mexico to the depths stated:

Depth.	Area.	Differences.
2,000 fathoms	55,000 square miles	
1,500 "	187,000 "	132,000
1,000 "	260,000 "	73,000
500 "	326,000 "	66,000
100 "	387,000 "	61,000
Coast line	595,000 "	208,000

This table shows that the greatest slopes occur between the depths of 100 and 1500 fathoms. The maximum depth reached is at the foot of the Yucatan banks 2119 fathoms. From the 1500 fathom line on the northern side of the Gulf to the deepest water close to the Yucatan banks, say to the depth of 2000 fathoms, is a distance of 200 miles, which gives a slope of five-ninths to 200, and may be considered practically as a plane surface. The 2000 fathom area has received the name of "Sigsbee Deep," after its explorer. The Yucatan Channel with a depth of 1164 fathoms has a cross-section of 110 square miles while the Straits of Florida, in its shallowest part opposite Jubiter Inlet, with a depth of 344 fathoms has a cross-section of only 10 square miles.

A view of the model reveals at once some important facts which a study of the plan conveys but imperfectly to the mind, and which were unsuspected before the great exploration of the Gulf was completed.

Among the more striking features displayed by the model to which Mr. Hilgard called attention, were:

1. The great distance to which the general slope of the continent extends below the present sea level before steeper slopes are reached. The 100 fathom line represents very closely the general continental line. The *massifs* of the Peninsulas of Florida and Yucatan have more than twice their present apparent width.

2. Very steep slopes lead from this submerged continental plateau to an area as great as that of the State of Georgia at the enormous depth of over 12,000 feet. There are three ranges on the Florida and Yucatan slopes extending in the aggregate from five to six hundred miles, along which the descent from 500 to 1500 fathoms (or 6000 feet), is within a breadth of from six to fifteen miles. No such slopes and correspondingly elevated plateaus appear to occur on the un-submerged surface of the earth —the suggestion presents itself, that while the latter have suffered atmospheric erosion, those which we are considering have not sensibly changed from the positions assumed in the mechanical shaping of the earth's crust.

3. The far protrusion of the Mississipi Delta towards the deep water of the Gulf, seems to give evidence to the Engineer, of the probably permanent success of the Mississippi Jetties, as delivering the silt of the river into water of so great depth that but few extensions will ever become necessary. In connection with the same feature, the strong indentation to the westward of the present mouths of the Mississippi, indicating the probable site of the original fracture between the two slopes of the Mississipi Valley deserves attention.

4. In regard to the problem of general ocean circulation in connection with the Gulf Stream, the most important feature is the shallowness and small cross-section of the Straits of Florida between the Peninsular and Bahama banks, having at the shallowest part a cross-section of 11 square miles, with a greatest depth of 344 fathoms only. From observations published in the Coast Survey Reports the average northwardly current of the warm water through this Strait is probably not greater than 2 miles per hour—certainly not more than 2½ miles. It is evident, at once, that the warm water which so greatly modifies the climate of Western Europe, cannot all be supplied by the flow through this small channel. The concentration of the warm surface current from the Gulf of Mexico gives to this vein of the general circulation a marked velocity, which is not found in other portions of the Atlantic, and which, being perceptible to the navigator, has given its name of "Gulf Stream" to the whole system of the northeasterly surface-flow in the Atlantic Ocean. It is now necessary to assume that the so-called Gulf Stream is largely reinforced by a general northerly current from the outside of the West India Islands.

SCIENTIFIC SOCIETIES OF WASHINGTON.

THE BIOLOGICAL SOCIETY.—The Society met in the Smithsonian Institution, Friday evening, March 11th, President Gill in the Chair. The discussion was renewed upon Mr. True's paper respecting suctorial organs. Mr. Seaman spoke of certain plants, such as the American Woodbine, which seem to mimic the suctorial organs of animals. Professor Riley drew attention to the suctorial anal pseudopod of caterpillars, and Mr. Goode to the peculiar provision for prehension in

the marsupials. Dr. A. F. A. King read a paper on Septennial Periodicity, drawing attention to the phenomena of menstruation, œstration in animals, gestation, contagions, epidemics and climax of fevers. He was partially supported by Mr. Goode, who said that since the lunar month of four weeks had such an important bearing upon tides, etc., there is no absurdity in supposing that the same cause may have been at work through myriads of years to bring about periodicity as indicated in the paper. Professor Riley, Mr. Ward, the President, Dr. Prentiss, and others, took the opposite side of the question.

THE ANTHROPOLOGICAL SOCIETY.—Major J. W. Powell, the President, being in the Chair, the following papers were read : "Politico-Social Functions," Lester F. Ward ; "The Savage Mind in the presence of Civilization," by Otis T. Mason. Mr. Ward first drew attention to the schism which ever manifests itself between theory and practice. Political philosophy taught in the schools is one thing, political rules and maxims of society are quite another. The speaker criticised the interpretation of the old legal school of politics as well as the modern naturalistic school. The latter, in holding that nature's fixed laws cannot be violated, forgot to include in nature the struggles of human reason. This is well exemplified in the anecdote concerning Plato. When about to flog a slave for stealing, the latter thought to get off by crying, "It is my fate to steal." The philosopher quickly reminded the slave that it was also his fate to get thrashed for his theft. The paper took the ground that Society was tending more and more to protection, and, from a large collection of statistics showed that gradually new interests were passing under control of the State. Major Powell warmly endorsed Mr. Ward's remarks, and affirmed that the conviction had been growing upon him in favor of the following view : Society begins with the kinship tie, passes on to the property basis of organization, and culminates in the evolution and protection of industries. Mr. Mason's paper was partly theoretical and partly practical. Under the first head it was maintained that the conflicts of the human family in all time had brought the different races of men face to face with higher and better methods, and from these much aid had been received in their own advancement. The practical portion of the paper related to the education of our Indians. The speaker had gone over the history of the subject, had corresponded with every respectable school and college in the country, and had collected the statistics of government operations from the Indian Bureau. The conclusion arrived at was that much had been wasted through ignorance of anthropological methods, and that the organization of a Bureau of Ethnology had been the wisest scheme the government had undertaken in this regard.

MICROSCOPY.

We have received from Dr. William Hailes, of the Pathological Laboratory, Albany Medical College, specimens of injected preparations cut with his improved microtome, which was figured and described on page 187, vol. 1, of "SCIENCE." The sections are from the kidney of the cat, and are very perfect, showing the excellence of his microtome and his own methods of manipulation. Dr. Hailes also sends us three photographs of magnified specimens of the Embryo of the Chick, taken, respectively 24, 36, and 72 hours after commencement of incubation. These photographs are highly interesting, and may be seen at our office by those pursuing such studies.

Messrs. Lennis and Duncker, both of Berlin, have published an interesting paper in the Zeitschrift für Mikroskopische Fleischschau on a new parasite with which they have met while performing their official duty. In examining pork for trichinæ they discovered a vermicular *diatomea* imbedded between the muscular fibres which they describe in the following terms : It is exceedingly thin and transparent, of a greyish color, and of about the size of the cyst-wall of a trichina.

Professor Leuckardt is inclined to consider its presence in the pork as accidental, and believes that it is of little importance to government inspectors of meat in their official work.

A WRITER in *Nature* makes the following observations on the minute structure of metals hammered into thin leaves which are quite instructive. Notwithstanding the great opacity of metals it is quite possible to procure, by chemical means, metallic leaves sufficiently thin to examine beneath the microscope by transmitted light. Such an examination will show two principal types of structure, one essentially granular and the other fibrous. The granular metals, of which tin may be taken as an example, present the appearance of exceedingly minute grains, each one being perfectly isolated from its neighbors by still smaller interspaces. The cohesion of such leaves is very small.

The fibrous metals, on the other hand, such as silver and gold, have a very marked structure. Silver, especially, has the appearance of a mass of fine, elongated fibres, which are matted and interlaced in a manner which very much resembles hair. In gold this fibrous structure, although present, is far less marked. The influence of extreme pressure upon gold or silver seems to be, therefore, to develop a definite internal structure. Gold and silver, in fact appear to behave in some respects like plastic bodies. When forced to spread out in the direction of least resistance their molecules do not move uniformly, but neighboring molecules, having different velocities, glide over one another, causing a pronounced arrangement of particles in straight lines.

A new edition of Messrs. Beck's catalogue corrected to the first of this month has been received. It is a work of 176 pages, well illustrated and appears to cover all the wants of a microscopist. Mr. W. H. Walmsley, the manager of the American branch of this house, informs us that there is a large demand for microscopes at this time, and that orders are in advance of their means of producing instruments. We notice some change in the prices and that the "Economic" has been raised to $40 including objectives. Messrs. Beck & Co. have been very successful in producing good models for their microscopes, and their workmanship is excellent. Both Mr. Beck and Mr. Walmsley are accomplished microscopists, and can thus anticipate the requirements of their customers.

ASTRONOMY.

VARIABLE STARS OF SHORT PERIOD.*

Under the above title, Professor Pickering has read before the American Academy of Arts and Sciences, the second of two papers, both of which are to be regarded as preliminary, rather than final discussions, upon the causes of variability in the light of fixed stars. In the preceding paper (Proc. Amer. Acad. XVI., 1.) the following classification of variables was made:

I. Temporary stars. Examples, Tycho Brahe's star of 1572, new star in Corona 1866.

II. Stars undergoing slight changes according to laws as yet unknown. Examples o Ceti and χ Cygni.

III. Stars whose light is continually varying, but the changes are repeated with great regularity in a period not exceeding a few days. Examples, β Lyrae and δ Cephei.

IV. Stars which every few days undergo for a few hours a remarkable diminution in light, this phenomenon

* Proceedings of the American Academy of Arts and Sciences, Vol. XVI.

recurring with great regularity. Examples, β Persei and δ Cephei.

In order to avoid all prejudice, the present discussions are made to depend entirely on the work of previous observers, while awaiting the completion of more precise observations now in progress at Harvard College Observatory.

An investigation was given in the article referred to above, of stars of the fourth class. It was shown that in the case of β Persei at least, the observed variations could be very satisfactorily explained by the theory that the reduction in light was caused by a dark eclipsing satellite.

Variables of the third class are considered in the present paper. Perhaps the most natural supposition of the variability of a star of short period, is that it is due to rotation around its axis. The difference in brightness of the two sides of a star, which such an explanation demands, may be due to spots like those of our sun, to large dark patches, or to a difference in temperature. The theory that variation is due to the absorption of a rotating mass of gas, does not appear probable for stars of the third class, since no evidence of absorption is in general shown in their spectra, beyond the appearance of lines such as are seen in our sun. For the stars of the second class, however, this view seems more reasonable, since many of them exhibit spectra which are strongly banded.

"One great advantage of the study of the stars by physical instruments, as the spectroscope and photometer, is that some clew is given to certain laws, for our knowledge of which we must otherwise depend on theoretical considerations alone. While the conclusions to be drawn from micrometric measurements are, in general, much more precise, and the effects of the errors can be more certainly computed, they fail entirely to aid us in studying such laws as are here considered. For example, the present investigation serves to study the following important problem in cosmogony, to which micrometric measures contribute nothing, and which can otherwise only be examined from the standpoint of theory.

If we admit a common origin to the stars of the Milky Way, a general coincidence in their axes of rotation seems not improbable, especially as such an approximate coincidence occurs in the members of the solar system. If the coincidence was exact, the direction must be that of the poles of the Sun, or, approximately, that of the pole of the ecliptic. On the other hand, since the stars of the Milky Way are supposed to be arranged in the general form of a flattened disc, we should more naturally expect that the axes of rotation would be symmetrically situated with regard to it, or would coincide with its shortest dimension. According to this theory, then, the axes of rotation would be directed towards the poles of the Milky Way. If now we suppose that a great number of variable stars were distributed over the heavens, it is evident that those seen in the direction of their axes would not appear to vary, since, as they turned, they would always present the same portions of their surfaces to the observer. Those at right angles to this direction would show the greatest variation, and, other things being equal, would appear to be more numerous, since they would be more likely to be detected. If then the axes are coincident, we should expect that most of these variable stars would lie along the arc of a great circle whose pole would coincide with their axes of rotation."

"Thirty-one stars are known whose period is less than 72 days. Of those, six belong to the fourth class, or that of *β Persei*, in which the variation is probably due to the interposition of an opaque eclipsing satellite. Of the remainder, seven may be excluded, since they are red, and may belong to the second class, or that of *o Ceti*. Eighteen remain, whose periods vary from less than a day to 54 days, and which may be placed in the third class. All lie within 16° of a circle whose pole is in R. A. 13h, Dec. + 20°. The distances of eleven are from 0° to 5°, of five at distances of 8° and 9°, one at 14° and one at 16°. The average distance is 5.°5, while, if the stars were distributed at random it should be 30°."

THE dome erected by Sir Henry Bessemer for the reception of his new and powerful telescope is now nearly finished. The telescope itself has arrived from the makers, and is now ready to be set up. It has been constructed on a plan devised by Sir Henry Bessemer, which it is believed will permit of telescopes being made on a much larger and more powerful scale than even the present one, which is the largest in the world. The present instrument is capable of being directed to any part of the heavens at the option of the observer. The upper portion of the dome is made of glass, with windows facing in every direction, and within there will be placed mirrors of silvered glass, which is part of the new invention, silvered glass being used in place of metal. The room and dome with its windows will revolve and keep pace automatically with every motion of the telescope, and the upper end of the instrument will reach a height of about forty-five feet.

WASHINGTON, *March* 24. W. C. W.

DISCREPANCY IN RECENT SCIENCE.

There are two classes of statements in current scientific literature that do not harmonize. Their teachings are opposite ; yet, the sayings are daily used by men who believe both to be true. One series of doctrines is known as the "Conservation of Energy;" the other, the "Nebular Hypothesis." The structure of nature rests on one, while the history of cosmic evolution is based on the other. Then they should agree. Men are fascinated with cosmogony, and for ages have sought the laws by which the Universe developed. This research culminated in the existing Nebular Hypothesis. Other fields of study were opened, man scrutinized his environment, analyzed matter, searched for its ruling laws and summed up results in the doctrine of the Conservation of Force. Now the laws by which nature was in the past evolved, and is in the present governed, must be, and are the same. Such does not seem to be the teaching of some late popular books on science.

By a generalization of late research it is annnounced that the Universe is a unit. All suns visible in the telescope are composed of similar material, since they emit light, having like properties, and are dominated by the same laws of gravity and motion as rule the solar system.

Like matter, like laws, is the postulate of nature for all time. Some scientists ignore this apparent truth, as will be seen in comparing ideas advanced in recent works.

The fundamental axiom in the law of the interaction of force is, that when one mode of energy appears, another vanishes, and vice versa.

No form of force can become sensible without the retirement of another of equal intensity. This mutual displacement never ceases for an instant, and the system of nature is kept up by the flow, interchange and conversion of force. Conservation is the law of energy, and no one force can long act without waning and giving rise to another. Gravity, motion, electricity, magnetism, chemism, heat and light, are forms in which energy exists; yet one never can work eternally by itself, but must suffer conversion into another mode of power. Motion in molecules evolves heat, and heat acting upon still molecules appears as motion. Chemism acts, gives rise to heat and in doing so expires ; or it may exhaust its energy in conserving electricity, which in turn may develop into heat. Numberless like instances might be given to prove the conservation of energy, were they necessary, but they are not; this great law is universally accepted by students of nature throughout the world, and the closest reasoner cannot find objection to this deduction of science. Among many facts revealed by the discovery of the laws of force, one only is

here sought to be made prominent, that relating to the evolution of heat. Heat cannot come of itself; some other mode of energy must precede it. Suppose all matter in existence to be dissociated, resolved to gas so attenuated that no two atoms touch. It would have "potency" for future development of every form of force, but at that time only one would be in existence—gravity. It could reign supreme only for an instant; obeying the law, it would suffer "conservation," and give rise to motion.

Hence, motion is the second mode of energy, and all the heat that ever existed came later. The only sources of heat known are motion and chemical action, itself a most rapid motion. Gravity caused the movement of original atoms, bringing them near enough to be within the influence of affinity, which acting, conserved heat, the fourth form of force awakened in the evolution of atoms hitherto separated. Or a little heat might have been derived from collision of atoms not having affinity; in either case heat had antecedent forces. Heat is not a primal affection of matter, but secondary; being always preceded by gravity and motion. And molecules must be separated by space in order that gravity can cause motion to appear and vanish in heat. It is not conceivable that primordial dissociated matter should have obeyed any impulse at first, save gravity, then motion, then Chemism, then heat and subsequently all other states of force.

The Nebular Hypothesis seeks to account for the evolution of all solar systems from primordial dissociated matter, requiring as Helmholtz says: "Several cubic miles to weigh a single grain." Nearly all physicists accept this theory, and admit that all existing matter was once in this condition of gas. It seems, by reason of known laws of matter, to be true. Thus, no two atoms coalesced; they were as far apart in proportion to their diameters, as the Sun and Polaris. No ascertained law of nature disputes this theory; and within limits of human knowledge, it must be so. Matter dissociated is in its most primitive condition; and nature begins in simplicity and develops complexity. Matter in fluid states is complex, and shows itself to have been wrought by force. All analogy points to the fact that at one time in the history of matter, its atoms were entirely separated; in which condition no force whatever save gravity was in existence to act thereon.

Yet, strange to say, some advocates of the nebular theory teach that this rare gas was intensely hot! They call it "fire mist,[1]" and aver that it was hotter than the sun is now! We read[2]; "There was a time when the materials composing it (the Universe), were masses of glowing vapor," and "we find that the further we go back into time the hotter the sun must have been. Since we know that heat expands all bodies, it follows that the sun must have been larger in past ages than it is now, and we can trace back this increase in size without limit. Thus we are led to the conclusion that there must have been a time when the sun filled up the space now occupied by the planets, and must have been a very rare mass of glowing vapor." True, the materials of the sun extended into a ball of gas thousands of millions of miles in diameter, far lighter than hydrogen; but the gas was intensely cold. No law of matter or force known to man; nor any analogy in nature leads to the conclusion that the primitive cosmical sphere of atoms was hot. It was cold and dark, neither chemism, heat, or light appeared until gravity made conservation in motion, making chemical action possible. Affinity must have been slow at first, so that heat could not have appeared until after ages of chemical and molecular activity had expired, and heated fluid nuclei begun to condense and shine. The original cosmical mass was as dark, cold and silent as interstellar space is now, and "fire mist" never had a place in nature. If the primeval "glowing vapor" ever existed, then the greatest monument ever reared by man, the "Law of Interaction of Force" falls crumbling to final ruin.

EDGAR L. LARKIN.

NEW WINDSOR OBSERVATORY, Ill., *March* 21, 1881.

[1] Winchell's Geology of the Stars.

[2] Newcomb and Holden's Astronomy, p. 494.

NOTES.

SOLUTION OF STARCH.—Zulkowsky proposes to make starch perfectly soluble in water by heating it to 190° C. along with glycerine. This process is most successful with potato-starch, less so with wheat-starch, and very difficult with rice-starch.

SALICYLIC ACID IN TEXTILE MANUFACTURES.—Dr. F. von Heydon recommends salicylic acid to be applied in dilute solution to woollen yarns, and to be mixed with sizes to prevent mildew, unpleasant smells, &c. Five grms. acid are sufficient for a litre of size.

ACTION OF HYDROCHLORIC ACID UPON METALLIC CHLORIDES.—The chlorides which are rendered more soluble by hydrochloric acid are divided into two groups; the one (*e.g.*, mercuric chloride) exceedingly soluble in the concentrated acid form with it crystalline compounds; the other (*e.g.*, silver chloride) very sparingly soluble, even when heated, yield on cooling the chloride considered as anhydrous.—A. DITTE.

ACTION OF CAUSTIC LIME UPON PURE SOLUTIONS OF SUGAR AND RAW BEET-JUICE.—If free alkalies or alkaline earths are added to a solution of sugar the rotation which sugar occasions in polarized light decreases, and is restored on neutralizing the alkaline liquid with acetic acid.—F. DESOR.

NEW STUDIES ON THE PART PLAYED BY BONE-BLACK IN THE SUGAR MANUFACTURE.—Free lime is almost entirely absorbed by bone-black. Salts of lime and potash are also absorbed to a certain extent. Potash and lime, the latter in saline form, promote each other's absorption.—H. PELLET.

CHEMICAL CHANGE OF STARCH ON EXPOSURE TO STEAM AT A HIGH PRESSURE.—A heat of 140° to 150°, and consequent pressure of 3½ to 4½ atmospheres convert 71 per cent. of starch into glucose. Dr. M. Stumpf considers that with the aid of 1 to 2 parts of acid per thousand saccharification may be carried so far as to render the use of malt unnecessary.

DECOMPOSITION OF SALTS BY LIQUIDS.—The laws of dissociation by heat, applicable to the decomposition of salts by pure water and saline acid solutions, apply also to decomposition by alcohols.—A. DITTE.

INFLUENCE OF THE SOIL UPON THE TANNIN OF OAK BARK.—A comparison was made between the bark of young oaks grown respectively upon sandy loams, upon peaty soil which had been once burnt, and upon a similar soil thrice burnt. The proportion of tannin was found higher in case of the peaty soils.—M. FLEISCHER.

INFLUENCE OF MANURES ON THE APPEARANCE OF DISEASE AND THE PROPORTION OF STARCH IN POTATOES.—Three plots dressed with stable manure showed 6, 6, and 5 per cent. of diseased tubers. Plots where superphosphate and small quantities of ammoniacal superphosphate were used did not increase the percentage, but with larger proportions of the latter it rose to 8 per cent. Chili saltpetre was attended by a proportion of 11 per cent., and when used as a top-dressing 12 per cent.—M. MARCKER.

INFLUENCE OF BORAX ON THE DECOMPOSITION OF ALBUMEN IN THE ANIMAL ORGANISM.—The ingestion of borax is found to increase the decomposition of albumen.—M. GRUBER.

TITRATION OF BISMUTH SUBNITRATE.—This process is based upon the facts that as to 9·9074 grm. of monohydrated sulphuric acid correspond to 1 grm. anhydrous nitric acid these two weights of acids will require the same quantity of alkali for exact saturation, and that bismuth subnitrate is capable of yielding all its nitric acid to an excess of alkali on boiling.—E. BAUDRIMONT.

BOOKS RECEIVED.

THE POWER OF MOVEMENT IN PLANTS. By CHARLES DARWIN, LLD., F. R. S., assisted by FRANCIS DARWIN. D. Appleton & Co., Bond street, New York. 1881.

The announcement of a new work from Dr. Darwin brings joy to the heart of every naturalist, and the present volume will be much cherished by botanists, because it introduces a line of research which is comparatively unworked and one which promises interesting results to those who have time and patience to continue it.

The object of Dr. Darwin in writing this book was to describe and connect together several large classes of movements common to almost all plants, which is chiefly noticed in climbing plants, the tips of which revolve, bending successively to all points of the compass. This movement is called by Darwin *circumnutation*, and a plant is said to *circumnutate*.

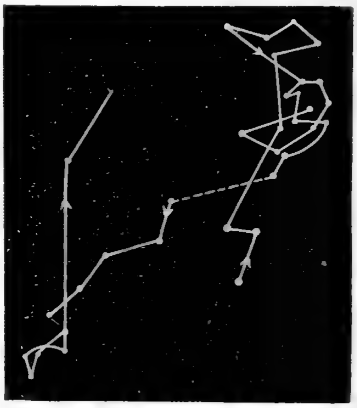
In the course of the present volume it is shown that all growing parts of every plant are continually circumnutating, though often on a small scale. Even the stems of seedlings before they have broken through the ground, as well as their buried radicles, circumnutate, as far as the surrounding earth will permit. In this universally present movement we have the groundwork or basis for all the varied movements which are essential to the requirements of plant life.

Thus the great sweeps made by the stems of twining plants, and by the tendrils of other climbers, result from a mere increase in the amplitude of the ordinary movement of circumnutation. The position which young leaves and other organs ultimately assume is acquired by the circumnutation movement being increased in one direction. The leaves of various plants are said to sleep at night, and it is shown that their blades then assume a vertical position through modified circumnutation in order to protect their upper surfaces from being chilled through radiation. The movements of various organs to or from the light are all modified forms of circumnutation, as are the equally prevalent movements of stems, &c., toward the zenith, and of roots toward the centre of the earth. The method of observation employed by Darwin is thus explained:

" Plants growing in pots were protected wholly from the light, or had light admitted from above, or on one side as the case might require, and were covered above by a large horizontal sheet of glass, and with another vertical sheet on one side. A glass filament, not thicker than a horsehair, and from a quarter to three-quarters of an inch in length, was affixed to the part to be observed by means of shellac dissolved in alcohol. The solution was allowed to evaporate until it became sufficiently thick to set in two or three seconds, and it never injured the tissues, or even the tips of tender radicles. To the end of the glass filament an exceedingly minute bead of black sealing wax was cemented, below or behind which a bit of card with a black dot was fixed to a stick driven into the ground. The weight of the filament was so slight that even small leaves were not perceptibly pressed down. The bead and dot on the card were viewed through the horizontal or vertical glass plate (according to the position of the object), and when one exactly covered the other, a dot was made on the glass plate with a sharply-pointed stick dipped in thick Indian ink. Other dots were made at short intervals of time, and these were afterward joined by straight lines. The figures thus traced were therefore angular, but if dots had been made every one or two minutes, the lines would have been more curvilinear, as occurred when radicles were allowed to trace their own courses on smoked glass plates. To make the dots accurately was the sole difficulty, and required some practice. Nor could this be done perfectly when the movement was much magnified, say 30 times and upward, yet even in this case the general course may be trusted."

To make this clear we give a diagram of one of the most simple of Darwin's experiments, and the following further explanation:

"*Brassica oleracea*" (cruciferæ.)—Radicle. A seed with the radicle projecting .05 inch was fastened with shellac to a little plate of zinc, so that the radicle stood up vertically; and a fine glass filament was then fixed near its base, that is, close to the seed coats. The seed was surrounded with little bits of wet sponge, and the movement of the bead at the end of the filament was traced (see figure) during sixty hours. In this time the radicle increased in length from .05 to .11 inch.



Brassica oleracea, circumnutation of radicle traced on horizontal glass from 9 A. M., January 31, to 9 P. M., February 2. Movement of bead at end of filament magnified about forty times.

We trust that those who would take up this subject will consult this work, as the amount of detail there given is most essential to a thorough comprehension of this study, but in case any of our readers are unable to do so, the explanation we have given may suffice.

The chapters on the sleep of plants are most interesting and instructive, and many discoveries relating to this phenomenon are presented.

There are also certain movements in plants which are not due to circumnutation, such as when a leaf of the Mimosa is touched it suddenly assumes the position as when asleep, but this movement occurs from a different cause to that which produces the sleep of plants. The sleep movement of plants is due to modified circumnutation; this would not happen from a touch.

Space will not permit us to further describe this important branch of the subject, but we hope on a future occasion to again refer to it, and offer some illustrations of the most striking instances. But as Mr. Darwin observes, it is impossible not to be struck with the resemblance between the sleep movements of plants and many of the actions performed unconsciously by the lower animals. With plants an extraordinarily small stimulus suffices; and even with allied plants one may be highly sensitive to the slightest continued pressure, and another highly sensitive to a slight momentary touch. But the most striking resemblance is the localization of their sensitiveness and the transmission of an influence from the excited part to another which consequently moves. Yet plants do not of course possess nerves or a central nervous system; and we may infer that with animals such structures serve only for the more perfect transmission of impressions, and for the more complete intercommunication of the several parts.

INFLUENCE OF THE VENTILATION OF MUST UPON ALCOHOLIC FERMENTATION.—E. Rotondi considers that ventilation mechanically promotes the decomposition of the sugar, and acts also chemically, because the albumenoid bodies are transformed into more diffusible matters, and because oxygen by increasing the quantity of ferment indirectly intensifies the fermentation.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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It has been well said, that the poorest day that passes over us is the conflux of two eternities: it is made up of currents that issue from the remotest past, and flow onward into the remotest future.

On the 27th of June, 1829, an event took place which was to have a marked influence on the intellectual development of the United States, for on that day James Smithson died at Genoa, Italy, bequeathing his whole fortune to the citizens of the United States, in trust, "*for the increase and diffusion of knowledge among men.*"

On the 6th of December, 1838, President Van Buren had the satisfaction of announcing to Congress that the claim of the United States to this legacy had been fully established, and that the money had been received by the Government.

The question then arose, what plan could be devised to carry out the intentions of the testator. In other words, how could "the increase and diffusion of knowledge among men" be best accomplished.

One of the first proposals for utilizing the Smithsonian fund was a scheme of founding a university of high grade, to "teach Latin, Greek, Hebrew, Oriental languages, and other branches of learning, including rhetoric, poetry, laws of nations, &c." Fortunately, such counsel did not prevail, and after nearly eight years of debate, and even a proposal to return the money to England being voted on, a bill was passed by Congress organizing the Smithsonian Institution on its present basis.

Such, briefly stated, was the origin of the Smithsonian Institution, and in memory of its founder the present Secretary, Professor Spencer F. Baird, directed Mr. William J. Rhees to compile a biography* of James Smithson, this work being one of the most recent publications of the Institution.

The general scope of this work is good, and it must be admitted that some account of the establishment of this Institution was called for. We must, however, express our regret that such an elaborate description of Smithson's aristocratic connections was presented, especially as the history would have been equally complete without this superfluous addition. The connection of the "proud" Dukes of Northumberland and Somerset with Smithson was hardly of a nature to be recorded in a form which should constantly bring the facts before the present generation and posterity.

The circumstances of Smithson's birth cannot be ignored, but there is no reason why they should be paraded before the public; we therefore would have dispensed with the portrait of the first Duke of Northumberland in this volume, and relegated the history of his life and death to the highest shelf in the Smithsonian Library.

Stript of such surroundings, the memory of Smithson must ever be dear to the people of this country. He was a man thoroughly imbued with the spirit of true science, and an active and industrious laborer in one of the most interesting and important branches of research—"mineral chemistry." His happiest hours were spent in the laboratory, where he carried on a series of experiments, which were recorded in the transactions of the Royal Society of London and other scientific journals of the day. Such being the direction of Smithson's scientific pursuits, we trust that the advancement of the physical sciences may claim the attention of the officers of this institution, and that they may be more duly represented in future reports.

Since the death of Smithson, Chemistry has attained a higher rank among the exact sciences. New methods and instruments of analysis have been introduced, while other branches of science have advanced at an equal ratio. New means "for the increase and diffusion of knowledge among men," have come to light, and among these the production of improved scientific manuals, and the increased number and excellence of scientific periodicals and journals, may be mentioned as having largely contributed to such results. Science at the present day is no longer monopolized by a select few, but is claimed as the common heritage of the thousands who have the intelligence to appreciate its value in developing the highest faculties of man.

Thomas Carlyle considered that "to know the divine laws and harmonies of this Universe must always be the highest glory of a man, and not to know them the highest disgrace for a man." This Journal represents one of the latest attempts to place at the disposal of all interested in scientific pursuits and human progress, a weekly journal worthy of the subject discussed. We are glad to find that our efforts have been appreciated, and the constant receipt of letters of welcome, co-operation and aid, increases our hopes for the future. Among our latest subscribers, we find three residing in Japan, one in Lucknow, India, another in New Zealand, and the directors of the Royal observatories of Brussels, Lisbon, and Rome have added their names. If "Science" is thus in demand in foreign countries, we trust to find our home subscription list rapidly increase, which will enable us to enlarge and improve the journal in various ways, thus adding to its usefulness.

Lord Brougham observed, that to instruct the people in the rudiments of philosophy, and to obtain

* James Smithson and his bequest, by William J. Rhees, published by the Smithsonian Institution, Washington, 1880.

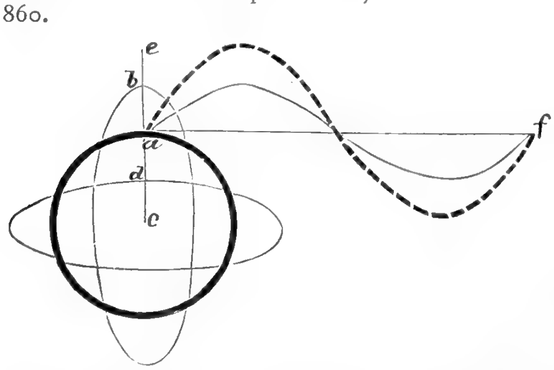
for the great body of our fellow creatures that high improvement, which both their understanding and their morals fit them to receive, is an object sufficiently brilliant to allure the noblest ambition. Without claiming such lofty aspirations, the promoters of "SCIENCE" yet look forward to the time when their efforts to establish this journal may be recognized as at least a step in that direction.

ON THE AMPLITUDE OF VIBRATION OF ATOMS.

PROF. A. E. DOLBEAR, TUFTS COLLEGE, MASS.

There is now sufficient evidence for the belief that the kinetic energy of atoms and molecules consists of two parts, one of which is the energy of translation or free path, the other of a change of form due to vibrations of the parts of the atom or molecule toward or away from its centre of mass. The pressure of a gas is immediately due to the former while the temperature of the gas depends solely upon the latter. These two forms of energy must indeed be equal to each other in a gas under uniform conditions; for if one exceeded the other in energy when there is as free a chance for exchange as among the atoms of a gas, there would result an increase of pressure on the one hand, or an increase of temperature on the other. Now the kinetic energy of a mass m and velocity v is expressed by $\frac{m v^2}{2}$ and applies as well to an atom as to a musket bullet, and if we take the mass of the hydrogen atom as unity and employ the calculated velocity of hydrogen atoms at 0° Cent. and 760 mm. pressure, namely 1860 metres per second, the energy will be $\frac{(1860)^2}{2}$.

We know also how many times the hydrogen atom vibrates per second, by dividing the velocity of light per second by some chosen wave length λ; so that $n = \frac{v}{\lambda}$. If attention be now directed to the vibrating atom possessing the same energy as in the free path movement, it will be seen that its *velocity of vibration* must also be equal to 1860 metres per second. But vibratory velocity is the product of a number n into an amplitude a, so that $v = na = 1860$.



Adopting the vortex-ring theory of matter, the dark ring represents the atom which, when executing its simplest vibration assumes consecutively the conjugate ellipses and any point a in the circumference will move over the line $b\ d$, the latter distance constituting the amplitude of the vibration. The limits to this movement must clearly be between $b\ d = 0$ when there is no vibration, the absolute zero of the atom and $c\ e$ which can never exceed $\frac{1}{2}\ \pi r$ and indeed must always be less than that value; for when half the major axis of the ellipse is equal to that it has become a straight line. As atomic vibrations result in undulations in ether it is evident that amplitude $b\ d$ will give an undulation $a\ f$ shown in continuous line, while maximum amplitude $c\ e$ would give same wave length shown in broken line. The greater the actual thickness of the ring the less must be the possible maximum amplitude.

The amplitude then becomes comparable with the diameter of the atom, and in this discussion the assumed diameter is the one given by Maxwell, namely .0000005 mm.

The numerical value of $\frac{1}{2}\ \pi r$ for such a diameter is .0000004 mm. which represents the theoretical maximum amplitude for a hydrogen atom.

If any hydrogen wave length be taken, say $C =$ .0006562 mm. the ratio of wave length to maximum amplitude is $\frac{.0006562}{.0000004} = 1640$, that is wave length is 1640 times such amplitude. But hydrogen C is not the fundamental vibration, but according to Stoney is the 20th harmonic of a fundamental having a wave length of .013127714 mm. and $\frac{.013127714}{.0000004} = 32819$. That is, it is 32819 times greater than the amplitude.

Now, Sir William Thomson, in his calculations on the amount of energy in the ether, assumed that the amplitude should not exceed one-hundredth of the wave length, but that value is evidently very many times too large. An undulation with the wave length of this fundamental for hydrogen is nearly twenty times longer than the longest one that can be seen; and as the sensation of light depends upon wave length and not upon amplitude, or what the energy of the ray, it follows that Dr. Drapers' deductions concerning the temperature of bodies beginning to be luminous will not necessarily apply to gases, for when extra energy is imparted to the atoms of a gas it is the amplitude of their vibrations that is affected, and if the impacts are sufficiently frequent some of the harmonic vibrations may appear continously, but they will not thereby necessarily indicate a higher temperature, but show that the energy is distributed in two or more periods, some of which have resulting undulations which may be seen; but this will depend upon the density of the gas. Suppose a body capable of vibrating a times per second for its fundamental, be struck b times per second; then will the rate of vibration be interfered with $\frac{a}{b}$ times. If b be less than a, then will the fundamental vibration have more than its required interval between impacts, and a certain number of these fundamental vibrations will be made per second. If b be equal to a, then, after the first impact, a will vibrate in its own period with increasing amplitude, without interference. If b be greater than a then will the impacts interfere in all phases of the vibrations, the fundamental will be destroyed, and only some harmonics and irregular vibrations will be possible; but the number of impacts

per second depends upon the density, and in solids and liquids this secures the destruction of the fundamental vibrations as the energy of vibration is increased, at the same time developing the multitude of irregular ones shown in the spectrum; while in a gas the number of impacts per second is many times less than the regular rate of vibration, and this secures the time for either fundamental or harmonics, and the consequent spectra. The number of vibrations n the hydrogen atom makes when the wave length is .0131277 mm. will be $n = \frac{v}{\lambda} = \frac{3 \times 10^{11}}{.131277} = 2286 \times 10^{10}$.

Let v^1 represent the velocity in free path motion of the atom at 0° Cent. and 760 mm. pressure = 1860-000 mm. Their amplitude a will equal $\frac{v^1}{n} = \frac{1860000}{2286 \times 10^{10}}$ $= 8134 \times 10^{-11}$ m. Comparing this with the diameter of the atom $\frac{8134 \times 10^{-8}}{5 \times 10^{-7}} = .162$. That is the amplitude is equal to .162, the diameter of the atom at 0°.

Assuming a temperature higher than this, say 273° Cent., then the energy of the atom in its free path motion compared with that it has at 0° will be as $\sqrt{2}$: 1 and 1 : $\sqrt{2}$: : 1860 : 2630 m. per second, and as be fore amplitude a will equal $\frac{v^1}{n} = \frac{2630000}{2286 \times 10^{10}} =$ 115×10^{-7}. This compared with the diameter of the atom gives $\frac{115 \times 10^{-7}}{5 \times 10^{-7}} = .23$. That is, the amplitude is equal to .23 the diameter at 273° Cent., a difference of .068 for 273°.

With same data the maximum temperature of the hydrogen atom may be calculated for as

$$(.162)^2 : (.7854)^2 : : 273^\circ : 6419^\circ$$

which would be the highest temperature the atom could have if it could have such an amplitude, and this will be reduced as the thickness of the ring increases. Any additional energy the atom would receive could not possibly heat it but would be expended either in rotating it or in giving to it a free path motion. In like manner the amplitude for a single degree is found to be .0098 diameter, or very nearly one-hundredth the diameter.

For other atoms than hydrogen when they have the same energy their amplitude must vary inversely as their mass, so that for oxygen the amplitude at 273 would be $\frac{.162}{16} = .01$ its diameter, and its maximum temperature will be $6419 \times 16 = 102704^\circ$ Cent., a number altogether too high for the same reason as was given for hydrogen, namely it assumes that the ring has no thickness.

If these computations have any value they may be applied to the solution of the temperature of the sun.

The elements having the greatest density must have the highest maximum temperature. In the sun twenty-five elements have been determined spectroscopically and the average density of these twenty-five is 63. Now on the hypothesis that these elements exist in equal quantities in the sun, which is not very probable, the maximum temperature of that body woulp be about 400000° Cent.

As at absolute zero each atom is quite independent of every other atom, that is, matter has not a molecular structure, so, at certain high temperatures that differ for different substances, all molecular groupings must be broken up and the atoms are quite dissociated from each other, and this dissociation must occur before the maximum temperature is reached; it would appear that whenever at the sun the temperature approached its maximum, then the elements would be elementary, uncombined, and if compounds are observed or appear probable from phenomena witnessed, that will be the best evidence that the temperature is decidedly lower than the above figure. For hydrogen the dissociation temperature is only about 700° Cent. which is only about one-ninth its maximum.

MARSH'S ODONTORNITHES.*

Were there no other proofs of his zeal and success in extending the bounds of knowledge, the writer of this magnificent monograph would be famous as—for ten years at least,—the sole discoverer, describer and possessor of the remains of Extinct Toothed Birds of North America.

It may befall almost any diligent explorer to find the remains of some species previously unknown, but few have had— or so well-deserved—the privilege of presenting to the world a new series of facts embodying a new idea, at once easily appreciated by the many, and serving the few as material for profound consideration. That a bird with teeth is, most literally, a *rara avis*, may be conceded without extensive acquaintance with either Latin or Ornithology; on the other hand, it is probable that naturalists have not yet wholly realized the import of this fulfillment of a prediction which might have been made legitimately—though we are not certain that it ever was —at any time during the last twenty years.

Aside from the Appendix, the present volume embraces detailed descriptions of the bones and teeth of *Hesperornis* and *Ichthyornis*; a general description of the "Restoration" of each genus; and a "Conclusion" embracing the author's views upon the taxonomic relations, and probable evolution of these two forms, together with *Archæopteryx*.

The following are the principal characteristics of the two American genera, chiefly as recapitulated upon p. 187. In *Hesperornis*, the articular ends of the vertebral centra are saddle-shaped, as in recent birds; in *Ichthyornis* they are biconcave, as in many fishes: *Ichthyornis* has a prominent sternal keel for the attachment of the muscles of the well-developed wings; in *Hesperornis*, the sternum is without a keel, and each wing is represented by only a rudimentary humerus: the wing-bones of *Ichthyornis* have tubercles evidently for the attachment of feathers; no signs of feathers have been observed with *Hesperornis*, but they doubtless were present in life: in both genera, the caudal vertebræ are few, so that the bony tail is short as in recent birds: in both, the mandibular rami seem to have remained permanently ununited by bone: in both, as indicated by casts of the cranial cavity, the prosencephalon was narrower than in recent birds of

* Odontornithes: A Monograph on the Extinct Toothed Birds of North America; with thirty-four plates, and forty woodcuts. With an Appendix giving a Synopsis of American Cretaceous Birds. By Othniel Charles Marsh, Professor of Palæontology in Yale College, Memoirs of the Peabody Museum of Yale College, vol. 1; pp. 201. This memoir will also form vol. vii, Survey of the 40th parallel.

similar size, a point of much interest, in view of what has been noted by Prof. Marsh with regard to the brains of extinct Mammals: finally, both forms had *well-developed teeth in both jaws*, but those of *Hesperornis* were implanted in a continuous groove, while those of *Ichthyornis* had separate sockets.

Prof. Marsh calls attention to the peculiar combination, in *Ichthyornis*, of a low feature—the biconcavity of the vertebræ—with a comparatively high method of implantation of the teeth, and adds: "Better examples than these could hardly be found to illustrate one fact brought out by modern science, that an animal may attain great development in one set of characters, and at the same time retain other low features of the ancestral type. This is a fundamental principle of Evolution."

Naturally, the teeth are described and figured with especial fullness and accuracy. Their general features are distinctly reptilian, as would have been inferred. Curiously enough, in neither genus does the dental series reach the tip of either jaw, and, in *Hesperornis*, "the extremity of the premaxillary bone, back to the nasal openings, has its surface pitted with irregular vascular foramina, indicating. apparently, that it was once covered with a horny bill, as in modern birds." P. 8.

With the exception of *Archæopteryx*, all the known odontornithic remains are in the Museum of Yale College, but their discoverer is clearly of opinion that more are to be found:

"These three ancient birds, so widely different from each other, and from all modern birds, prove beyond question the marvellous diversity of the avian type in mesozoic time, and also give promise of a rich reward to the explorer who successfully works out the life-history of allied forms, recorded in ages more remote." P. 189.

He even ventures to define the leading features of the, at present, hypothetical progenitor of the entire group of birds: "In the generalized form to which we must look back for the ancestral type of the class of birds, we should therefore expect to find the following characters: Teeth in grooves; vertebræ biconcave; metacarpal and carpal bones free; sternum without a keel; sacrum composed of two vertebræ; bones of the pelvis separate; tail longer than the body; metatarsal and tarsal bones free; four or more toes, directed forward; feathers rudimentary or imperfect; quadrate bone free." P. 188.

As compared with this generalized form, our modern birds, while endowed with intense functional activity, and in some structural features—especially as to their true dermal appendages—a most highly specialized group, are nevertheless, odontologically considered, degenerated and retrograded creatures.

The general bearing of the facts given in this memoir upon the question of evolution has been well stated by Prof. Marsh upon a previous occasion.

"*Compsognathus* and *Archæopteryx* of the Old World, and *Ichthyornis* and *Hesperornis* of the New, are the stepping-stones by which the evolutionist of to-day leads the doubting brother across the shallow remnant of the gulf, once thought to be impassable."

So far we have had to deal either with facts, or with hypotheses based upon those facts and warranted by the prevailing opinions respecting evolution in general. There remains to be considered the bearing of these same facts upon the zoological relations of the toothed birds to the rest of the class. Here there is room for very wide disagreement, and the only point, perhaps, upon which all seem to be in accord, is that the Birds, as a whole, form a *class* of vertebrates, whether or not they should be combined with the reptiles as a super-class or sub-branch—Sauropsida.

The advantages of employing a single technical term like *odontornithes* in place of *aves dentatæ* or *toothed birds* will be generally conceded, and the use of the term as a convenient designation of certain forms need not imply more than is implied by the words *swimmer*, *flier*, *apoda*, etc. The real question is, dó the toothed birds constitute a natural subdivision of the class Aves, comparable for instance with the Marsupials among the mammalia? If not do they constitute an order or a family, or, finally, are they—or some of them—simply representatives of two or more natural groups, differing from the other members of those groups, and associated together, by the possession of teeth?

In a natural classification, we expect to find animals collocated either because they agree in many particulars, or because they have in common one or, more features of primary importance For example notwithstanding their immense variety in size, form, habit, existing birds present a remarkable uniformity of structure, even in some apparently insignificant details. On the other hand, although *Amphioxus* differs from all other Vertebrates in so many respects that nearly all generalizations as to the branch must be accompanied by a qualification, yet it shares with the rest a developmental feature and a general arrangement of organs which keep it within the branch and separate it from all other animals, excepting perhaps the Ascidians.

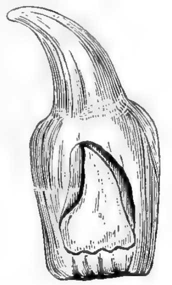
Prof. Marsh regards *Archæopteryx*, *Hesperornis*, and *Ichthyornis*, as the representatives of as many orders of the subclass Odontornithes, to which he applies the names Saururæ, Odontolcæ, and Odontotormæ. The first of these names had been employed already by Hæckel and Huxley, who, however, had made the Saururæ, Ratitæ(ostrich, etc.) and Carinatæ, (all other birds) subclasses of the class Aves. Marsh does not say what he thinks should be done with the Ratitæ, but if he is correct in his opinion (p. 3.) that "*Hesperornis* and *Ichthyornis* differed more from each other than do any two recent birds," it would seem to follow that the Ratitæ can no longer constitute a subclass of the recent and toothless birds.

In the condensed statement of the characters of the orders (p. 187) it is shown that we are unacquainted with the mode of implantation of the teeth of *Archæopteryx*, with the form of its vertebræ and sternum, and with the extent of union of the mandibular rami. The characters enumerated are the *presence of teeth*, *small wings*, *separate metacarpalia and a bony tail longer than the body*.

It will be seen that, excepting the teeth, any generalization respecting the Odontornithes as a whole, must be accompanied by a qualification respecting one or two of the orders. Prof. Marsh points out that the three groups present unequal degrees of affinity. But even if we exclude *Archæopteryx*, the only characters which are at the same time common to the

Odontolcæ, and Odontotormæ and absent from recent birds, are the *narrowness of the prosencephalon*, *the persistent separation of the mandibular rami and the presence of teeth.*

That the presence of teeth has been regarded by Prof. Marsh as the principal—if not the only essential—characteristic of the Odontornithes, is indicated by the following passages from the present work, or from previous papers.



TOOTH OF *Hesperornis Regalis*, SHOWING GERM OF YOUNG TOOTH.

"Both of these types possessed teeth, a character hitherto unknown in the class of birds, and hence they have been placed by the writer in a separate sub-class, the Odontornithes." P. 3.

"That *Archæopteryx* belongs to the Odontornithes, the writer fully satisfied himself by a personal examination of the well-known specimen in the British Museum. The teeth seen on the same slab with this specimen agree so closely with the teeth of *Hesperornis*, that the writer identified them at once as those of birds and not fishes." P. 186.

In speaking (p. 191) of the "bird remains found in the Green-Sand deposits of New Jersey," our authoı says; "as neither jaws nor teeth have yet been detected, it is at present impossible to say whether the Eastern species belong to the Odontornithes."

Before the discovery of the teeth, he had characterized the *Hesperornis regalis* as a "gigantic diver related to the Colymbidæ." His preliminary description of the same bird had been to the same effect, with the addition "that it differs from the Colymbidæ so widely in the structure of the pelvis and posterior limbs as to demand a place in at least a separate family."

In the present publication, however, our author is of opinion that "the struthious characters seen in *Hesperornis* should probably be regarded as evidence of real affinity, and in this case *Hesperornis* would be essentially a gigantic swimming ostrich." P. 114.

That Prof. Marsh's opinion as to the taxonomic value of the teeth is shared by zoologists generally, is shown—at least negatively—by the absence of dissent from his own views and from those of such reviewers as Newton and Woodward. The former speaks of the "teeth, whence the *Ichthyornis* has been made the type of a distinct sub-class." The latter, writing of the same genus, says: "The possession of teeth and biconcave vertebræ, although the rest of the skeleton is entirely avian in type, obviously implies that these remains cannot be placed in the present group of birds, and hence a new sub-class, Odontornithes is proposed for them." In the added note, respecting *Hesperornis*, Woodward does not state whether he was then aware that the vertebræ of that genus lacked the biconcave character. Hence it is not certain whether he would regard it as an odontornith by reason of the teeth alone.

Prof. Huxley does not distinctly mention the degree of separation of the toothed birds from the rest, but he says that the *Hesperornis regalis* "in a great many respects is astonishingly like an existing diver or grebe, so like it indeed, that had this skeleton been found in a museum, I suppose—if the head had not been known—it would have been placed in the same general group as the divers and grebes of the present day."

So far as I am aware, no objection to the erection of a sub-class upon a purely dental basis, has been offered, even upon the part of some who have not usually been slow in critcising our author's conclusions.

Yet Prof. Marsh himself appears to be by no means settled in his conviction as to the taxonomic relations of the forms in question, since his "Conclusion" contains the following qualified expression of opinion: "For the present, at least, it seems advisable to regard the Odontornithes as a sub-class, and to separate them into three orders."

The above intimation of a willıngness to review this part of the subject removes the hesitation which one naturally feels in differing from the highest—and, in one sense, the only—odontornithological authority, and I therefore venture to offer certain considerations which seem to have been overlooked hitherto.

1. Are the other characters of the toothed birds such as to warrant their separation as a sub-class? In other words, can we conceive of *edentulous* Odontornithes as we have Vertebrates without vertebræ, and Edentates provided with teeth?

2. Why should the presence of teeth in certain birds be accounted of more taxonomic significance than the absence of the same organs in the members of other classes? The truly edentulous edentates are held to form merely families or sub-orders; the (toothless) turtles are commonly regarded as an order of reptiles; and Prof. Marsh himself has established the sub-order Pteranodontia, the "distinctive feature of which as compared with the other Pterosaurıa, is the absence of teeth."

3. If birds with teeth had been known to us at all times, or in the recent state, or in great number and diversity, is it probable that, the entire group having the rank of a class, we should have been led to form two primary groups, the Odontornithes and the Anodontornithes.

4. How would the question appear in case unmistakable evidences of teeth are found in the embryos of recent birds? That such signs will be sometime discovered can hardly be doubted, especially when the embryology of the ostrich is as well known as that of the common fowl. Some are even now of opinion that such structures have been seen. So cautious a compendium as Rolleston's Forms of Animal Life, says: "dental papillæ, with caps of dentine, have been observed in the embryos of Psittacidæ." Since, however, Prof. Marsh holds (p. 13) that the "vascular papillæ seen by St. Hilaire and others were apparently portions of the horny beak," we may consider the point unsettled.

5. May it not be that, in our natural surprise at the

HESPERORNIS REGALIS, MARSH.

unexpected presence of teeth in connection with an otherwise bird-like structure, we have overestimated the true taxonomic significance of the facts, and lost sight, for the moment, of our customs in other groups? May it not be, indeed, that we have been unconsciously affected by the phenomenal nature of most of Prof. Marsh's palæontological discoveries, and that we have not only been unduly impressed by the facts, but also influenced in some degree by the general admiration for the discoverer's achievements, so as to refrain from questioning his conclusions? Yet, as has been shown already, our author has kept his own mind open upon this yery point, and it is to be hoped that he may have the pleasure and the honor of discovering other forms of Aves dentatæ, affiliated in other respects to the several groups of existing birds, and held together only by their teeth.

Hereafter such problems as are involved in this memoir will be discussed more advantageously in the light of the considerations respecting the Evolution and Classification of Vertebrates which have been presented recently by Prof. Huxley.

So admirable is the present work as a whole that one shrinks from any criticism of details. Upon the following points, however, some improvement could, perhaps, have been made:

While insisting upon the lack of bony union of the ends of the mandibular rami in the American Odontornithes, our author makes contradictory statements in regard to the tissue by which they were joined during life. On pages 11 and 179 it is said to have been *ligament;* on page 123, and in the explanation of plate 1, *cartilage* is specified, while on page 112 the union is said to have been "as in serpents." Judging from the appearance of the surface shown in plate 1, fig. 4, the union was ligamentous rather than cartilaginous, but there may have been a mingling of the two kinds of tissue.

The date of the discovery of *Hesperornis* is given as November, 1870, on page 2, but as December on page 195.

It would have greatly facilitated references if there had been given in this volume a complete Bibliography of Odontornithology, together with a statement of the dates of discovery of the various forms, and the dates of their assignment to more comprehensive groups than species and genera. The synonymy as given under the species named in the Appendix does not quite meet this want.

In view of the aid which evolution has received from embryology, it would seem that even a special palæontological memoir like the present might have contained some expression of the author's expectation that light may sometime be thrown upon the problems involved by the careful scrutiny of the development of certain recent birds, notably the Struthionidæ.

B. G. W.

REPORT SUBMITTED TO THE ACADEMY OF MEDICINE ON THE SUBSTITUTION OF MARGARINE FOR BUTTER AND LARD IN THE PUBLIC ASYLUMS OF THE DEPARTMENT OF THE SEINE.—M. Riche finds that pure butter yields a quantity of fatty acids insoluble in water ranging from 86.5 to 88 per cent of the weight of the pure fatty matter, whilst in all the other fats and animal oils, and in almost all vegetable oils, there is from 95.20 to 95.80 per cent of insoluble fatty matter.

ON THE SOUTHERN STARS AND OTHER CELESTIAL OBJECTS.

By J. H. POPE, NEW ZEALAND.

This paper embodies the results of observations made during the last eight years. While most of the work is original, yet, when the object described is important, and an account of my observations could not be satisfactorily given without reference to the work done by previous observers, their facts and opinions have been quoted. An apology is scarcely needed for giving a short *résume* of the facts known about the great star *Alpha Centauri;* accordingly, a very brief history of this remarkable object, from Lacaille's time (1750) to the present has been given.

The instruments used were an 8½ inch reflector, by Browning, and a 4¼ inch equatorial of superior quality. The measures of angles and distances have been obtained by the methods described in my paper in last year's "Transactions."* The angles of position will, I have little doubt, be found to be good, but the atmosphere has not been steady enough of late to admit of the best use being made of oblique transits. I have, however, little doubt that such measures of distances as are given will be found to be very satisfactory approximations to the truth. For the spectroscopic work recorded in this paper I have used an admirable little star-spectroscope, by Browning. This instrument has enabled me to determine, quite satisfactorily, the class to which the stars examined belong, and, in many instances, to say that the spectrum lines of certain elements are probably present. As, however, the means at my disposal did not permit me to make accurate *measures* of the positions of lines, my work in this department should be looked upon as the results, so to speak, of a "flying survey," useful perhaps, in its way, but to be superseded when more thorough and accurate determinations can be obtained.

It should be stated, however, that, while depending on eye estimation alone, it would be very unsafe for an observer to say, that a conspicuous line, for instance, in the greenish blue of the spectrum of a certain star was certainly the F hydrogen line; yet it is unlikely that a practiced eye, one trained to recognize the position of certain lines in spectra that have been already measured, could be mistaken, in any large proportion of cases, in picking out, say, the principal Fraunhofer lines in a stellar spectrum. On the whole, it seems to me that such determinations as are given in this paper are not without a real value, if carefully made. Many years must elapse before the lines in the spectra of the southern stars can be accurately measured by methods like those employed by Dr. Huggins. In the meantime such results as those here given are all that are available. These serve to give us a certain amount of information that can be thoroughly relied on; they enable us to state, further, that the existence of certain physical conditions, and the presence of certain elementary substances in certain stars, are highly probable; and, possibly, they are calculated to create or stimulate in us a desire to learn more certainly and fully the constitution and physical habitudes of the stars.

The objects are treated of in the order of their Right Ascension, and the places of the stars when given, are taken from the "First Melbourne Catalogue," epoch, 1870.

The first star on the list is *Achernar* or *α Eridani.* This fine first magnitude star is very nearly pure white, without any discernable tint, except possibly a slight shade of blue. This star belongs to Padre Secchi's first class of stars, the type of which is the giant sun *Sirius.* In the case of typical stars of this class, the spectrum is

* Trans. N. Z. Inst., Vol. XI., Art. X.

remarkable for the great breadth and distinctness of the hydrogen lines. Indeed these stars are for convenience often called "the hydrogen stars." All of them are white or bluish-white. In *Achernar* the hydrogen lines are not nearly so strongly marked as they are in some others of the class. Indeed the star by no means nearly approaches the type, and is probably to be considered as holding a position between such stars as *Sirius* and stars of the second class, like *Procyon*, though much nearer to the former than to the latter.

π *Eridani.*– This beautiful little double-star is just visible with the naked eye in fine weather. It is about one degree from *Achernar*, north following. The two components are of the same orange color, and of very nearly equal magnitudes 7 and 7. When Sir John Herschel measured this star (anno 1835.0), he found the angle of position with the meridian to be 122.3°. Powell, in 1863, found the angle to be 73.9°. Last week (say anno 1879.75) the angle was 58.8°. The distances for the same dates are 3.65″, 4.88″, and 5.3″. This interesting double is, therefore, very probably a binary star of comparatively short period.

θ *Eridani.*—R. A. 2hrs. 53min. 19.9 secs. Decl. 40° 49′—35′ 17″ S. In the Melbourne Catalogue, the magnitudes 5.2 and 6.2 are assigned to the components of this fine double-star. There is most certainly serious error here. The star is plainly, taken as a whole, a large fourth, or a small third-magnitude star. Probably magnitude 3.9 for the larger star, and 5.9 for the smaller one, would not be far from the truth. The color of the larger star is yellowish-white, with a faint green tinge; the smaller is a light shade of indigo blue. Sir John Herschel's angle of position and distance, in the year 1835.75, were 81.5° and 8.68″. The angle at the epoch 1879.75 is 85.4°. I have not been able to get a thoroughly satisfactory distance, but it is now somewhat over 10″. Time and accumulated observations will, of course, show either that the change in the angle and the distance of the two stars of this double is owing to the proper motion of one, or both, of the components, or that θ *Eridani* is a binary system. The latter alternative appears to me to be by far the more probable.

232 *Reticuli* of the Melbourne Catalogue is a fine star of a magnificent scarlet color. It is of magnitude 6½. There is a distant companion white star of the eleventh magnitude. The R. A. of 232 *Reticuli*, is 4hrs. 35min. 15.15 sec., and the declination 62° 20′ 0.63″ S. The spectrum of this star is very remarkable. It belongs to Secchi's fourth class. The typical star of this division is small—invisible, in fact, to the naked eye; it is variable both in light and color; it is a very distinct red, ruby, crimson, or scarlet; and its spectrum consists of bands of light, sometimes containing faint bright lines with dark spaces between the bright bands. 232 *Reticuli*, though so small, gives a fine spectrum when the spectroscope is used with the reflector, because the light is not spread out over the whole length of the spectrum, but is concentrated in certain parts of it. Thus the red part of the spectrum is very bright, but the place of the orange is occupied by a very thick black "bar." The yellow, again, is pretty bright, and so is part of the green, but towards the violet end of the spectrum the light is very faint, and the colors are quite cut out for large spaces by intervals of almost complete darkness. I failed to notice here what is said to be characteristic of this class of stars—a gradual diminishing blackness of the bars in the direction of the violet end of the spectrum; nor could I distinguish any bright lines in any part of the spectrum. The study and observation of stars of this class is none the less interesting to us, because in the present state of our knowledge their spectra are unintelligible, for it is generally felt by those who have been in the habit of observing them, that there is a great secret of nature waiting to be discovered in connection with them. Their being for the most part so very variable both in light and color, the strongly-pronounced red color of all of them, and their strange and beautiful spectra, all point to the conclusion, that the man who succeeds in "reading the riddle" of the nature and constitution of the red variable stars, will have made a very important contribution to our knowledge of the process by which suns and systems are evolved out of the primordial nebula, or whatever the substance may be, from which such systems are formed, and to which, perchance, when their mission is fulfilled, they again return. In the meantime these red stars seem to set anything, even like rational conjecture, at defiance.

α *Argus* (*Canopus.*)—This great star, the only rival of *Sirius*, is a hydrogen, or first-class star. In its spectrum, the F and C hydrogen lines, and that near G, are broad and distinct, though less so than in the spectrum of *Sirius*. There are a great many very fine lines in the spectrum of *Canopus*, but these are not generally visible. It is only when the atmosphere is very steady and clear that they can be plainly seen. A fine line, however, or rather a small group of lines, in all probability that called *b*, and due to the presence of magnesium in the photosphere of the star, can generally be made out in moderately fine weather.

π *Argus.*—This is a wide telescopic double-star, forming, with a very distant companion of about the fifth magnitude, another double, easily visible as such with the naked eye. The color of the large third-magnitude star is a strongly-marked orange; the other two are indigo-blue. It is a well-known fact, that a large yellow or orange-colored star has frequently a distant companion of a blue or green color. It is generally supposed that this is a sort of *primâ facie* evidence that the two stars are in some way physically connected. It seems to me that the existence of these complementary colors in apparently neighboring stars in no way indicates *per se* that they are physically connected. I am inclined to think that, given a large bright orange star, with a smaller star naturally very white and nearly in the line of sight, this latter must appear greenish or bluish. The light of the bright orange star fatigues the eye as far as its power of receiving the impressions which we call red, orange, and yellow is concerned. Now, when the eye is directed to the smaller star, the less refrangible portion of the light coming from this fainter object is unable to act with its normal effect, while the green, the blue, and the violet rays, by which the eye has not been fatigued at all, produce their ordinary impression.

It is commonly said that this explanation may be true enough in a few cases; but that, if the bright star is hidden behind a thick bar placed across the field of the telescope, and the smaller star still appears blue or green, it is a proof that the light of the smaller star is really blue or green, and that its color cannot be the effect of mere contrast. This is, I feel sure, fallacious. I have often tried the experiment and at first it was very disappointing, for one would naturally expect that a star, which appeared colored in the presence of a very bright companion, would show its color still more distinctly when that companion was hidden from view. But this never happened, the more completely the light of the larger star was removed, the less was color in the companion observable. I feel persuaded that, if the light of the larger star could be entirely cut off, which, by-the-by, is impossible, the blue color would entirely disappear. It is worth noting, too, that the longer one looks at a blue star, its companion being hidden, the more completely does the blue color disappear; that is, I take it, as the eye recovers its normal condition, after being exposed to a severe strain from the light of the large star, so does its sensitiveness to the feeble red, orange, and yellow rays of the small star return, and it sees the small star to be white or nearly so. On the other hand, I have often noticed that the longer one looks at a double star of this kind, both stars being in the field, the more pronounced does the blue become. There is only one instance, that I am aware of,

in which this theory will not hold good. The small companion of *a Scorpii* is undoubtedly really greenish. I saw it on the 23rd of March, 1878, emerge from behind the moon after an occultation while its bright companion was still hidden, its color then was a pale pea-green. There could have been no contrast here, except with the moon's light; admitting this exception, however, it seems to me highly probable that while, in such wide double stars as *π Argus* and *γ Crucis* the orange or yellow star is really what it seems, the star that appears green or blue is, *as a rule*, really white. If this view is the correct one, it follows that those observers who spend a great deal of time in observing the tints of the companions to large stars, are, to a great extent, wasting their time.

γ Argus.—This fine second-magnitude multiple-star has a very curious spectrum. It belongs to a very small class of stars, the only other one that I have heard of is in the Northern constellation *Cassiopeia*. In the spectrum of *γ Argus* there are certainly three very *bright* lines, one rather faint, and, I believe, many finer ones. I am almost certain, too, that there are several fine dark lines in the spectrum. The brightest line is, not improbably, the F hydrogen line; and the somewhat fainter one, the C hydrogen line. Of the other two very distinct bright lines, one is certainly not very far from the position of the D sodium line; but I cannot place the other. The presence of bright lines in the spectrum renders it far more difficult than usual to estimate the positions, but the other line seems to be about one-third of the distance from D towards the iron line E. Not improbably then, outside the photosphere of *γ Argus*, there are ever-present enormous masses of hydrogen and sodium, as well as other substances in the gaseous condition, which have been ejected from the more central parts of this sun; and, the temperature of these incandescent gases being much higher than that of the solar photosphere below, their spectrum is super-imposed on the ordinary spectrum of the star proper.

ε Argus.—This yellow star belongs to Secchi's second class. In these stars the lines are very fine, and not easily seen unless the weather is very favorable. To this class our sun belongs. In the spectrum of *ε Argus* the F line can be seen pretty easily, but the D sodium line seems to be the most distinct of this spectrum.

β Argus.—Magnitude, one and a-half. Color, white. A first-class star. The hydrogen lines are pretty broad and distinct.

The blue Planetary Nebula near the Southern Cross.—This object, No. 3365 in Sir John Herschel's Catalogue, is in R. A. 11hrs. 44m., and decl. 56° 31′ S. The color of this strange object is a bright unmistakable blue. This nebula, like other planetary nebulæ that have been examined in the Northern Hemisphere, gives a spectrum of one bright line. Possibly, in a larger instrument, more lines might be seen. It is, of course, impossible with my apparatus to determine the position of this line, as there are no landmarks, so to speak, to guide one to a decision. It is most probable, however, that it is one of the hydrogen or of the nitrogen lines, and that this planetary nebula is a spherical mass of one or both these gases in an incandescent state.

a Crucis.—This superb *pair* of stars, by far the finest in the sky, consists of two stars, bluish white in tint, and very nearly equal in size, each being of the second magnitude. There is a distant six-magnitude companion, of a sea-green color, as well as three smaller comites of magnitudes, 12½, 14, and 13 respectively, These latter are well seen in the 8½-inch reflector, but a small telescope of course does not show them. I have made a very great number of measures of the angle of position of this star, and having weighted the observations with reference to the state of the atmosphere, etc., at the time when the measures were taken, I find the angle of position for the year 1878.7 to be 118.5°. This by a very singular coincidence, is exactly the same angle as that obtained by Powell in the year 1863. Herschel gives the angle for 1835.33 as 120.6°. I may say that, if I had rejected two of my observations, which were made in rather bad weather, and which exceeded the average of the rest by 1½° and 3½° respectively, the angle obtained, taken in connection with Powell's and Sir John Herschel's, would have indicated, I believe, a very slow but really regular angular motion, in a retrograde direction, since Herschel's time, and would, with the measures of distance given below, have convinced me, at all events, that *a Crucis* is a binary star of very long period. The temptation in such cases to "cook one's accounts" a little to omit taking into account facts or numbers which do not square with one's own views or wishes, is very strong, but the man who cannot resist it had better give up science altogether and take to something else in which it is not of vital importance that he should tell the truth, the whole truth, and nothing but the truth, with regard to his observations. The distance between the two stars at the epoch, 1836.36, was 5.65″; in 1863, Powell made it 4.98″; and at the end of last month, 1879.75, the distance, a mean of several measures, was 4.79″. *a Crucis* is a hydrogen star, but its spectrum is very difficult to observe, except in the finest weather. Even then the only lines that I can make out are the hydrogen lines, and they are by no means very easy to see.

γ Crucis.—It has been customary for astronomers to catalogue this star—the "Head of the Cross"—as a double star; but the proper motion of the large orange-colored star is rapidly carrying it away from its five-and-half-magnitude blue companion. The spectrum of *γ Crucis* is perhaps the finest of all stellar spectra. The groups of lines are so numerous and so well marked that this spectrum may be observed under almost any atmospheric conditions, if the star can be seen at all. *γ Crucis* is a typical star of Secchi's third class, which are all orange color verging towards red. In their spectra there are numerous, easily-seen, close groups of lines; but the hydrogen lines are either very indistinct or altogether absent. *a Orionis* and *a Herculis* are good specimens of the two principal subdivisions of this class. In the spectrum of *γ Crucis* there are at least eight broad groups of lines, and some of these occupy the parts of the spectrum at which sodium, iron, magnesium, and calcium lines are found in the solar spectrum. But, because they are groups, it is much more difficult to say whether they contain the lines belonging to those elements or not, than it is in the case of a first or second-class spectrum. Still, I anticipate that careful measurements will confirm my opinion that iron and magnesium lines, especially the latter, are present in the spectrum of this star; the sodium line is probably there too, There is, also, a fine line just at the part where the green merges into the blue of the spectrum. This is possibly the F hydrogen line.

There is one very significant feature in this spectrum, so at least it seems to me. It is well known that when the Sun is near the horizon, especially in damp weather, his spectrum contains certain groups of lines which are due to the aqueous vapor in our own atmosphere, and that, as he reaches a greater altitude, these lines become faint or disappear. Now, two at least of the groups in the spectrum of *γ Crucis* appear to occupy the same position as two of the principal groups of atmospheric lines. Now this being verified, important conclusions might follow. Secchi, on grounds of this sort, infers the existence of aqueous vapors in the neighborhood of sun-spots. The Spectroscope knows nothing, so to speak, about distance, except indeed where motion of approach or recession is concerned. If these aqueous vapor-lines are produced in spots on the sun, may they not be produced in much the same way in *γ Crucis*, the principal difference being that on the distant star the cause is more general and the effect greater than it is on our own Sun. If I am not mistaken, the existence of these spectrum lines should enable us to read a certain portion of the "life history" of a star.

This history might be something like this: Let us suppose that, countless ages ago, *γ Crucis* was a white star, like *Sirius*. It was then far more intensely heated than it is now. All the elements of which it is composed were there uncombined. Hydrogen, the gas of the smallest density, ordinarily extended furthest from the centre of the globe, and this hydrogen, its outer envelope, was nearly always near the confines of the normally cold regions of space. Thus it would have a somewhat lower temperature than the rest of the sphere, and hence well-marked hydrogen lines would appear in its spectrum at this period. Comparatively small quantities of other elements, however, would frequently be erupted from the interior portions of the sphere, and would reach what may be called the surface. The presence of these would cause the appearance of numerous fine lines in the spectrum. As eternity went on, if I may use the expression, the star radiated a large portion of its heat into space, the elements began to combine chemically to a certain extent, large volumes of hydrogen ceased to exist as such, through combining with oxygen and forming water-vapor, of which the outer star envelope would now consist. In place of the hydrogen lines of the white star therefore, we now find the aqueous vapor spectrum—"the atmospheric lines" as they are called. The result of the combination of the oxygen and the hydrogen would, of course, be a great decrease in the volume of the outer envelope. This would probably bring the lines of sodium, magnesium, iron, and calcium into greater prominence, and we should have the spectrum which *γ Crucis* now presents. Between the two conditions described there would be an intermediate one. Through such a stage our Sun may possibly be passing now. It may be, in short, that our Sun was once a *Sirius*, is now a *Procyon*, and will by-and-by be a *γ Crucis*. This is a mere hypothesis, of course, though it appears to account pretty fairly for some of the phenomena of the stars. In fact, I give it merely as a suggestion, feeling that it is as little entitled to carry weight with it as an hypothesis, founded on observed phenomena and not at variance with known facts, can be.

γ Centauri.—R. A. 12hrs. 34min. 21.46secs. Decl. 58° 14′ 43.24″.—This is a very fine close pair of stars, each component being of the fourth magnitude, and purely white. In his "Results of Observations at the Cape of Good Hope," Sir John Herschel gives the position-angle as 354.3°, the epoch being 1835.89, while the distance is stated to be ¾″. To this estimate of distance Herschel attaches no value. For the year 1878.93 the angle of position is 6.6°, or 186.6°, and the distance 2.2″.

β Crucis.—This fine white star has a distinct deep blood-red companion, the position angle being 260½° and the distance (1879)–208″. It seems to me that the small star varies in size from about the eleventh to nearly the eighth magnitude. It would be well if the small star could be watched, so that its period and the amount of its variation in brightness might be accurately ascertained.

α Centauri.—R. A. 14hrs. 30min. 47.07secs. Decl. 60° 17′ 53.93″. Magnitudes, 1-2. The following table will give the position-angles, and the distances of the components of this star, for selected epochs during the forty-five years which have elapsed since 1834, when it was first accurately measured by the greatest of all astronomers, Sir J. Herschel:

Observer.	Date.	Position.	Distance.
Sir J. Herschel	1834.7	—	17.43″
Sir J. Herschel	1834.8	218° 30′	—
Sir J. Herschel	1835.7	219° 30′	—
Sir J. Herschel	1837.3	220° 42′	—
Sir J. Herschel	1837.4	—	16.12″
Pcwell (from Webb)	1864	5° 7′	7.85″
Computed from mean places in F. M. G. C.	1870.0	17° 19′	10.73″
My recent measures	1878.7	156° 19′	—
My recent measures	1879.75	183° 8′	4.55″

With this table as a basis, it will be found that the major axis of the apparent orbit lies nearly in the direction 26½° to 206½°, and that the greatest elongation north is about 11″, while the greatest elongation south is 27″. Mr. Powell makes the period between 76 and 77 years. If the places of the two stars given by Lacaille (1750) were correct, however, the period would be just about 85 years, for the angle of position computed from his places of the stars is 218° 44′, which a reference to the above table will show, was very nearly Sir John Herschel's micrometrically-determined position 84.79 years afterward. As, however, the *distance* obtained by Sir John Herschel disagrees very materially with that deduced from Lacaille's places of the stars, but little weight is attached to the observation of 1750.

This magnificent double star is the finest object of the kind in the heavens. Besides being a binary star of very short period, every one knows that *α Centauri* is our next neighbor among the stars, and that it was the first to give up the secret of its parallax under direct Transit Circle observations. The color of this star is straw-yellow, or sometimes golden-yellow, according to the state of the atmosphere. When there is haze, of course the smaller star is somewhat more affected by it than the larger. This tends to give it a slight brownish tint when the sky is not clear. *α Centauri* is a star of the second class. Its spectrum is very like that of the sun. Even the principal dark lines are fine, and they apparently occupy the same relative positions as do the well-known lettered lines in the solar spectrum.

The resemblance between the two spectra is so striking that any one seeing the two spectra for the first time could hardly fail to notice the similarity. More dispersive power, however, and the means of accurately determining the position of the lines of *α Centauri* might show that they are not the same as the solar lines. Such a result would surprise me much. The D sodium line, the E iron line, the *b* magnesium line, and the F hydrogen line of the Sun have, almost certainly, their counterparts in the spectrum of *α Centauri*. There can be little doubt that the physical constitution of this great star is, in most respects, the same as that of the Sun. It is probable, however, that *α Centauri* is less developed than the Sun; for, as Mr. Proctor has pointed out, its light is brighter than its mass would lead us to expect it to be, judging from the light of our Sun, as compared with his mass. While the mass of the star is to the mass of the Sun as 2:1, the light of the star is to the light of the Sun as 3:1. Now, if it is true, as physicists have good grounds for believing, that the Sun is, and has been, very slowly but surely losing his heat, just as our earth has most certainly lost an enormous amount of hers, there must have been a time when the Sun and his system were less developed, but far hotter and brighter than they are now—when they formed, probably, as I said when speaking of *γ Crucis*, a white star—that is to say, there was, quite possibly, a time when the light from our Sun bore the same relation to his mass as the light from *α Centauri* bears to its mass. We may also believe that matters are less advanced in the planets (if there are any) of this neighboring system than they are with us.

α Trianguli.—The spectrum of this star is not very striking, but it is rather curious, as showing, apparently that the star is in a condition intermediate between that of *α Centauri* and that of *γ Crucis*. The lines of the second class, and also the groups, are very faint, but they are there. It will be seen that this fact has some bearing on the suggestion I made respecting the gradual development of stars while speaking of *γ Crucis*. Here it looks as if we had, so to speak, caught a star in the act of changing from the second to the third class. What I have seen of the spectra of the stars, so far, leads me to think it probable that if every star, down to the sixth magnitude, could be examined even with my instrument, and mapped roughly, it would be found that the spectra

obtained could be so classified that a series might be made, each member of which would differ from the next almost insensibly. This, of course, would take a long time to do, as small stars can be examined only in very fine weather. When it was done, however, the results would be very valuable and interesting.

α Gruis.—This is a second-magnitude white star, with the usual spectrum crossed by distinct hydrogen lines.

β Gruis is a second-magnitude star, and nearly as bright as the *lucida* of this constellation. Its color is reddish-orange, and its spectrum is much like that of *γ Crucis*, but the groups of lines are not so distinct, and, generally, there is a sort of approach to the appearance presented by the spectrum of *Mira Ceti*, which I find thus described in my note-book, under the date October 8th, 1878: "Saw to-night the spectrum of *Mira:* it is really wonderful—something like that of *α Herculis*, as given by Chambers. It seems to consist of bright broad bands, with narrow ones in between. These bands are dark, but hardly black. The effect produced is, as it were, that of an irregular set of columus. The brightest part of the spectrum is at the yellow and the green."

α Piscis Australis (*Fomalhaut*).—This star is visible at home sometimes, but its altitude there is so small that it can scarcely be properly observed with the spectroscope. *Fomalhaut* is a first-class star of the most pronounced type; it is very remarkable for the great breadth of the F hydrogen line. In *Fomalhaut* it is far broader than it is even in *Sirius*. As an increase in the breadth of the hydrogen lines has been shown to be due to increased pressure, and as the increase in breadth is also proportional to the pressure brought to bear upon the gas which gives the lines in the spectrum, we may, I would venture to suggest, conclude that the pressure at the surface of this star is extremely great. That is to say, *Fomalhaut* is either extremely dense and compact, so that its radius is very small compared with its mass (which is not very likely), or it is one of the very largest stars in the sky.

In conclusion, I would ask you to overlook any faults of style that may be observable in this paper. It claims to be nothing more than its title announces it to be—"Notes on Southern Stars."

SCIENTIFIC BOOKS.

Mr. W. H. Farrington recently gave an interesting Lectures on scientific books before the American Institute, a full report of which may be found in *Engineering News* of March the 19th. He said that in spite of the large number of scientific works published, there still are constant enquiries for books on certain subjects, which have yet to be written. This he explained was due to various causes, one being that the demand for certain books do not warrant their publication, and secondly, that many works treating on manufactures are withheld, it being the policy of those who could write them, to keep from the public such information. He stated that the English publishers seldom stereotype their better class of books, but print from the type, whereas in America it is the custom to print from such plates, permitting a much smaller edition to be issued. Those interested in the literature of Mechanics, and "*Engineering Science*," should read Mr. Farrington's Lecture from which they may gather many practical hints respecting the purchase of such books.

ASTRONOMY.

THE March number of the *American Journal of Science* contains a paper by T. C. Mendenhall, of Tokio, Japan, 'On the Determination of the Coefficient of Expansion of a Deffraction Grating by Means of the Spectrum." The object of the research was to find the coefficient of expansion of the peculiar alloy of tin and copper, now generally used for ruling gratings. The value of the coefficient of expansion is independent of the wave-length of the line upon which the measurements are made and of the number of lines to the inch. The temperature of the grating was altered by placing the plate in one end of a small wooden box which could be filled with water and brought to any given temperature. The resultant value for the coefficient of expansion from the mean of twenty measurements is

$$E = .0000202$$

DR. MEYER, Assistant Astronomer at the Geneva Observatory, has employed the microphone in transmitting the beats of the standard clock of the Observatory to different parts of the building, and also to the Regulating Clock of the city Time Service. The microphone is fixed upon the outside of the clock-case and placed in circuit with a small battery and a telephone. The beats of the clock can then be readily heard throughout the room.

AT the request of the Treasury of the Royal Astronomical Society, a committee has been appointed to advise the Government upon the steps which it is desirable to take in order to secure observations of the Transit of *Venus* across the sun's disk, 1882, December 6. The committee—which consists of the Astronomer Royal, the President of the Royal Astronomical Society, the President of the Royal Society, Professor J. C. Adams, the Earl of Crawford and Balcarres, Dr. De La Rue, Dr. Huggins, Professor H. J. S. Smith, Professor Stokes and Mr. Stone—has already commenced its labors.

ABOUT a year ago Admiral Moucher asked for a credit of 4,000 francs per year in order to publish a monthly astronomical review. M. Jules Ferry refused the grant, but a similar review is now being published at Brussels under the name *Ciel et Terre*. It appears twice a month and is devoted to meteorology and astronomy.

THE second number of *Urania* contains quite an elaborate article by H. C. F. C. Schjellerup, entitled, "Recherches sur L'Astronomie des Anciens;" also a short communication upon Observations of the Spectrum of Comet 1880 f. (Pechüle) at Dun Echt, and a "Circular from the Smithsonian Institution."

A NEW VARIABLE STAR.—M. N. C. Dunér, of the Lund Observatory, reports upon the 24th of February, 1881, the discovery of a new, small variable. The star is given in the Bonn Durchmusterung (1855.0)

9.4 mag. R. A. $= 5^h, 17^m, 32^s.7$.
Dec. $= + 34^\circ 2'.1$.

THE asteroid, No. 217, discovered by Coggia at Marseilles on the 30th of August, 1880, has recently been named "*Eudora*."

WASHINGTON, MARCH, 1881. W. C. W.

We are informed by Professor Davidson that the following is the correct geographical position of the Davidson Astronomical Observatory, San Francisco, Cal.:

Latitude $= 37^\circ 47' - 22''.3$ North.
Longitude $= 122^\circ 24 - 39.0$ West of Greenwich.
In time $= 8^h 09^m 38^s.6$

This differs from the figures we recently gave at page 107, in the number of seconds in time.

BOOKS RECEIVED.

THE TELESCOPE. By THOMAS NOLAN, B. S. D. Van Nostrand, 23 Murray Street. New York, 1881. Price 50 cents.

This little handbook presents very briefly the principles involved in the construction of refracting and reflecting telescopes, illustrated with about thirty diagrams. For an amateur desirous of following Herschel's example of making his own telescope, this work will be found a practical guide. We notice the author gives the form of object glass suggestod by Messrs. Alvan Clark & Sons, the noted makers of astronomical object glasses. They say "many forms may be used, but from our experience, we have found that to *make the crown glass lens of equal curvature, and the flint glass lens nearly flat on the side next the eye*, is the most convenient, and *gives as good results as any other form.*

MICROSCOPES.

The *American Journal of Microscopy* for March is an excellent number full of interesting matter and two full pages of illustrations.

We notice Professor Phinn corrects a statement mentioned in Journal of R. M. S. that 80,000 to 100,000 diameter were within the power of his appliances. He now gives as the limit of amplification of a high angle objection, say, an object of one tenth, and a one eight eyepiece, 8,000 to 10,000 diameters. We notice in another part of the Journal that Professor J. Edwards Smith obtains 8,000 with the same-eye piece and a 1/6 object.

Dr. E. Cutler describes a flagellate infusorium called *Asthmatos Ciliaris*, which occurs in connection with one form of contageous cold coryza or influenza. These parasites may be easily detected in the early sneezing stage, the nose runs and the eyes water; they are located in the anterior nasal passages, on the mucus membrane of the conjunctiva of the eyes, and of the pharynx and larynx. Simply transfer a drop of the thin mucus to a slide, cover, then examine with a good 1-5th and 1 inch occular.

There appears to be confusion as to the classification of this parasite, and as the opportunity for studying it will probably be oftener than agreeable, we shall be glad to hear from any of our readers who throw light on the subject.

Dr. F. L. Bardeen considers that wax cells have been too hastily abandoned by their originator, Professor H. L. Smith, who fully described them in "SCIENCE." Dr. Bardeen says that if prepared as he suggests, they are the best cells for opaque mounting.

Dr. A. C. Stokes has an excellent paper on "Growing Slides," and treats the subject in a most exhaustive manner; as most of the contrivances can be made by the microscopist, this article will be of the greatest benefit to this class,

Dr. Smith Baker, in a paper on the "Microscopal uses of the Cat," offers a plea for the more universal use of this domestic animal in microscopical study.

In view of the advice offered by Professor Burt G. Wilder, in regard to the use of the cat by anatomists, and the increasing disposition of students to use the cat for such purposes, we fear that this genus will soon be at a premium.

MANURIAL EXPERIMENT WITH SUGAR BEETS. — Phosphoric acid, applied preferably in the spring, increased the yield of sugar most decidedly.—M. MARCKER.

OCCURRENCE OF VANILLA IN RAW SUGARS.—The authors have succeeded in isolating small quantities of vanilline from crude-sugar.—E. v. LIPPMAN and Prof. C. SCHEIBLER.

THE GLYCERINE BAROMETER.

Mr. James B. Gordon has published the following description of his glycerine barometer—which appears to have at least one advantage in being easily read off, as the usual tenth of an inch on the mercurial barometer is represented in the glycerine barometer by something more than an inch; thus the changes which take place are rendered obvious even to an unpracticed eye.

Our readers may have heard of Daniell's water barometer, which was destoyed in the fire at the Crystal Palace in 1866. Mr. Jordan constructed another, which has since continued in operation. In the course of his experiments on various fluids, he was led to try glycerine, which appears well adapted for the purpose. Its vapor has a very low tension at ordinary temperatures, and as its freezing-point is much below zero, it is, so far, excellently adapted for use in barometers. The mean coefficient of expansion by heat is, according to Professor Reinold, .000303 for a degree of Fahrenheit's scale, and a table has been computed on this basis for reducing the observations to 32° Fahr. Glycerine possessing the capability of absorbing moisture from the atmosphere, its surface in the cistern is covered by a layer of mineral oil, which has no effect whatever on the glycerine, and which does not evaporate at ordinary temperatures. At sea-level the pressure of the atmosphere supports a column of glycerine of a mean height of 27 ft., and accordingly the tube of the barometer is made some 29 ft. in length. It is formed of composition gas-pipe, ⅝ths of an inch in diameter, but the upper part, 4ft. or so in length, is of glass tube, having an internal diameter of 1in. The top end, instead of being sealed, is spread out into a cup-shape, having a small orifice plugged with a stopper of rubber. The cistern is of tinned copper 4in. deep and 10in. in diameter, and the air is allowed to press on the surface through a small hole leading into a chamber containing a filter of cotton wool. At the bottom of the cistern is a closed channel opening into the centre, and to this is attached a projecting vertical tube, to which the main tube is soldered. The object of this channel is apparently to provide a means of closing the tube by a screw-plug when refilling is necessary. The quantity of glycerine required for such an instrument is about a gallon, and this being warmed in a water-bath and tinted with rosaniline, sufficient is poured into the cistern to cover the orifice of the channel. The plug at the top end is then removed, and the tube completely filled by pouring the glycerine gently down one side. After allowing it to rest for some time, the air bubbles will be found collected at the top, when the tube is again filled up to the cup, and the stopper replaced. The screw-plug in the cistern being removed, the column will fall until balanced by the pressure of the atmosphere, and the vacuum is as perfect as it is possible to get it, the small quantity of glycerine remaining in the cup above the stopper hermetically sealing it. The glycerine barometer is therefore a simple and easily managed instrument; but it is not pretended that it can take the place of the standard mercurial instrument for precision. It is comparatively a new instrument, and its value as a piece of scientific apparatus has yet to be shown.

HYDROBROMIC ACID AS A REAGENT FOR COPPER.—A drop of the solution in question is placed in a watch-glass, a drop of hydrobromic acid is added and the mixture evaporated at a gentle heat. When it is reduced to the bulk of one drop a rose-red coloration appears, three or four times more intense than that produced by potassium ferrocyanide. In this manner 1-100th milligrm. copper may be detected.

DETECTION OF METHYLIC ALCOHOL IN VINIC ALCOHOL.—MM. Cazeneuve and Cotton propose as reagent a solution of potassium permanganate containing 1-10th per cent of the dry salt. The permanganate at ordinary temperatures is reduced slowly by vinic alcohol, but instantaneously by methylic alcohol. If to 10 c.c. of alcohol at 20° there is added 1 c.c. of the permanganate solution, twenty minutes are required before the liquid takes the yellow tint indicating complete reduction. If 10 c.c. of alcohol are used containing 1 c.c. of methylic alcohol the yellow tint is instantly obtained with potassium permanganate.

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JOHN MICHELS, Editor.

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UNIFORM TIME.

The question of the introduction of uniform standard time into daily use, for both popular and scientific purposes, having been examined by the American Metrological Society, the president, Prof. F. A. P. Barnard directs attention to the following considerations, and invites exchange of views upon this subject.

He says "local time," in the astronomical sense of this term, varies with every change of meridian; it can therefore not be conveniently retained by travellers, and transportation and telegraph companies, which adopt whatever meridian may be the most convenient Over seventy such standard meridians are now in use by railroad and other companies throughout the United States and Canada; the larger towns and cities frequently adopt their own special local times, and the smaller ones adopt the railroad times most convenient to them; there are thus now in ordinary use at least 100 local times or meridians, many of them differing but a few minutes from each other.

Professor Barnard believes that a more thorough uniformity of accurate time would be to the daily advantage of all members of the community and all business transactions, and would immensely facilitate the study of certain natural phenomena, such as tornadoes, auroras, earthquakes, meteors, &c., for the observation of which we must depend largely upon those who chance to be favorably located.

It is accordingly proposed that the community unite upon a division of this continent into a few sections, throughout each of which the times adopted by railroad, canal, steamboat and telegraph companies, the city or town clocks and the clock makers, shall all be kept as nearly as possible in agreement with one standard meridian.

The system that especially commends itself for adoption, is that which also has the best prospect of being ultimately adopted by all nations throughout the world. It requires that, for the United States, we should adopt a central meridian in the Mississippi Valley, exactly 90° or six hours west of Greenwich, and proceed to the east or west by steps of exactly one hour each, so that the sectional times would be about as in the following schedule.

We have already given attention to this subject, and in "SCIENCE," Vol. I. p. 13, will be found some excellent suggestions in regard to "Uniform time," by Professor Ormond Stone.

In this article we merely present the views of Professor Barnard, the President of the American Metrological Society, and reserve a fuller consideration of the same for a future occasion. We may state, however, that we are heartily in accord with the object Professor Barnard has in view, and are pleased to find the matter in such able hands.

PROPOSED SCHEDULE OF STANDARD TIMES.

Geographical Section.	Standard Meridian west of Greenwich.	Standard Time slower than Greenwich.	Standard Time Slower or Faster than True "Local Times."	Designation of Proposed Standard Time.
Newfoundland New Brunswick Nova Scotia	60°	H. M. S. 4. 0. 0.	29 minutes slower than St. Johns, N. F. 24 " faster than St. John, N. B. 14 " faster than Halifax, N. S.	Eastern Time.
Canada Maine to Florida. Ohio to Alabama. Lower Lakes.	75°	5. 0. 0.	15 " slower than Quebec 18 " faster than Toronto 16 " slower than Boston 3 " slower than New York 8 " faster than Washington 19 " faster than Charleston 45 " faster than Montgomery 14 " faster than Buffalo 30 " faster than Detroit 28 " faster than Cincinnati	Atlantic Time.
Mississippi Valley Missouri Valley Upper Lakes Texas	90°	6. 0. 0.	0 " faster than New Orleans 1 " faster than St. Louis 12 " faster than St. Paul 18 " faster than Kansas City 19 " faster than Galveston 10 " slower than Chicago	Valley Time.
Rocky Mt. Region	105°	7. 0. 0.	0 " faster than Denver 28 " faster than Salt Lake City	Mountain Time.
Pacific States British Columbia	120°	8. 0. 0.	12 " slower than San Diego 10 " faster than San Francisco 11 " faster than Olympia 12 " faster than Victoria	Pacific Time.

HOW TO OBTAIN THE BRAIN OF THE CAT.

Felis domestica.

BY PROFESSOR B. G. WILDER, M.D.

In the first number of "SCIENCE," under the title "A Bit of Summer Work," the writer suggested that teachers and students of the several sciences in which an acquaintance with the brain is required should try to gain some definite and personal knowledge of the organ by the preparation and dissection of the brain of the domestic cat.

The publication of the article was followed by numerous expressions of a desire to adopt the suggestion, but accompanied often by requests for reference to some work containing explicit directions as to the best methods of manipulation.

No such work is known to the writer. The "Dissector's Guides" and some general treatises on Human Anatomy give more or less complete instructions for the removal of the human brain: but the conditions are usually such that the most expert manipulator can hardly avoid some injury to the organ.[1]

For the removal of the brains of the lower mammals, no adequate directions have been published, so far as the writer is aware, although Chauveau enters (A, 716)[2] into some detail with regard to the horse's brain. Straus-Durckheim expressly states (B, I, 321) that the method for animals is the same as that for the human subject, and the "Practical Physiology" (Foster & Langley, A, 215) contains merely the caution that "the brain of the dog or sheep should be removed from the skull as carefully as possible, especial pains being taken to cut the internal carotid arteries and the cranial nerves close to the skull,"

As guides to practical work for beginners in anatomy, the works just mentioned may, in respect to the brain, be likened to some "Manuals for Young Housekeepers," whose accomplished authors seem to realize neither the inexperience of their readers nor the possibility of conditions very different from their own, and whose teachings, therefore, prove ill-suited to the comprehension and the circumstances of those whom they desire to assist.

Now it is probable that few readers of "SCIENCE" have had the benefit of a full medical education, and it is certain that the anthropotomical method for the extraction of the brain does not answer for the removal of the brains of most other mammals. The skulls are usually so irregular in outline that the use of the saw is difficult and apt to do injury to the brain; moreover, at least for the purposes of preliminary study, the integrity of the brain should be ensured even when it involves the complete destruction of the skull.

The writer is therefore led to hope that the number of those who desire to obtain and dissect the brain of the cat is large enough to warrant the publication in "SCIENCE" of the directions which are followed by the students in the Anatomical Laboratory of Cornell University. Any criticisms or suggestions will be thankfully received. The method here described in detail is to be preferred when the brain is wanted entire, and especially when the length of the nerve roots is an object. The more expeditious methods which may be adopted under other conditions will be described hereafter.

INSTRUMENTS AND MATERIALS.—Medium scalpel;[3] sharp scalpel; arthrotome; tracer; curved scissors; bone-scissors; forceps; nippers; a cat's skull; large tray for the cat; small tray, or a folded cloth, for the head; block; small towel, or piece of muslin, for aiding the grasp of the head; paper for scraps; basin and towel; dish of 7 p.c. brine, about 6 cm. deep, and 20 wide, containing some well-soaked cotton; bowl of normal saline solution (15 grains of salt to 2000 cc. of water) sufficient to cover the head after its separation from the body; bowl for catching the blood; wide-mouthed jar or covered dish, of 60-70 p.c. alcohol, with some well-soaked cotton at the bottom.

Some of these items need explanation. The *arthrotome* —sometimes called "disarticulator"—is a short and strong double-edged scalpel, with a steel handle like that of the common "cartilage knife." The same use can be made of any short strong scapel ground down so as to have two edges of only moderate sharpness. Such an instrument saves the keener and thinner edges of the ordinary scalpels. The *tracer* looks something like the ordinary dental excavator, but its end tapers to a blunt point, which is so curved as to form about the quarter of a circle, and moderately sharpened on the concavity. This is used for tracing and isolating nerves and vessels, and is not only safer than the scalpel, but less liable to injury. Its cost is only 25 cents. The *bone-scissors* are simply a strong pair of curved scissors, employed for comparatively rough work. The *nippers* here referred to are the "diagonal side-cutting pliers" of the dealers in hardware. Instead of being parallel with the handles, as with most "bone-nippers," or at a right-angle therewith, as with the ordinary "cutting-pliers," the blades of these form with the handles a very open angle, confering upon the user an advantage similar to that which is gained by the employment of curved scissors. The nippers are to be had of seven sizes, from 10 to 20 cm., (4 to 8 inches) in length, and cost from 70 cents to $2.25, according to the size and the maker, those of Stubbs being the more expensive and highly finished. For use upon cats, those which are 5 inches long are to be preferred, and their points, if too wide, may be ground off.

The writer has been accustomed to use the nippers since 1872 for the removal of the brains of cats, dogs and young human subjects. It was not until after the year mentioned that he noted, in Flower's paper (3, 194), a remark as to "clipping away the skull from the brain of a monkey," the instrument, however, not being specified.

The nippers are equally applicable to living animals; with the rabbit, cat, and all but the larger dogs, the skull may be penetrated with them, and the opening easily enlarged to any extent desired. Perhaps the surgical "bone-forceps" have been employed for this purpose, but the "Hand-book for the Physiological Laboratory" (Sanderson, A, 305 and 418), directs that even so thin a skull as the rabbit's should be removed with the trephine and the scissors, and Dalton's recent paper (2) mentions only the trephine for exposing the brain of dogs.

Alcohol of the proper strength is readily prepared by adding 1 part of water to 2 parts of 95 p. c. alcohol. According to the size of the bowl or jar, the amounts may be 100 and 200, or 150 and 300 cc.

For the hardening and temporary preservation of the brain, the common deep finger-bowl is convenient. It may be covered with a piece of window-glass. Flat-bottomed dishes, with wide edges ground for the reception of covers, are made by Messrs. Whitall, Tatum & Co., of New York and Philadelphia, and the same firm have on hand wide-mouthed vials and specimen-jars of many sizes.

KILLING AND BLEEDING THE CAT.—When the brain is to be studied the animal should not be "pithed," on account of the injury to the medulla, and the settling of blood at the base of the organ. The cat may be drowned, but the following method is to be preferred as less distressing, more convenient, and permitting the evacuation of most of the blood. The *bleeding* may how-

[1] The writer has employed a modification of the ordinary method, and will take an early opportunity of submitting it to other anatomists.

[2] The system of references adopted in the present paper is the same as that described by the writer in No. 38 of this journal, p. 122, excepting that the numbers of papers published since 1873 are in smaller type than those of the papers which appeared prior to that date, and which are included in the "Royal Society Catalogue."

[3] Cases of dissecting instruments containing the arthrotome, tracer, scalpels of three sizes, curved scissors, forceps, fine-pointed forceps and curved scissors, and blow-pipe, are sold by Messrs. Codman & Shurtleff, of Boston, for $9.00. The nippers and bone-scissors must be obtained separately, as will be explained presently.

ever, be dispensed with. Put the cat in a close box, little larger than itself, and pour in 5 cc. of chloroform, or 20 cc. of ether. It usually becomes quiet in from 5 to 15 minutes. When touching the conjunctiva causes no winking, remove the cat to the large tray. Bring the head and neck nearly into line with the trunk. Part the hair upon the neck along a line between the angle of the mouth and the convexity of the shoulder. Along this line, divide the skin for 6—8 cm., opposite the larynx. This will either expose the *Vena jugularis* at once, or permit it to be seen when the borders of the skin are raised.

With the tracer, separate the vein from the adjoining parts for about 1 cm., and pass a bit of string entad * of it. Then turn the cat on the side, with the exposed vein over a bowl; the string makes it easier to pass the scalpel or blade of the scissors entad of the vein, which may then be cut. Let the blood flow into the bowl, occasionally lifting the body so that the blood may come more readily from the abdomen.

When the flow ceases, replace the cat in the box, with an additional 5 cc. of chloroform, or leave it upon the tray, and apply the chloroform upon a towel held very closely at the nostrils so as to prevent access of air. Death usually ensues in a few moments. If it be desired to ascertain the weight of the entire animal, the blood should be weighed.

If the more delicate internal parts or the microscopic structure are to be studied, the remaining operations for the procurement of the brain should be performed within 24 hours. But if the specimen is desired only for the fissures or the coarser anatomy, removal may be deferred for a week, provided the head be kept in a cool place. It should not, however, be allowed to freeze.

Separation of the skull.—Connect the angle of the mouth with the incision already made. If the sk n is to be mounted, this should be the only incision, and the skin must be dissected from the mandible as well as from the rest of the head. But if, as is more often the case, the skin is not to be preserved, while the muscles etc., of the neck are to be examined, make a corresponding incision from the angle of the mouth upon the opposite side.

In all subsequent operations, unless otherwise stated, both sides are to be treated alike.

Dissect the skin from the maxilla as far as the ventral margin of the orbit and cut the nasal cartilages. Dissect the skin from the nasal and frontal regions, including the upper and lower lids, but leaving the third lid, *Membrana nictitans*, attached to the ball. Remove the skin from the rest of the head, dividing the *meatus auditorius* close to the head. The *parotid gland* will be removed with the ear, but the *submaxillary*, of a darker color, will remain with the head. Reflect the skin from the cervical muscles for about 2 cm. caudad of the *crista lambdoidalis*.

Dissect the origin of the *M. massetericus* from the *zygoma*, noting that its cephalic and caudal borders are strengthened by tendinous bands which must be cut. Push a nipper-blade between the eyeball and the cephalic root of the zygoma, and nip the latter as close as possible to the maxilla. Then nip the caudal root at the angle between the transverse and longitudinal parts of the zygoma, just laterad of the *Fossa glenoidalis;* remove the zygoma with the bone-scissors.

Grasp the lateral aspect of the eyeball with the forceps, and rotate it mesad so as to expose its attachments, by the muscles and *N. opticus*, to the bottom of the orbit; cut the attachments with scissors, leaving the *Mb. nictitans* connected with the ball. If the eyes are to be studied or preserved, mark them right and left by numbers or tags; the proper position is always indicated by the *Mb. nictitans*.

Slightly ventriduct the mandible and move it from side to side so as to indicate the position of the *Ath. temporomandibulare*. Often the capsule has been opened already in nipping the caudal root of the zygoma. If not, it is to be cut while on the stretch by inserting the arthrotome, and cutting until separation is complete on that side.

Dissect the *M. temporalis* from its cranial origin, and then from its insertion upon the *processus coronalis* of the mandible. Then bring the mandible to a right angle with the rest of the head; feel for the caudal border of the hard palate, and for the tips of the *processus pterygoidei;* at a point midway between them push a scissor-blade entad of the soft palate, and divide it; then divide the mucosa forming the dorsal wall of the *postnares*, and dissect it from the basis cranii to the atlas.

The mandibles are now attached to the rest of the head by some muscles, by the mucosa at the angles of the mouth which may now be divided, and by the slender piers of the *hyoid arch*. These last join the skull at the lateral side of the bullæ, where they are to be divided with the arthrotome; if it be desired to examine the mode of their attachment, they may be cut with the bone-scissors at a little distance from the attachment.

Turn the tip of the mandible still farther ventrad and caudad, and dissect off the muscular masses that are inserted between the bullæ; near the caudal ends of the mesal borders of the bullæ emerge several nerves, which should be divided with the scissors or a sharp scalpel at about 1 cm. from the skull. By continuing the removal of the muscles across the *Ath. atlo-occipitale* this is exposed. Put the membranes upon the stretch, and divide them with a sharp scalpel along the cephalic border of the atlas. This exposes the *myelon*, which is to be divided in the same way. The remaining ligaments and the cervical muscles may be cut with the arthrotome and the skull proper is then separated from the rest of the body. Place the skull in the *n. s. s.*, and wash the hands and the instruments which have been used.

EXPOSURE OF THE BRAIN.—The method here described is by successely removing bits of the skull with the nippers.

Caution.—In the later stages of the operation there is considerable risk of injuring the brain by the unintentional pressure of the nippers. In whatever way the bone is grasped, when force is applied, the tendency is to approximate the cutting edges as nearly as possible, and thus to bring their planes into right angles with the surface of the bone. This of course crowds the convexity of the ental blade against the brain, and may crush it seriously. It may occur either from the turning of the nippers in the hand, or more frequently from the escape of the skull from the grasp of the other hand. The accidents may usually be avoided by keeping the matter in mind, by having the right hand dry, and aiding the grasp of the more or less slippery skull by a small towel or bit of coarse muslin; this last is also desirable during some stages of the operation as a protection of the hand itself from abrasion.

In using the nippers another precaution is to be observed. If the bit of bone to be removed is attached only to bone it may be either cut or broken, or twisted off; but if it adheres to the *dura* or other soft parts, only *cutting* should be employed, and that done with care.

During the exposure of the brain the head should be frequently dipped into the *n. s. s.* If obliged to suspend the operation for more than an hour, wrap the head in a cloth wet with the *n.s. s.*, and set in a cool place.

Nip off the caudal root of the zygoma, including the *Fs. glenoidalis*. Insert a nipper-blade into the *meatus auditorius*, and remove the bulla in fragments. With the scissors cut away the membranes attached to the margin of the *Fm. magnum*. Nip off the occipital condyles, with the intervening area of the *basioccipital* for 2-3 mm.

* The meaning of this and some other unfamiliar terms may be learned from a paper (9) in "SCIENCE" for March 19 and 26. Most of the words employed are to be found in a Human Anatomy, or a dictionary, general or medical.

from the foramen. Insert a nipper-blade between the dura and the bone 5–6 mm. from the meson and in line with the mesal border of the cephalic part of the bulla, and nip out the basioccipital as far as the middle of the length of the bulla. At or near the angle left after the removal of the condyle and the basioccipital, the *N. hypoglossalis* enters the *Fm. condylare*, and passes cephalad to emerge on the ventral aspect of the skull by the *Fm. jugulare*. If the series of roots do not appear, carefully remove a little more bone until they do. If the nerve-roots are to be especially studied, endeavor to nip off the bone surrounding the *Fm. condylare*, so as to save the trunk. On emerging upon the ventral aspect of the skull, the *N. hypoglossalis* will be found to lie practically in the *Fm. jugulare*, and to be more or less intimately attached to the *NN. glossopharyngealis, vagus* and *accessorius*, which penetrate the bone by that foramen. In attempting to separate the *N. hyp.* great care must be used to avoid any traction upon the roots, which readily pull out of the medulla. Of the other three nerves, the *accessorius* is the most caudal, and the most readily distinguished, but at this stage it is as well to leave them together, simply endeavoring to remove the bone surrounding the foramen, and to save the trunks pretty long, at least upon one side. Upon the other, it will save time to cut the roots just entad of the skull, and the same may be done on one side with the remaining nerves, or with all upon both sides in case the brain is not to be employed for the study of the apparent nerve-origins.

The dorsal wall of the bulla is hard, but readily crumbles between the nippers. It may be removed in small pieces, so as to save the *NN. facialis* and *auditorius* which enter the *Fm. auditorium internum*, and the little *Lobulus appendicularis* of the cerebellum which is lodged in a slight fossa just dorsad of the foramen.

Since no nerves are transmitted by the mesal region of the basis cranii, it may be removed with comparative freedom, as far cephalad as the *pituitary fossa* where some care is needed to avoid injuring the *hypophysis*.

The skull may now be held more securely by the facial region, especially if a towel is employed. In removing the bone at each side of the meson, and just cephalad of the bullæ, great care is required to disengage the nerves which emerge by the *FF. ovale, rotundum*, and *sphenoidale*. These nerves, the *NN. oculomotorius, trochlearis*, and *abducens*, with the *ophthalmic, superior maxillary*, and *inferior maxillary* divisions of the *N. trigeminus*, penetrate the bone more or less obliquely, and are closely surrounded by dense connective tissue.

Just cephalad of this series of foramina is the *F. opticum*, and the *N. opticus* is particularly hard to disengage without tearing some of the delicate parts (*terma*, or *lamina terminalis*, etc.,) which are connected with the *chiasma*. Since the optic nerves are always easily recognized, it is usually better to cut them pretty short with the scissors, than to run the risk of rupturing the terma.

The entire maxilla is now to be removed by first nipping the interorbital region just cephalad of the fronto-maxillary suture, and then, with the bone-scissors, cutting toward this point from just caudad of the cephalic root of the zygoma. The scissors should be kept as far cephalad as possible, so that the *Bulbi olfactorii* may not be injured. This plan serves equally well for some dogs, but with the larger breeds, which have prominent *bulbi olf.* the interorbital region should be nipped at about the middle of the length of the nasal bones.

Remove the mesal walls of the orbit, and the turbinated bones, using care not to crush the very soft *Bulbi olf.* The *Nervi olf.* should be divided, a few at a time, with the scissors or the tip of the scalpel, and all pulling and twisting of the parts must be avoided.

During the remaining steps of the operation, the head must be held by the parietal regions, and with great care, so as to avoid pressure of the tips of the fingers upon the brain. The bone, also, must now be *cut* by the nippers rather than twisted or broken. Nip off the *supraoccipitale*, including the dura, as far as the *Crista lambdoidalis*. To remove the ventral part of the bony tentorium, introduce a nipper-blade between it and the hemisphere on either side, in such a way that the greater convexity is toward the hemisphere rather than the cerebellum; the cut is to be made at the level of the *Sutura squamosa;* the width of the tentorium at this point is about 8 mm., and the nipper-blade should not be introduced to a greater depth than that, for fear of injuring the *Lobi optici*. In closing the blades the head should be held very firmly so that no rotation may occur. The detached ventral piece of the tentorium may be extracted by the forceps, or by the nippers used as forceps, any adhesions being carefully separated with the tracer or scissors.

Hold the head with the ventral side down, support the caudal divisions of the brain with a disengaged finger, and with tracer and scissors separate the cephalic surface of the cerebellum from the tentorium. Then hold the head with its caudal end down, and complete the disengagement of the *Bulbi olf.* Hold the head over the 7 p. c. brine, with the ventral side down, and nip out, piecemeal, a triangular piece of the calvaria, nearly to the tentorium. The mesal adhesions of the dura may be divided with the scissors, but elsewhere the dura is to be left upon the hemispheres. As the hemispheres begin to fall, hold the head so that they are supported by the brine, and then snip all remaining adhesions until the entire brain is free and floats in the liquid.

REMOVAL OF THE DURA.—Saturate some cotton with the brine, and place it under the brain, so that about one-third of the organ projects above the surface. Avoid handling and lifting the brain; move it by shifting the cotton, or by grasping the dorsal portions of the dura. Remove the dorsal and lateral parts of the dura by grasping the free borders left by cutting along the dorsimeson, and cutting out piece by piece with the scissors. Then grasp the *falx* just dorso-caudad of the *Bulbi olf.*, at the straight transverse fissure—*F. cruciata;* introduce the scissors about 5 mm., and cut the *falx*. Gently draw the cephalic portion cephalo-ventrad between the *Bulbi olf.*, and remove it. Draw the caudal portion caudad, and carefully cut all its attachments.

Turn the brain upon its dorsal surface, and remove the ventral portions of the dura with great care and in small pieces. Especial pains are needed in connection with the hypophysis and the nerves, and all pulling must be avoided. On one side, at least, it is well to leave the dura still attached to the nerves and the great *Gasserian ganglion* upon the *N. trigeminus*, to be more completely removed at the time of the removal of the pia.

TRANSFER TO THE ALCOHOL.—Place a large spoon or watch-glass at the side of the brain, and pull the cotton which supports it, so as to roll it into the glass, resting upon its dorsum. Let the brain slide off into the alcohol so as to rest on the cotton therein, still with the ventral side up.

Set the bowl with the alcohol in a cool place, and change the position of the brain at intervals of five to ten hours during the first three days, by pulling the cotton in various directions. At the end of about three days, transfer the brain to 95 p. c. alcohol, where it may remain indefinitely. For a few days, however, it should rest upon cotton, and its position be occasionally changed.

WEIGHING THE BRAIN.—If this is to be done, handling the brain may be avoided as follows; Place the bowl of alcohol into which the brain is to be put, upon the scales, and pour in alcohol of the same strength until it balances an even number of grams, *e. g.*, 400, 410, or 420. While the brain is in the spoon or watch-glass, pour over it some of the same alcohol, and then let the latter drain off as much as possible, by tilting the glass,

and supporting the brain with the fingers or a bit of cotton. Then transfer to the bowl of alcohol as above directed, and the increase in weight will represent, with approximate accuracy, the weight of the brain.

REMOVAL OF THE PIA.—This is most easily accomplished at the time of the removal of the brain to the stronger alcohol. At any subsequent period the pia is apt to be more firmly adherent. If the brain has been allowed to dry at all during its removal from the skull, the pia comes off with great difficulty.

Instruments and materials.—Forceps; fine forceps; medium scissors; wetting-bottle of 15 p. c. glycerine; cotton thoroughly wet with water, and so moulded as to form a sort of shallow cup in which the brain may rest without danger of rolling off.

Place the brain upon the cotton, and wet it with the glycerine. Then let it rest upon its ventral side, and grasp it in the cotton, firmly yet gently. Grasp with the forceps the fold of *pia* which occupies any one of the fissures, especially at the point of forking or junction with another fissure, and pull along the line of the fissure. Usually the fold of pia will come out easily, and with it will be removed some of the pia covering the free surface of the gyri between it and the adjoining fissures. Proceed thus until the pia has been removed from the dorsal and lateral aspects of the hemispheres. Avoid pulling across the line of the fissures. The larger forceps are easier to work with, and less apt to puncture the brain; but the fine forceps are sometimes required for the removal of the pia from the bottom of a deep fissure. The caudal surface of the hemispheres may be reached by slightly ventriducting the cerebellum. The mesal pia can only be removed close to the margins of the hemispheres.

On one side, preferably that on which the *N. opticus* was cut shorter, raise the mass of nerves formed by the divisions of the *N. trigeminus* and *N. abducens*, by its lateral border, and cut with the scissors the *N. oculomotorius* which holds the mesial border close to the brain. This will permit the mass to be turned caudad so as to expose the course of the slender *N. trochlearis* which emerges from between the hemispheres and the cerebellum. It also permits the removal of the pia from the region just laterad of the hypophysis. Grasp the pia on the ventrimeson just caudad of the *Bulbi olf.*, and pull caudad so as to remove it as far as the chiasma, taking care not to tear the delicate *terma* just dorsad of the chiasma. Then remove the pia from the olfactory tracts.

In removing the pia from the medulla the position of the nerve roots should be constantly kept in mind, and the traction should be laterad and cephalad. One of the most difficult things is to preserve uninjured the series of roots of the *N. hypoglossalis*, for their connection with the pia seems to be closer than with the medulla. Sometimes it may be necessary to let the brain be wholly below the surface of water or alcohol so as to float the roots out, and render them more apparent.

As suggested on a previous page, it is often as well to leave the roots longer on one side than the other, but the choice may be determined mainly by the degree of success in the various operations which have been described.

If desired, later numbers of "SCIENCE" will contain directions for the general dissection of the brain. Meantime, it would be well for the student to make outline drawings of the brain he has prepared, especially of its base. Most of the principal features of this surface can be identified from the figure of the corresponding surface of the human brain to be found in any good Human Anatomy. The drawing should be enlarged two diameters, and the brain should be kept wet with the glycerine mixture, while it is out of the alcohol.

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ATOMS AND MONADS, THEIR METAPHYSICAL DEVELOPMENT.

By DR. DIODATO BORRELLI.

(Translated from the Italian by the Marchioness Clara Lanza.)

In previous chapters of this work [1] it has been shown that the whole product of our psychological activity typifies a purely metaphysical world. It has likewise been seen, that the vast compound of forms by means of which exterior nature is represented to us, is not an extrinsic reality, but merely our own impressions, the result of slow and unconscious practice. A minute physio-psychological analysis leads us to this necessary conclusion. Colors are mere modifications, induced in certain groups of ganglion and cephalic cells by a stimulus which acts upon the organs of sight. Sounds are another form of cellular modification determined by a different kind of stimulus. Weight and resistance are phenomena of muscular sense. Form and size, synthetic relations, and therefore purely subjective phenomena. All the complex qualities by means of which physics are able to recognize different bodies, are nothing more than our own determinations. From this we may properly conclude that body and matter are not extrinsic realities, but a complication of modifications produced within us by exterior impulses.

Our world is therefore purely phenomenal, and not a reality. Herbart reasonably maintains that the first moment of research must necessarily be one of doubt, or scepticism, which is degraded or elevated in proportion as the uncertainty concerns things as they seem to us, or whether it relates to existence itself. Does the reality exist? This is the first question which presents itself to the philosopher. And if it does exist, what constitutes it and the consecutive research? "We cannot deny the reality," says Herbart, "because, to do so, is to remove all possibillity of the phenomenal world before mentioned. Sensations, representations and thoughts would be completely annulled."

This phenomenal world, resulting from the data of experience, is that which induces us to admit the existence of positivism. But these data do not constitute real existence, because they are not self-subsisting, but depend upon something else. That is to say, they exist in something else and by means of something else. *Actual existence* does not admit of either relation or dependence, it is based upon itself, and is, therefore, an absolute con-

[1] Borrelli. *Vita E Natura. Studii sui temi più importanti del Moderno Naturalismo.* Naples, 1880.

dition to the full comprehension of which we cannot attain, although we cannot fail to recognize it. The positive is, therefore, something to which this absolute existence attaches itself—it is in fact, a quality.

According to Spencer,[2] positiveness is nothing more than *persistence in the consciousness;* "unconditional persistence, such as the mental perception of space, or conditional, such as the intuition of a body we hold in the hand. That which establishes the persistence is really what we call positiveness, of which, (although we have demonstrated that the positive within our own consciousnsss is not objective) we, nevertheless, form an indefinite idea as being something which persists absolutely, in spite of all change of mode, form or appearance."

Spencer's definition, however, is in some respects open to criticism. First of all, if by consciousness, individual consciousness is to be understood, can anything persist which is merely an illusion without any definite existence? This can only happen under certain pathological conditionns of the mind. But there is still another point. Persistence in the consciousness is certainly a relation, because no thought can be produced without a relation, and even Spencer affirms this when he says; "We think relatively, every thought is based upon a relation."

However, according to Herbart, one of the principal conditions of absolute reality, is to be free from everything pertaining to dependence or relation. To avoid confusion, we must give more than one signification to the word positiveness. To begin with, we cannot ignore a *relative positiveness* which comprises all the conscious conditions of our being, and the famous sentence "*cogito, ergo sum,*" is in itself a peremptory demonstration of it. Sensations, representations, sentiments and all other familiar modifications are embodied in such positiveness. Whether they correspond to an objective effect or not: whether they are illusions of a diseased mind, or normal representations, is of little consequence. We know that they exist in our consciousness, and that is sufficient, inasmuch as they typify a real function.

We cannot say as much for objective or absolute positiveness. In regard to this, the experience of our senses teaches us nothing. We only know that it does not correspond to our individual sensations and that it differs from them essentially. In all our relations with the exterior world there is nevertheless a common and constant condition—an inexplicable something which acts upon our organs of sense and determnes the inward modifications. If, however, we rob Nature of our complex determinations, we leave nothing remaining but *stimulus* or *action*, which works upon us incessantly. The idea of objective or cosmic positiveness, originating from the data of experience, presents to us a conception of force or energy, combined with continual action. Absolute positiveness we can only understand as something corresponding to *permanent acitvity.*

The most ancient philosophers of Greece and India made extensive speculations as to what this natural force or activity might be, which operates in such manifold ways upon our senses and creates in us the most stupendous and varied phenomena. Indeed, human reason in Ionian, Pythagorean and Eleatic schools, seems to have been directed solely upon Nature under its various aspects.

According to Thales, water is the first general principle from which all other things are derived. In Anaximander of Miletus, a more condensed cosmological conception appears concerning the universe as being constructed out of *primitive matter*, which he called fundamental principle—an eternal, infinite, indefinite basis, from which everything originates and to which everything in the course of time, returns. This principle is not, as Aristotle appears to think, a compound which upon separation resolves itself later into particular forms. It seems easier to believe that the specification could only occur under some peculiar influence.[3]

We may well be astonished when we consider that six centuries ago a conception concerning the universe arose, which intimately resembles our modern cosmology. And our wonder is increased ten-fold when we see Anaximander produce from his fundamental principle the original antithesis of heat and cold by means of an inherent and eternal movement of the substance.[4]

According to Anaximanes, *Air* is the first general principle from which everything is produced by means of the double process of condensation and rarefaction. This theory should not appear strange to our modern mechanical school, according to which, particular forms of ethereal atoms are diffused throughout sidereal space, from which chemical atoms, and, consequently, all ulterior bodies are produced.

While the Ionian school deals principally with the sensible qualities of bodies, it aims more directly towards their inward substance. But we see in that of Pythagoras a new tendency, an increased abstraction. Paying but little attention to Nature, which is unknown to existence, he turns to consider order and quality, which are, indeed, realities. Numbers are the principle of all things; the Universe is only measure and harmony. Our quantitative relations, dimension, extension, form, distance, etc., are impossible without the aid of numbers, and therefore numbers are the first principle in all things, as they determine the order in which everything presents itself. Without stopping to discuss with Zeller as to whether the Pythagorean numbers are the substance or model of sensible things, we must particularly note that the idea of order and numbers is chiefly important in our modern conception of the Universe. If Nature really consists of but a single substance of various formations from whose elementary parts the specification of individual bodies is produced, it is natural to suppose that the true essence of all things by which they are determined, cannot be the indefinite cosmic principle, but a special disposition, which assumes its elements and the number in which they unite. *Numbers and disposition* form, as we shall see, the basis upon which modern chemistry rests.

With the Eleatic school which arose from a conception of *unity* and *immovability*, exaggerated to such an extent as to lead Zeno to a paradoxical denial of all motion, we come to Heraclitus, who, in direct opposition to the Eleatic school, speaks of perpetual flux and movement. The permanence of existence is merely an illusion. Positiveness may be compared to a river which disappears as it rises, and into whose waters, consequently, we can plunge but once. Heraclitus affirms that nothing remains equal to itself, that everything increases, diminishes, and finally dissolving, passes into other forms. Hence, from life to death, and from death to life again. The appearance and disappearance of these forms is, therefore, the perpetual vicissitude of the universe. In this stupendous doctrine we have the conception of future existence, which is nothing more than the harmonious blending of adverse tendencies. And in it we think to perceive the germs of the future theory of evolution. But it contains something else also. According to Heraclitus *fire* symbolizes the law of vicissitude. This is a profound doctrine which demonstrates to modern theories that no new formation or division of elementary bodies is possible without a corresponding modification in the inward action from which thermal phenomena are derived.

In the four roots of all things—fire, air, water and earth—there is first of all connection with the *sphere* and later a division. Upon this blending and separation depends the source and dissolution of all particular forms.

[2] Herbert Spencer. *First Principles.* 1871.

[3] Zeller, quoted by Fiorentino. *Manuale di Storia della Filosofia,* Naples, 1879.

[4] Schwegler. *Geschichte der philosophie.*

In Empedocles we find for the first time a confused perception of *attraction* and *resistance* in the sympathy and conflict which are the determining causes of the union and disunion of the elements.

Up to this time we have an irreconcilable antithesis between the Eleatic conception and that of Heraclitus. On one side, by exceeding the data of experience and elevating to the highest degree abstracts of material things, we find existence robbed of all determination and unchangeable. On the other, we have existence and non-existence bound together by means of the Future, from which springs the change and perpetual vicissitude of all things. But there is no fixed law for the Future of Heraclitus ; it is merely the result of experience, nothing more. Why, therefore, does existence change? Why are forms produced only to be again dissolved into something else? An attempted explanation was given, as we have seen by Empedocles; sympathy and resistance attract and repulse the four radicals of all things, and all forms are produced by the attraction which the repulsion afterward disunites and destroys. This is a profound conception, but yet somewhat obscure and undecided.

It was the *Atomical* school which took gigantic strides along this path, finally reaching those massive theories which even to-day we must look upon with admiration. Its founders were Lucippus and Democritus,but the latter is undoubtedly the most celebrated. We will go over the most important points in his doctrine, as they are related by Fiorentino.

"Existence is not a unity, but a combination and an infinite one, composed of many minute and invisible bodies which move about in space, unite and produce life, then separate, and cause death. They are capable of union and disunion, but never of change, and just as they are in the beginning, so they will always remain.

"We can distinguish in atoms form, order and position which are the primitive qualities which serve to produce others. All atoms are not equal; all have a downward tendency, but the lighter rise above the heavier producing a rotatory motion which extends and forms bodies."

Atoms, moreover, are impenetrable, and as units cannot be divided. They are consequently distinct one from the other, well defined and unconfused. This necessitates the interposition of something which tends to keep them separate. It can be nothing more than the *opposite* of the mass, vacuum, which causes interceding intervals between the atoms and holds them apart. Without vacuum, no motion could be possible, as the mass can receive nothing more in itself, or be augmented in any way, because this can only be obtained by the introduction of new atoms in the vacuum. We have, therefore, two contending agencies—existence (atoms) and non-existence (space), which go to represent objective positiveness. The final and most important of atomic theories bears the stamp of unconscious and unintelligible *natural necessity*. Motion can be determined by no cause. It is as eternal as the atom itself and is a part of its nature. It is easy to understand therefore, however far it may depart from the truth, the opinion of those followers of Democritus who attribute the origin of the world to chance.

By the atomical theory we have reconciled therefore, the unchangeableness of existence with the perpetual transformation of things; transformations which have nothing to do with the substance, but which spring from special arrangements of the atoms determined by motion. We shall shortly see how the fundamental doctrines of the atomical school have been reproduced in our modern mechanical one after a lapse of four centuries.

The Grecian mind was not satisfied with the mechanical explanation of future existence. We consequently see brought to light for the first time by Anaxagoras, an immaterial principle *Nous*—an intelligence apart from all matter, maker of the world. In short, an agent with a definite purpose. This intelligence, although motionless itself, is the cause of movement, and the formation of the *panspermia* or *omeomeria*, as Aristotle calls it, is the result of its action. This is the systematic and beautiful origin of the world.[6]

This intelligence, however, is not a personal god, because it possesses no action in itself and its operation develops solely in the motion and order of matter. Plato and Aristotle are quite right when they blame Anaxagoras for holding to the mechanical doctrine while having an instinctive perception of the final cause.

The *Nous* of Anaxagoras, as Schwegler has observed, closes the period of anti-Socratic realism, that is to say the conception of natural positiveness as represented by ancient Grecian philosophy. Anaxagoras embraced the principles belonging to the preceding schools which he attempted to reconcile, but he made apparent for the first time an *ideal* principle, which being accepted by Socrates, afterwards expressed the new and adverse current of Grecian thought.

Atomism reappeared with Epicurus, not presenting, however, any novel determination, except that the atoms did not all descend in a direct line giving rise to a whirling motion, as Democritus affirms, but proceeded each separately in its own way guided by a kind of free will.

Throughout the long period of ideal speculation which succeeded ancient Grecian philosophy, investigation in regard to cosmic positiveness being looked upon as a matter of secondary importance or else neglected altogether, naturally made no progress whatever Thus we come to the Sixteenth century, during which a single voice in England was raised to deplore the false road upon which human thought had traveled for so long, agitated and confused by empty and useless discussions. Logic seemed to aim towards the "strengthening of error rather than the search for truth." "And this," said Bacon, "can proceed from nothing but the fact that scientific research is alienated from its true source—nature and experience—to which it must return if anything is to be achieved." Although many errors crept into the facts accumulated by Bacon among his perceptions of great truths, he, nevertheless, rendered an immense service to science by recalling it to experiment and to the inductive method. About the same time, a great Italian, Galileo, not only proclaimed the system, but applied it, gathering much more fruit from his enterprise than did the English philosopher.

This was one of the grandest moments known to the human mind. In this period, which we call the Renaissance, while man, no longer satisfied with the narrow boundaries of the old world, discovered new paths while in search of other lands, human conscience oppressed by centuries of overbearing slavery advanced towards reformation. Then speculation, shattering its scholastic fetters, opened a new field for research, and resolved to cultivate it by fresh methods. Later we shall see the abundant fruit which grew, not so much from the field of abstract speculation as from that of natural investigation.

René des Cartes here broke in with past traditions, endeavored to make the research over again from the beginning, and commenced to exclude all supposition and to entertain doubts about everything. But the new structure of facts which he built upon Thought was precisely the contrary of his method. Positiveness according to des Cartes is represented by three substances—God, Mind and Matter. Thought is the attribute and essence of mind; extension is the attribute of matter. Here, then, is cosmic positiveness reduced to nothing more than *expanse*, while in our opinion it is the very opposite. Expanse is merely a relation, and it annuls the absolute condition of existence reducing it to a simple *rapport*.

[6] The panspermic theory affirms that the germs or elements of all things exist in the earth, and only require a particular combination of circumstances to bring them forth.—TRANSLATOR.

In the monadology of Leibnitz, we find a reversion to atomism under an ideal form. He considers the substance of the universe as an active force, represented by monads. These, after the manner of atoms, are a distinct unity, unchangeable and indestructible. Contrary, however, to atoms, which do not present any qualifying diversity in themselves, monads are distinguishable one from the other, each one personating, as it were, a distinct form. Moreover, atoms being capable of expansion, can be regarded as separable, but monads cannot, because they are metaphysical conditions. And inasmuch as metaphysical conditions, no matter how they unite, can never go beyond a certain limit, Leibnitz denies the objective reality of space, and looks upon it as a kind of co-existence.

But the most important part of his doctrine is the conception he places upon the action of monads. Each one has its peculiar representation apart from the other monads and consequently, the universe. All the ulterior developments of the latter are therein portrayed, so that in monads we may read the future. Such representative power is not the same in all of them, however. Some, monads of the lowest degree, have a confused representation which may be compared to vertigo or dreamless sleep; a condition in which representations are not wanting, but being neutralized cannot attain consciousness. These lower orders of monads represent the first link belonging to the chain of existence, which is called inorganic nature, and the bodies resulting from them may be likened to a fish pond whose elements are alive while it is not.

Occupying a higher grade, in the vegetable kingdom, are monads in which representation acts as a formative vital force, but always totally unconscious. Higher still, in the animal world, monad life rises to sensations and memory, and finally to reason and reflex action. However, let us repeat, in order that it may be well understood, that the representative contents of the various orders of monads do not differ, because each one, like God, reflects the entire universe *(parvo in suo genere deus)*. The difference lies solely in the clearness and perfection of the representations.

We will not linger here, however, that we may slowly follow the ideas of Leibnitz in regard to the relations existing between God and monads, or between them and the soul by means of pre-established harmony. We will merely observe that if we remove from monadology all the purely imaginary elements with which it overflows, there still remains something both novel and important which is not to be met with in old atomical theories. This novel determination consists in a peculiar active force which each monad possesses *internally*. It is a prior intuition of *pampsichism* which being enriched moreover by positive facts, can lead the way perhaps, to the greatest reconciliation of which the human mind is capable.

We find another reversion to atomism in the metaphysics of John Frederick Herbart. We have already seen his conception of absolute positiveness. However, experience receives many suggestions from the phenomenal world, which is composed of manifold appearances. And as every appearance insinuates a determined Reality, the latter must be considered as a compound of several single entities or monads, each one posessing different qualities. The individual groups of these monads are those which, working upon our senses, there produce the representation of definite objects. We find a vast difference between Herbart's conception and that of Hegel; while the former considers Nature as a plurality, the latter conceives it to be a unity. To one, absolute positiveness is the Ideal, while for the other, on the contrary, it is Reality.

But how can we reconcile the absolute condition of the Real, the peculiar conservation of monads with the phenomenon of mutation. Herbart has recourse to *accidental perceptions* and *intelligible space*. By accidental perceptions, we mean the manifold relations which can proceed from a single conception, according as it may be compared with others, but, nevertheless, remaining always unchanged. Thus, for example, a straight line can be considered as a radius or as a tangent without changing its position, just as a sound can be harmonious or discordant, according to the relation it bears towards other tones. In the same way, in the grouping of various qualities of monads, while on one side there is no change, on the other there is a very perceptible one. By means of *intelligible space* we may consider existence either as a complex form or as an individuality.

This theory, which in some ways closely resembles the old atomic dogma, is far removed from it, inasmuch as the monad or atom, according to Herbart, does not possess an impenetrable character.

Looked at from a mathematical point of view, several monads may coincide perfectly one with the other. Between the monad of Leibnitz and that of Herbart, there is also a noteworthy difference, because the former considers the *internal condition* as original and individual; while with the latter it is wanting, if we consider a single monad, but develops with the reciprocal relations between the monads.

We will finish with Herbart, our brief explanation of atomism revealed upon a field of pure metaphysical speculation. On the other hand, a new doctrine arises, an experimental one, from which we shall see produced an atomic theory, which is not the work of more or less arbitrary deductions, but the slow result and synthesis of a multitude of positive facts.

ASTRONOMY.

SPECTRUM OF "LALANDE 13412."

We are indebted to Prof. Pickering for the following note upon some observations recently made at Harvard College Observatory:

"The star *Lalande* 13412 has a very curious spectrum. It belongs to the same class as *Oeltzen* 17681 and the three stars in *Cygnus* having bright lines. Besides the yellow and blue bands, it has a marked line in the green, which is faint, if not wanting, in the other stars. It is also about a magnitude brighter than either of them, so that it is the only object of the kind within reach of small telescopes. Professor Young found *Oeltzen* 17681 difficult with 9-inches aperture, while I discovered this object with 4-inches aperture. The position for 1880 is:

R. A. 6^h. 49.3^m.
Dec. —$23°$ $47.'$

or about 15′ north of *o Canis Majoris*. In winter this star is conveniently observed when all the other stars of this class are below the horizon.

The same evening I found that the spectrum of a^2 *Puppis* is banded. As the declination of this star is —44½°, this is probably the most southern object ever usefully observed here. Its altitude at the time of observation was only about 2°!"

The Transit of Venus Commission established by the French Academy of Sciences, has resumed its labors under the presidency of M. Dumas. A credit has been given by the Government for constructing new refractors. Not less than twelve are now building, to be used on the several stations which have been already selected, and will be ready by the end of the year. The heads of the scientific missions will soon be appointed, as well as their staff. The greater number of instruments built for the 1874 transit has been disposed of to several public institutions.—*Nature*. W. C. W.

WASHINGTON, D. C., April 6, 1881.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

It is with mingled pleasure and profit that I have read the very suggestive paper on cerebral nomenclature contributed to your latest issues by Professor Wilder[1]. Some of the suggestions which he has made have been latent in my own mind for years, but I have lacked the courage to bring them before my colleagues. Now that he has broken ground, those who prefer a rational nomenclature to one which like the *present* reigning one, is based upon erroneous principles, or rather on no principles at all, will be rejoiced at the precedent thus set for innovations. As Professor Wilder has invited criticism, I take the opportunity of offering the following remarks upon the leading points of his papers, in so far as they refer to the brain alone.

1. The principles announced are such as zootomists and anatomists generally will agree with, to the fullest extent. He who has himself been compelled to labor under the curse of the old system, the "beneath," "below," "under," "in front of," "inside," "external," "between," etc., of anatomy, as taught at our graduating mills, will look upon the simple "ventral" "dorsal," "lateral," "mesal," "cephalic," (or "nasal" or "proximal") and "caudal" ("distal") as so many boons. I have no hesitation in saying that the labor of the anatomical student will be diminished fully one-half when this nomenclature shall have been definitely adopted. I suppose, however, that the present generation of teachers—I am speaking of our medical schools, not of our universities—will have to become extinct before even the attempt can be made. In Germany the older system has gone out of use almost entirely, and not the least charm about the works of Henle, Schwalbe, Forel, and Gudden, is the fact that these authors have more or less done away with the ambiguous terms once rampant.

2. At present two terms are used convertibly; these are *crus* and *pedunculus*. The chief parts to which these terms are given are the *crus cerebri* (*pedunculus cerebri*) and the *pedunculi cerebelli* (*crura cerebelli*). If anatomists would agree to use the term *crus* only for the cerebral tract, and *pedunculus* for the cerebellar, it would save us the necessity of adding another word. *Crus* would mean what *crus* or *pedunculus cerebri* now designates, *pedunculus* a cerebellar tract. The modifications suggested by Professor Wilder of *præpedunculus*, etc., are excellent. The word *pedunculus* has been applied to a number of other structures, but, I think, inappropriately; thus, *pedunculus conarii*, *pedunculus hypophyseos pedunculus flocculi*, *pedunculus nuclei lenticularis*, *pedunculus substantiæ nigræ*, from all of which it should be removed, as there are other terms in use for these structures, or they are non-descriptive, as the latter two given.

3. In proceeding to comment on some of the terms proposed by Professor Wilder, I wish it to be distinctly understood that I do so merely tentatively and to promote discussion; in so doing I feel certain that I am carrying out that writer's wish. It is but just to state that the majority of the terms cannot be discussed, they are perfection and simplicity combined.

AMYGDALA (*Cerebelli*), W.—Since there is a *nucleus amygdalæ* in the temporal lobe of the cerebrum of man, simians and carnivores, which should be called *amygdala* briefly, just as the *nucleus lenticularis* and *n. caudatus* are termed *lenticularis* and *caudatus* and as the synonym *tonsilla cerebelli*[2] is at our disposal for the similarly named lobule of the cerebellum, I suggest replacing this term as applied to the cerebellum by *tonsilla*.

AREA INTERCRURALIS, W.—I have this term in a manuscript of mine, and am glad to find such a coincidence in baptism, according the priority, of course, to the first publication. I bound this area cephalad by the caudal border of the chiasm, caudad by the cephalic border of the Pons, laterad by the crura, and distinguish the deeper part as a *fossa intercruralis* (*substantia perforata post.*) The gray mass here located is the ventral face of Gudden's[3, 4] interpeduncular ganglion, which I propose, in order to secure nomenclatural uniformity, to term (*Ganglion*) *intercrurale*.

AREA POSTPONTILIS, W.—The objection can be made that this area is not homologous in different animals. A large part of the true Pons in man includes the portion homologous with a part of the *Area postpontilis* of the cat. The roots of the abducens nerve (6th pair) seem to me to constitute a more fixed boundary.

CAUDA STRIATI, W.—I have identified this structure in the cat; it does not make as fine a sweep as in man, but is distinct at the roof of the inferior horn and loses itself as has long been known[5] in the case of the human brain near the *Nucleus amygdalæ*. Professor Wilder's term is the only admissable one, both as being descriptive and on grounds of priority. *Cingulum* is otherwise appropriated.

CONARIUM, W.—Would not the retaining of this name deprive us of that convenient antithesis which can be established between *epiphysis diencephali* and *hypophysis diencephali?*

DENTATUM, W.—Some term should be devised which will at the same time express the fact that this gray mass is a nucleus of the cerebellum and differentiate it from the *nucleus fastigii* (*fastigialis*). *Dentatum* is not appropriate, in my judgment, because in those animals in which it is dentated, there are other dentated nuclei, and also because it is not dentated at all in the rodentia, the carnivora, and ungulata.

EPENCEPHALON.—Are there any reasons why a seperate segment of this name should be made? Some authors limit the term to the cerebellum, which latter is only a dorsal hypertrophy, not an entire segment. The difficulties which Prof. Wilder mentions could be obviated by abandoning the term altogether.

LEMNISCI W.—Can be identified in cat on transverse section; they are not distinct on the surface, nor indeed there well marked in any animal.

LOCUS NIGER.—This ganglion is not black in any animal except man; for this reason I have employed the non-committal designation of *Ganglion Soemmeringii*.[6] It is interposed between *pes* and *tegmentum* like a *diaphragma*.

MONTICULUS.—Modern authors[2], to my knowledge, employ this term only for the highest point of the dorsal cerebellar vermis.

NUCLEUS LENTICULARIS. Might be briefly termed *lenticularis*.

PONTIBRACHIUM, W.—Is identical with the *medipedunculus* of the same author. I have thought that analogous names might be adopted for the other *pedunculi*, thus *Restibrachium*, etc.

STRIATUM. W.—Why not *caudatus*? Both *lenticularis* and *caudatus* are parts of the old *corpus striatum*.

VENTRIPYRAMIS, W.—Since the "posterior pyramids" of descriptive anatomy are no longer known as pyramids, and the more generally used term of *Clavæ* has been employed to designate their intumescence, the prefix *ventri* may not be necessary.

4. Independently of the question of nomenclature, I should like to ask upon what grounds it is stated that *cerebrum* consists of the *prosencephalon* less the *striata*. The tissue of the cortex cerebri and of the two divisions of the *corpus striatum* are even in man continuous, and it would be impossible to peel out the lenticular nucleus from the white substance of the hemispheres. Indeed, embryologically the cortical gray and that of the cerebral ganglia are originally subendymal, and in tracing the development of the brain, as we proceed from reptiles to

man, we find that successively the *caudatus*, the *lenticularis* and the *claustrum* become differentiated from a common gray mass continuous with the cortex at the base of the cerebrum.

I would add in regard to the term CORTEX that the Optic lobes[3, 6] and the Rhinencephalon[5] exhibit the cortical structure as the cerebrum and the cerebellum.

The following terms not included in Professor Wilder's series, are submitted, and for them I invite the severest criticism. Some of them are established by others.

CAPPA (*cinerea*[1])—The gray cap covering the *Optici*, well developed in most mammalia, rudimentary in man.

ECTOTHALAMUS*.—The outer gray thalamic zone.

ENTOTHALAMUS*.—The inner gray thalamic zone.

INTERCRURALE*, (*Ganglion*).—*Ganglion Interpedunculare*[3, 4].

SIGMA*.—The *S* shaped involution of the nerve-cell layer of the cortex which constitutes the basis of the *Hypocampa*.

NUCLEUS TRAPEZII*. — The superior olive. The development of this body seems to bear an inverse relation to that of the true olive. In man the olive proper is highly developed, in the cat poorly—in the latter the nucleus of the trapezium is well marked and folded; in man it is ill-marked.

OBLONGATA*.—The post-pontinal area of man; the *medulla oblongata*.

STRIÆ*.—The *striæ medullares albæ* of the fourth ventricle.

VELUM CEREBELLI*.—The valve of Vieussens; this is the true embryonic starting point of the Cerebellum. The *velum medullare anterius*.

VELUM OBLONGATÆ*.—The *velum medullare posterius*. It arises from the internal division of the *post pedunculus* in its oblongata portion, and covers the posterior part of the fourth ventricle.

VELUM FLOCCULI*.—The *velum medullare inferius*.

GRACILIS* (*Funiculus*).—*Funiculus gracilis*, continuation of corresponding column in cord; part of the posterior pyramids.

CUNEATUS* (*Funiculus*).

TUBERIS* (*Funiculus*).—Funiculus of Rolando; the columnar field containing the Tuberculum of Rolando. There is a *lobulus tuberis*, which is otherwise provided for.

NODI*.— Two symmetrical eminences, situated each in the shallow depression bounded by the *opticus, thalamus* and *habena*, probably corresponding to the *ganglion habenæ (Gangl. habenulæ*[5]*)*. There is a notable large opening cephalad of these eminences, which resembles the opening under the *tænia* containing the vein which gives the latter its bluish color. I can find no notice of this opening anywhere. The eminences are represented obscurely in Fig. 70 of Henle[2].

DECUSSATIO FONTINALIS.**—Fontanen artige Haubenkrenzung.[5]

In conclusion, I would urge the adoption of some brief arbitrary affix or prefix in place of the words commissure and ganglion. He who limits himself to a study of surface contours will not appreciate the absence of such abbreviations as much as he who is compelled to wade through the labyrinth of the internal cerebral structure.

Gris for ganglion would perhaps do; thus *Grishabena, Gristegmentum, Grisfastigium* for *Ganglion habenæ, Ganglion* and *Nucleus tegmenti, Nucleus fastigii*. The term *nucleus* is a very unfortunate one as it has another and very different meaning, which in my experience as a teacher of cerebral anatomy, has led to confusion in the mind of every beginner. Professor Wilder, who appears to be as much at home in etymology as in cerebral anatomy, will solve these problems no doubt better than I could pretend to.

* Terms proposed by myself, not to be found in previous publications.

** A single affix or prefix might be devised in place of *decussatio*, or *fontidecussatio, pinidecussatio, pyridecussatio?*

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3. *Forel.* Untersuchungen uber die Hauben Region, Arch. für Psychiatrie, VII.

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5. *Meynert.* Vom Gehirn der Saugethiere. Stricker's Handbuch II. p. 724, line 11 from bottom.

6. *Spitzka.* The higher ganglia of the mid and hind brain. Journal of Nervous and Mental Diseases. July, 1880. (Designation of figure 10.)

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NEW YORK, 130 East 50th street. E. C. SPITZKA.

HOW DOES GRAVITY CAUSE MOTION?

To the Editor of "SCIENCE:"

The interesting article by Mr. E. L. Larkin in "SCIENCE" for March 26, on the Interrelations of Gravity, Heat, Motion, etc., induces me to offer you some thoughts on the subject, with the hope that I may throw light upon it from another point of view. There is one widely accepted doctrine of modern physics which I confess I could never understand, that of Potential Energy. It may serve as a convenient explanation of the mysteries of falling force to say that energy may be at one time motion, and at another time the possibility of becoming motion. The rule explains the problem, but what explains the rule? Can motion become anything else than motion? Can it now convert itself into Rest, into Gravity, into Potentiality, or into anything else than simply motion? Is it not, like force and matter, an unvarying infinitude of the universe?

Motion means simply the translation of substance through space, and it possesses a fixed energy dependent upon the weight of the substance and the speed of the translation. If the portion of substance moved be a minute portion of matter, either forming an elementary constituent of a solid mass, or a separate molecule of a gas, we call its motion heat; and the result of its impact with exterior particles, temperature. If it be a mass of such particles its translation should be particularized as mass motion. In addition to these modes of motion, Electricity and Magnetism must also be considered as more special modes of motion, unless we admit the possibility of motion becoming something else, and this something else again becoming motion.

Can we admit this? What does terrestrial gravity teach us? If gravity is convertible into motion, then we have reason to conclude that the gravity should disappear as the motion increases. The law of gravitation asserts that the action of the earth and of a falling body are necessarily reciprocal. The earth must fall towards the body with the same energy that the body displays in falling towards the earth. The body, then, can not derive its energy of fall from the earth, unless we claim that the earth derives its energy of fall from the body. Such a cross-lending of force is inadmissable. The energy displayed by the body must come from itself, not from the earth. It is not a transformation of the earth's gravity into motion. Is it a transformation of its own? This we cannot admit, since the body loses no gravity. It cannot well give and keep at the same time. The body falls 16 feet in the first second, and ends with a velocity of 32 feet per second. This 32 feet per second is a positive momentum, and must continue until over-

come by counter force. But if a portion of the gravity of the body has become transformed into this motion there will certainly be less to tr nsform during the next second. Yet in the next second the body adds to its 32 feet per second 16 feet more derived from gravity, and thus falls 48 feet, ending with a velocity of 64 ieet per second. In the third second it adds 16 feet to this 64, and falls 80 feet. And so on continuously, so far as observation has gone.

This certainly does not look like a transformation of gravity into motion, since the gravity appears to continue undiminished. And as to potential force, or possibility of motion, being converted into motion, I shall not attempt to combat it, for it is a sorry task to wrestle with an antagonist who changes into a mist when you attempt to grasp him. Gravity is something definite, whatever that something is, but this will-of-the-wisp of potentiality certainly lacks the bones of a solid body.

But if not gravity or potentiality what is it that becomes motion in the ball that falls towards the earth, and in the earth that falls towards the ball? This may seem a difficult question, and yet it admits of but one answer. Nothing becomes motion. Nothing can become motion. Motion is motion and cannot possibly be or become anything else. The motion which appears in the falling body was not created for the purpose. It existed in the falling body in some other form, and has been simply transformed, not created. Every mass of matter has its internal motions; its electrical, magnetic and chemical energies, which are more or less engaged in preserving the integrity of its molecules or of its mass; and its heat energy, which is engaged in a constant effort to overcome the integrity of its mass. The particles of the mass dart backward and forward continually. They would dart in one direction only were they not restrained by each other's resisting energies, and by external resistance. Consequently, any external energy which aids their vigor of movement in one direction and resists it in the opposite must give them a combined excess of vigor in that direction. They must all move more vigorously in that direction than in the opposite; that is, the mass must move as a whole in that direction. And this movement once gained is positive until overcome by exterior resistance. It is a definite energy which cannot be lost unless it be given to some other substance.

Such is the true principle at work in falling motion. Terrestrial gravity is the external energy which aids the vigor of the heat motion of particles in one direction and resists it in the other. This force is increasing. Although a mass be not fal'ing to the earth its particles are incessantly falling. The supporting body resists their fall and their excess energy in this direction expends itself upon this body. But if the support be removed there is no longer any resistance to their fall. The particles strike further downward than they return, since gravity aids their down stroke and retards their upstroke. Thus at each vibrarion of the particles the mass slightly descends. These slight descents continue. They are the energy derived from the pull of gravitative attraction. But each slight descent produces a fixed vigor of downward motion of the mass as a whole, and this vigor of motion is increased by constant new increments, so that the falling speed of the body rapidly increases.

This is the true meaning of potential energy—a change in the direction of motions already existing. No motion is created, or borrowed from any other condition of nature. The body gains force in one direction under the pull of gravity, but it is the force of a motive vigor which it already possessed, and which, instead of exerting itself equally in all directions, now exerts part of its energy specially in one direction. And this change in the direction of its energies is balanced by an equal opposite change in the direction of the earth's energies. The body does not possess the possibility of always falling, but it possesses the reality of always falling. Its particles constantly fall. But when it is supported their falls cannot accumulate. Each single fall is too slight to be observed, and the effect of each fall is overcome by resistance before another can be added to it. But these persistant falls produce a constant pressure upon the resisting substance, and constitute the weight of the body. It is on removal of the support that these rapidly repeated effects can be continuously added to each other, and become a visible descent. But the distance of the fall of the particles during each vibration is the same whether the body be supported or rapidly descending. It is only the preservation and accumulation of the positive mass motion given to the body by each slight fall, which causes the rapid increase in falling speed. These accumulating motions form an energy of motion separate from that of the fall, and which would keep the mass in motion at a fixed rate of speed were the force of gravity to suddenly vanish.

I would like to say a word here in reference to the presumed heated condition of the nebular mass from which it is claimed that the solar system originated. There is another reason than that advanced by Mr. Larkin, which renders it very improbable that the nebula was greatly heated. It is one thing to contain heat, another thing to be in what we call a heated state, that is, in a state of high temperature. For temperature and absolute heat are very different things. A mass of water at 32° contains far more heat than a mass of ice at the same temperature. And so a mass of water gas at 212° contains far more heat than an equal mass of water at that temperature. This rule probably holds good in all cases; namely, that as density diminishes the heat capacity increases, so that a very rare gas may contain a vastly greater quantity of heat than a solid at the same temperature. We see this exemplified in the matter of space. Heat has been pouring into it from the contracting spheres for an enormous period, yet its capacity for heat is so excessive that this outflowing heat has probably had very little effect in raising its temperature.

Such a consideration applies directly to the original nebula of the solar system. It was a very rare gas, and therefore had great capacity for heat. Its latent heat may have been great, and its effective temperature low. It was only after it began to rapidly lose heat that its temperature rose. For the contraction of the nebular mass must have, by condensing its substance, lessened its capacity for heat. If this change in condition took place more rapidly than radiation could balance it there must have been a steady increase in temperature, instead of a decrease as usually assumed. For all that we know to the contrary this phase of the process may not yet be completed. Contraction of the solar mass may yet be increasing its sensible heat, by lowering its capacity for heat, or its power of containing latent heat, more rapidly than this is balanced by radiation. In such a not impossible condition of affairs the sun would be yet rising instead of lowering in temperature, losing heat while increasing its apparent or sensible heat, and its process of actual cooling be not yet begun.

2223 Spring Garden St. Philadelphia. CHARLES MORRIS.

A CAUSE OF DETERIORATION IN CLOTH.—Goods dyed rust, buff, or chamois shades with salts of iron occasionally undergo a slow combustion. The ferric oxide is alternately reduced by the organic matter of the tissue and re-oxidized by the oxygen of the air.

AT a Berlin feather-dyeing establishment an ostrich feather dyed in shades with methyl-violet was layed upon a paper upon which some ammonia had been poured but had dried up again. After a time the feather became partially green, the green passing gradually into violet, and producing an extraordinary effect. This reaction is being utilized in feather-dyeing, and will probably be applied in the manufacture of artificial flowers.—M. BALLAND.

NOTES.

PERIODIC MOVEMENTS OF THE GROUND.—P. Plantamour gives an account of his observations on the movements of the ground from October 1, 1879, to September 30, 1880. The most remarkable feature is the sinking manifested on the eastern side from the end of November, 1879, to the end of January, 1880, which is much greater than might be expected from the absolute cold of the month of December, only —15°. A rise of temperature is always accompanied with an elevation of the ground level, and a fall of the thermometer is marked by a subsidence.

ON M'BOUNDON, THE ORDEAL POISON OF THE NATIVES OF THE GABOON; NEW PHYSIOLOGICAL, CHEMICAL, HISTO-CHEMICAL AND TOXICOLOGICAL RESEARCHES.—The poison employed contains exclusively one base, strychnine. E. Heckel and F. Schlagdenhauffen propose to examine whether the distinction between the tetanising and the paralysing species of the strychnos family may not depend simply on the proportion of the base which they contain.

ELECTRIC PHENOMENA OF TOURMALINE AND OF HEMIHEDRAL CRYSTALS WITH INCLINED SURFACES.—The hypothesis which J. and P. Curie put forward is that there exists a constant difference of tension between the opposite surfaces of two successive layers. Tourmaline being a compound body the different parts of a crystalline molecule may be formed of different substances, which would explain the difference of tension of the opposite extremities of two molecules.

VIOLET ILLUMINATION OF THE RETINA UNDER THE INFLUENCE OF LUMINOUS OSCILLATIONS.—A. Charpentier, fixing his eyes immovably on a sky illuminated by a uniform white light, and moving two fingers of his right hand rapidly and alternately backwards and forwards before them, saw, after a minute, a remarkable change in the uniform aspect of the heavens. There appeared on a white ground a mosaic composed of rather deep violet-purple hexagons, separated from each other by white lines, and forming a very regular design. The oscillations of the fingers should be from 300 to 400 per minute. He thinks that these hexagons are due to the cones in the fovea and in the yellow spot, and that the white lines are due to their intervals.

A GLYCOSIDE EXTRACTED FROM COMMON IVY.—The glycoside in question, $C_{64}H_{64}O_{22}$, is resolvable into a non-fermentible sugar, which reduces Fehling's liquid, and a neutral body, tasteless, inodorous, dextro-rotary, and agreeing with the formula $C_{52}H_{44}O_{12}$.—L. VERNET.

RADIOPHONY.—Radiophonic effects are thermic, not luminous, and are produced by gases alternately heated and cooled, and not by solids or liquids.—E. MERCADIER.

PERMANENCE OF HYDROCYANIC ACID FOR A MONTH IN THE BODIES OF ANIMALS POISONED WITH THE PURE ACID.—Hydrocyanic acid, if administered in a sufficient quantity to animals, preserves them perfectly for a month. It remains in the tissues, and especially in those of the stomach for the same time. It appears to combine intimately with the animal tissues. In the Carnivora it is more difficult to extract it by distillation than in the Herbivora. C. BRAME.

INFERIOR ORGANISMS PRESENT IN THE AIR.—The microscopic beings in the air are very unequally distributed. The germs of beer-yeast are not everywhere present. Bacteria are much less common than the moulds, such as *Penicillium glaucum*, *Mucor stolonifer*, etc.—E. C. HANSEN.

CHEMICAL CONSTITUTION OF ALBUMEN.—The transformation of albumen into peptones is produced by a hydratation, which in each phase takes place at a fixed part of the molecule. The regressive formation of albumen from its peptones is produced by a similar de-hydration. When the molecule loses calcium and phosphoric acid the carboxylic groups appear, and give an acid reaction to the groups thus obtained. In certain phases the molecule may lose a portion of sulphur without being destroyed or changing its properties.—DR. A. DANILEWSKY.

NEW RESEARCHES ON THE ALBUMENS OF MILK.—The albumen of milk is a mixture of stroma-albumen, with small quantities of orro-proteine and the synto-protalbes. The lacto-proteine of Millon and Commaille is a mixture of soluble synto-protalbes, of snytogenes, and of peptones, which alone are precipitated by mercuric chloride. The same mixture with small quantities of peptones represents the galactine of Morin.—DR. DANILEWSKY AND P. RADENHAUSEN.

DEVELOPMENT OF THE CADAVERIC ALKALIES (PTOMAINES).—MM. Brouardel and Boutmy have verified the presence of these poisons in the viscera of persons who had died either from the action of poisons or otherwise. The organs of an individual asphyxiated by carbonic oxide were analysed some hours after death, and found free from poison. On being re-examined eight days afterwards they contained a solid organic base, presenting the general characters of the alkaloids and proving fatal in small doses to frogs and guinea-pigs. The ptomaines are produced in the dead bodies of men and animals, and vary in their nature under circumstances not yet ascertained. They are poisonous in the majority of cases.

REPORT PRESENTED BY M. TROOST ON BEHALF OF THE COMMITTEE OF THE CHEMICAL ARTS ON THE MALLEABLE NICKEL OF MM. GASPARD AND BELLE.—The metal is first brought to a state of complete fusion, its surface is freed from all traces of scoriæ, a small quantity of metallic zinc or magnesium is introduced, the whole is stirred up and run. The metal thus added seems to lay hold of all traces of foreign matter derived from the sides of the crucible. Such nickel is ductile and malleable at all temperatures below its point of fusion, and can be welded either with itself or with iron or steel. Plates and wires of iron or steel can thus be coated with nickel.

SPECIFIC MAGNETISM OF OZONE.—Ozone being more magnetic than oxygen it is easy to see that the relation of the specific magnetism of ozone to that of oxygen is notably greater than the supposed relation of their densities. The specific magnetism of ozone is then greater than what would correspond to the quantity of oxygen which it contains.—H. BECQUEREL.

DETECTION OF ERGOT IN FLOUR.—The suspected sample is treated with cold ether or boiling alcohol to dissolve the greater part of the coloring-matters of the flour. The residue is then extracted with ether, mixed with a small quantity of sulphuric acid, and the extract is examined with the spectroscope. The ethereal extract of ergot, if concentrated, absorbs all the refrangible portion of the spectrum beyond D; if the solution is diluted, the spectrum is enlarged, and there appear three absorption bands: the first between D and E, wave-length 538; the second between E and F, wave-length 467. Hoffman agitates the acid ethereal extract with a little solution of sodium bicarbonate, which seizes the coloring-matter of the ergot and takes a fine violet color, whilst the coloring-matters of the flour remain in the ether.

ADDENDA.

In "SCIENCE," March 2, in paper on Amplitude of Vibration of Atoms, for paragraph beginning: "For other atoms than hydrogen," etc., read, "For other atoms than hydrogen, where they have the same energy, their amplitude will vary inversely as the square root of their mass, so that for oxygen the amplitude at 0° will be $\frac{.162}{\sqrt{16}} = .04$ its diameter and its maximum temperature will be $6419 \times 4 = 25676°$ Cent. Also the maximum temperature of the sun would be about 500000° Cent." A. E. D.

THE ODONTORNITHES.—In our last week's notice of the Odontornithes, in the middle of the second paragraph, on page 148, the dental series are said not to "reach the tip of either jaw." In place of "either" substitute "the upper."

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JOHN MICHELS, Editor.

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We present in another column a communication from Professor Alexander Winchell, of the University of Michigan, who, in response to a request from ourselves, has presented a very clear statement of his views relating to some conditions of primitive matter. This subject was introduced in "SCIENCE" by Mr. Larkin (March 26), and followed by Mr. Morris in our last issue.

Much difference of opinion exists on this subject, which is one of the highest interest, and we trust that the open discussion we have permitted may elicit some truth, and lead to a removal of many of the difficulties which underlie this question. Without anticipating our opinion on the merits of the respective arguments which have been adduced or the conclusions to which they lead, we may state that both Mr. Larkin and Mr. Morris, in speaking of gravity, magnetism, heat, light, electricity, motion, etc., etc., appear to make statements which do not accord with the standard authorities on these subjects.

That such difference of opinion should exist on what may be considered fundamental points, should cause no surprise, when even the nomenclature of the physical sciences is in a state of confusion. On this subject we refer our readers to an able article by Professor A. E. Dolbear, published in "SCIENCE" last November (Vol. 1, page 238); this paper demands the careful study of all who would take part in this discussion, and affords a basis on which it may be conducted with profitable results.

We make no apology for introducing this subject to our readers, especially as it has been recently mentioned in popular and scientific books, in connection with philosophical, ethical and theological questions. The objective point of Mr. Larkin's arguments appears to be directed against the Nebular hypothesis of Laplace.

Professor H. Helmholtz, whose lecture on this subject has been recently published by Messrs. Appleton & Co., makes a stout defence of this hypothesis. He asserts that "science is not only entitled, but is beholden, to make such an investigation. For her it is a definite and important question, as it involves the existence of limits to the validity of the laws of nature, which rule all that now surrounds us; the question whether they have always held in the past, and whether they will always hold in the future; or whether, on the supposition of an everlasting uniformity of natural laws, our conclusions from present circumstances as to the past, and as to the future imperatively lead us to an impossible state of things; that is, to the necessity of an infraction of natural laws, of a beginning that could not have been due to processes known to us."

As Mr. Helmholtz observes, to commence such an investigation as to the possible or probable primeval history of our present world considered as a question of science, is no idle speculation, for it is a question as to the limits of its methods, and as to the extent to which existing laws are valid.

We have received several interesting letters on this subject which will be found in our next issue.

THE SEA-SIDE LABORATORY.

The liberality and co-operation of the Woman's Education Association enables the Boston Society of Natural History to announce that a Sea-side Laboratory, under the direction of the Curator, and capable of accommodating a limited number of students, will be open at Annisquam, Mass., from June 5th to September 15th.

Annisquam is situated on an inlet of Ipswich Bay, on the north side of Cape Ann, and is about three and a half miles by coach from the Eastern Railroad Company's station in Gloucester.

The purpose of this Laboratory is to afford opportunities for the study and observation of the development, anatomy and habits of common types of marine animals under suitable direction and advice. There will therefore be no attempt, during the coming summer, to give any stated course of instruction or lectures.

It is believed that such a Laboratory will meet the wants of a number of students, teachers and others *who have already made a beginning in the study of Natural History.* Those who have had some limited experience in a laboratory, or who have attended the practical lessons given by the Teachers' School of Science of the Boston Society of Natural History, are sufficiently qualified to make use of this opportunity.

The work in the Laboratory will be under the immediate care of Mr. B. H. Van Vleck, Assistant in the Museum and Laboratory of the Boston Society of Natural History, a thoroughly competent instructor, and one who has also had long experience in collecting and observing at the sea-side.

Those who would avail themselves of this excellent opportunity to study living objects at the sea-shore should make application to Mr. Alpheus Hyatt, Curator of the Boston Society of Natural History,

THE AMERICAN CHEMICAL SOCIETY.

The April meeting of the American Chemical Society was held on the evening of the 1st inst. There not being a quorum present at eight o'clock, the business of the Society was postponed, and the reading of Dr. A. R. Leed's paper on "Anilo-metallic Compounds" took place. The aniline compounds of Aluminum, Antimony, Barium, Calcium, Cadmium, Chromium, Copper, Cobalt, Bismuth, Mercury, Tin and Zinc were described; how they were prepared and their important characteristics noted. Aniline will not combine with any monivalent element. This paper was a preliminary report of work which Professor Leeds proposes to extend and ultimately publish when he shall have obtained sufficient data. The second paper on the "Action of Concentrated Sulphuric Acid on Lead Alloys" was read by Lucius Pitkin, one of the most talented young chemists of the School of Mines, N. Y. In a paper presented by Mr. James Napier before the Glasgow Philosophical Society, it was held that impure lead was preferable to the pure article for use when in contact with sulphuric acid (see *Chemical News*, Dec. 23, 1880). Mr. Pitkin tried the action of both hot and cold concentrated acids on some forty samples of lead and its alloys. The alloys treated were of lead with antimony, tin, bismuth, cadmium, silver and zinc. In the case of cold acid, 2 sq. in. of each alloy and a sample of pure lead were exposed for 24 hours to the action of 10 c.c. of sulphuric acid at 20° C.; on hot acid the length of exposure was one hour, and his results are best given in the following table, with which his paper terminated.

Average solubility or liability to formation of sulphate of the alloys in terms of lead.

	Cold Acid.	*Hot Acid.*
Lead	1.	1.
" alloyed with Antimony	.81	2.75
" " Tin	1.42	.75
" " Bismuth	1.10	7.69
" " Cadmium	.86	1.10
" " Silver	.87	.93
" " Zinc	1.53	1.10

Considerable discussion followed Mr. Pitkin's paper, in which Dr. Gallatin, Dr. Geyer, Dr, Alsberg, Mr. Herreshoff and Dr. Squibb participated.

Mr. A. E. Hoppick was then elected a regular member of the Society, and Messrs. C. P. Sawyer, A. H. Van Sinderen, and Otto Grote, were proposed for election. Mr. J. H. Stebbins was elected to fill the vacancy caused by the resignation of Dr. Gallatin, as Recording Secretary, and Mr. Herreshoff elected to the positien on the Committee on Nominations which Mr. Stebbins had held.

Mr. Casamajor and Dr. Alsberg reported on behalf of the Committee for the Annual Dinner, and announced that the fifth anniversary dinner of the American Chemical Society would take place at Sieghörtner's restaurant, on Monday, April 18, at 6 P. M.

M. B.

ON SOME PHENOMENA PRESENTED BY VORTEX-RINGS.

PROFESSOR A. E. DOLBEAR, TUFTS COLLEGE, MASS.

1. If one vortex-ring strikes another vortex-ring upon the edge the two rings will bound away from each other as though they were solid elastic bodies, each one vibrating as it recedes.

2. If one vortex-ring overtakes another ring, both moving in the same straight line, and both are of the same size, then the forward one will expand in diameter, and the latter will contract in diameter, and will go through the forward one when each will return to its original dimension. At the same time the forward one will have its velocity retarded while the other will have its velocity increased, and it may overtake the forward one and go through it.

3. If a vortex-ring passes near any light object as, for instance, a silk thread suspended, or better still a small cloud of smoke or ammonium chloride dust, the latter will be seen to be apparently repelled from the front of it but attracted and drawn into the ring from the rear.

4. If a vortex-ring be projected parallel with any surface, and at not too great a distance from the surface, the ring will move in a curved path towards it and strike it.

5. If two vortex-rings are projected so as to start in parallel lines near to each other they will approach each other until they touch, when they may be either broken or else bound away from each other as in the first case above.

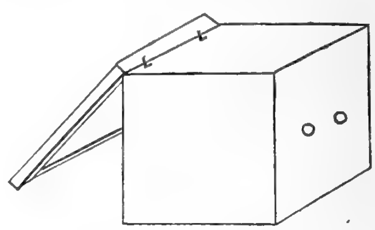
6. If two vortex-rings having the same rate of rotation be started in lines parallel to each other and at not too great a distance apart, they will not only approach each other but *they will combine to form one ring* which continues to move in the same direction.

7. The combination is effected by the breaking of each at the point of contact, and the welding of the opposite parts of each ring to form one ring with twice the diameter.

8. Three rings may in like manner be combined into one.

9. The structure of the vortex-ring is concentric, that is, a cross section of a ring generally shows a series of several concentric circles, with a hollow centre. The middle of the ring appears to be a cylindrical unoccupied space.

As experimental work with such rings is very entertaining as well as suggestive of the behavior of the real atoms of matter, it may be well to give the simple instructions necessary to perfect success.



Provide a cubical box with dimensions about a foot each way, having a swinging back frame, over which is stretched a piece of stout cotton cloth. On the opposite side two or more inch-holes may be bored two inches apart. Pour some strong hydrochloric acid into one saucer, and some strong ammonia water into another. Set the two into the box, and shut down the door. The box will at once be filled with the white fumes, and a tap with the finger upon the cloth back, will send out well-formed rings.

The phenomena 1 to 5, can best be seen by employing only one of the holes, so as to form but a single ring. By striking the cloth a little harder the second time than the first, the second ring may be made to overtake the first, and if it is desirable to exhibit the rings to a room full of people, there should be but a single hole in front, and that one about three inches in diameter; the rings can then be projected with force enough to make them go ten or fifteen feet from the box.

The other phenomena can best be studied by using only small holes, and tapping gently. The rings will come together within a few inches of the box. It seems to be essential that the two rings that combine, should have the same *rate of rotation*, a matter easily secured by forming the two at once in the above described way, but well nigh impossible, if one is formed after the other.

It is sufficient now to remark that the new phenomena described above simulate in a very striking way, what we call gravitation and chemism.

THE CO-EFFICIENT OF SAFETY IN NAVIGATION.*

By Prof. W. A. Rogers, of Harvard University.

It is customary among engineers and architects, in making allowance for the strain to be borne by any part of a structure, to assign to the materials used, a strength sufficient to withstand a strain somewhat greater than the structure is ever likely to be subjected to. By experiment it can be found, for example, what is the "breaking load" of a wooden or iron beam of given dimensions, and an empirical law is established which will give us approximately the breaking load of any beam, when we know the dimensions, material, etc.; but in order to cover all possible differences which may exist in various beams, a *Coefficient of Safety* is either introduced into the formula itself, or is applied to the result obtained from the formula. This co-efficient should be large enough to cover, not only the largest possible deviation between experiment and theory, but also to meet all unforeseen emergencies, such as time and age inevitably bring.

Passing now to the consideration of the term "*Co-efficient of Safety*," as applied to navigation, it is our object to find the limits within which, under ordinary circumstances, a vessel can be located at sea, and then adduce some considerations which will enable us to form an intelligent judgment in regard to the range of error to which observations are liable. The quantity wanted is the average number of miles error in latitude and longitude, which we may fairly charge upon a single observation at sea, under ordinary circumstances. We have then to find the co-efficient by which this number must be multiplied in order to secure absolute safety, as far as safety depends upon human means and exertions.

By an examination of the "British Wreck Register" and the official inquiries made into the causes of disasters at sea, it will be seen that the ratio of loss compared with the increase of tonnage afloat, has for many years been steadily increasing. This inquiry is, therefore not an idle one. It is our purpose to examine only those causes of wrecks which, in a measure, seem to have escaped attention in official investigations. They are :—

I.—Wrecks produced by causes clearly beyond human control.

II.—Wrecks resulting directly or indirectly from over-insurance.

III.—Wrecks caused by the deviation of the compass.

IV.—Wrecks caused by errors of observation at sea.

The first inquiry is an important one, since, if we can find how many wrecks are beyond human control, we ascertain, at the same time, how many are *within* human control. The method of investigation is by the examination of records of Courts of Inquiry for twenty years. Between 1785 and 1813 no less than eight British ships were either wholly or partially disabled by lightning. Of course, vessels lost and never heard from should be added to this list. Between 1864 and 1869 we find from the insurance records that 9999 sailing vessels and 589 steamers, or a total of 10,588, were wrecked. Of this number, the end of 846 is entirely unknown, or one-eighteenth of the whole number. It is probable, therefore, that seven out of ten wrecks occur from preventable causes.

In regard to the second head, it is certain that more insured than uninsured vessels are lost, and in not a few cases it has been possible to convict shipowners of purposely destroying their vessels.

The compass problem is an intricate one, and has never been fully solved, though the researches of Flinders, Barlow, Scoresby, Airy and Harkness have done much to convert great uncertainty into tolerable certainty. The first observations on the variation were by Bond, in 1668. It is well known that the variation of the needle is very irregular. There are yearly, monthly and diurnal inequalities, the diurnal variation being discovered by Graham, in 1722. But the complexity of the problem does not stop here. The tendency of the present time is to build iron ships, and all ships now have more or less iron in their construction. These ships become, to a greater or less degree, themselves great magnets. In wholly iron ships the uncorrected deviation of the needle often amounts to 50°, thus rendering it utterly useless. The Admiralty Law in regard to "swinging" for the variation of the compass is a very clear statement of the case. It reads as follows :

"As the deviation or error of the compass caused by local attraction of the ships becomes changed in amount by any change in the ship's geographical position, and may be entirely reversed in its direction by the ship's proceeding from the Northern to the Southern Hemisphere it is to be invariably tested by azimuth and amplitude observations at sea, and the ship is to be swung for ascertaining the change of error on arrival at a foreign station, and also once a year, and the same is to be inserted in the log book and sent to the Admiralty with the quarterly return for December."

The next important discovery in this connection was by Barlow, who found that all the influence of iron bodies exerted on the compass resides on the surface. This discovery paved the way for Airy's method of correcting compasses, which is by swinging the ship in the usual way, and then correcting the local attraction of the ship by means of permanent magnets of soft iron conveniently placed with respect to the compass. But the most important discovery was made by Dr. Scoresby. He found that every iron ship is itself a magnet, and that it gets its magnetism while building by the inductive magnetism of the earth, the poles of the ship's magnetism depending on the position of the building yard and the direction of the keel in construction. Dr. Scoresby made the voyage of the world in the *Royal Charter*, to test his theory, and found it fully confirmed. Before starting, his compasses were corrected by Airy's plan. On arriving at Melbourne, it was found that a complete inversion of the ship's magnetic polarity had taken place. Every stanchion, every standard, every davit, every mass of iron about the deck had in its upper surface acquired a Northern, instead of a Southern polarity, and the starboard compass had lost nearly one-half of its original errors. On returning to the place of starting in the Northern Hemisphere, and swinging the ship, it was found that a re-inversion had taken place, but the compasses did not quite return to their original deviations, but retained a fraction of their errors. It has since been found that these changes are much greater in steam than in sailing vessels, as shown by observations on board the *Vulcan* (steam), and the *Pandora* (sailing).

In 1852-53, Dr. Scoresby, in a paper before the British Association, showed that there is a sensible difference in the deviation before and after steam is up. It has been said that the compasses of a steam vessel, when light and running before the wind, with a high sea, are practically useless.

More recent experience has shown that the magnetism of an iron ship does not attain its normal condition till some twelve months after launching, and that for some time the variation is very irregular. In the *Great Eastern* a fixed compass changed its deviation nearly 3 points in the first 9 months of service. The observations of Prof. Harkness on board a monitor seem to be conclusive on this point.

We come now to the consideration of the fourth point. As early as 1598, Spain offered a reward of 100,000 crowns for the discovery of a correct method of finding the Longitude at sea. The States of Holland, at an early date, offered a reward of 100,000 florins, and France a reward of 100,000 livres. In 1714 the British Government offered a reward of £10,000 to any one who should

* Abstract of a paper read before the Naval Institute of Annapolis. Prepared under author's direction.

discover a method of finding the longitude at sea within 60 miles, £15,000 if with 45 miles, and £20,000 if within 30 miles. This offer did much to awaken interest in the subject. Though we have long since passed the lowest limit then mentioned, 30 miles, it is doubtful if any two navigators will agree as to what limit we have actually reached. The general testimony of sea-captains, in answer to my inquiries on this point, is that one mile is the ordinary limit within which the co-ordinates of a ship's place can be determined. A few placed the limit at half a mile. Only one navigator, with an experience of 30 years, placed the limit at 5 miles.

Two methods were proposed for the solution of the problem. Morin proposed what is now substantially the Lunar Method, and Maskelyne undertook the solution of the problem by observing the astronomical phenomena, such as eclipses of Jupiter's satellites. On the other hand, mechanicians devoted every energy to the mechanical problem. As the result of these labors, we have two essentially different methods for the determination of Longitude at sea.

I. By Lunar Distances, Occultations and Eclipses of Jupiter's Satellites, &c.

II. By Chronometers, assuming a rate at the beginning of the voyage.

The latter has for a long time been regarded as the more accurate method, but the difficulties to be overcome can be readily imagined, when we consider that even in the determination of the position of fixed observations, in which appliances of the utmost refinement are at hand, the places vary widely from the truth. For example, we find variations in the measured difference of longitude between Greenwich and Paris, as great as 5.5^s, or 1¼ miles, existing previous to the introduction of the telegraphic method of determining, longitudes. The range between the earlier determinations of the difference of longitude between Greenwich and Brussels is 10 miles. Moon Culminations are more accurate than Lunars, but the same in principle. They are the more accurate when the longitude depends upon observations at each station, since the errors of Tables are thus eliminated. From a careful discussion of a long series of observations made at fixed observatories, with the most perfect instruments, it is found that we must expect from the Lunar Method an absolute error of six miles as the result of any number of observations. This corresponds in a general way with Prof. Peirce's investigation. He found that the ultimate limit, when one limb of the moon was observed, to be 0.55^s. "Beyond this," he says, "it is impossible to go with the utmost refinement. By heaping error upon error, it may crush the influence of each separate determination; but it does not diminish the relative height of the whole mass of discrepancy." But the discrepancy between the results for different limbs of the moon often amounts to 10^s in the mean determination of a year. The assumption that the ultimate limit of accuracy is as great as 1^s seems to be a very moderate widening of the limits. I find it to be 2.4^s.

For fixed observatories, using the moon's tabular place, we must expect an error of 3.1 miles, with a range of 12.9 miles. For Lunar Distances with sextant, on land, we must expect an error of 10.2 miles, with a range of 24.2 miles. For Lunar observations at sea these quantities should at least be doubled.

We now come to the subject of chronometers. The sources of errors are:

(*a*) Variations of rate arising from the action of magnetism. Airy's experiments show an extreme variation of 5.8^s in the daily rate of a chronometer, due to terrestrial magnetism.

(*b*) When chronometers are swung on the same support, it is probable that there is a sympathetic action between them, similar to the results recently found by Mr. Christie with the Transit of Venus Clocks.

(*c*) Variation on account of change of barometric pressure. This varies between 0.3^s and 0.8^s per day for every inch of change in the barometer.

(*d*) Variation between land and sea rates. Almost every chronometer will change its rate, when its circumstances, either of rest or motion, are changed. The Boston standard clock of Messrs. Bond & Son, almost invariably has a different rate on Sunday from any other day of the week. So, also, it has a change of rate when the streets are covered to any considerable depth with snow.

(*e*) Variation of rate at sea, on account of change of temperature. Mr. Hartnup, of Liverpool, was the first to give, not only the general rate of a chronometer, but also the rate for different temperatures.

An elaborate discussion of the errors of chronometers, from data collected at the Greenwich Observatory, from chronometric expeditions and from chronometers used in the Merchant and Naval services, the following result has been reached.

At the end of 20 days the navigator must expect an average error of 36 miles. He must look out for an error of 36 × 32 or 11.5 miles, and the amount of his error may prove to be twice this quantity, or 21 miles, all on the supposition that he has an average chronometer, and this is independent of the errors of observation, which must still be added.

We come finally to the consideration of the problem,—How near is it possible to find the place of a ship at sea by astronomical observations, taking into account all the errors to which observations are liable?

For the sake of simplicity we shall consider but one method, the method usually followed, viz.: by measurement of the altitude of the sun with a sextant, at a given time before it comes to the meridian for longitude, and the measurement of its culmination, for latitude.

We must first of all ascertain the magnitude of the errors to which observations with the sextant are liable. The following are some of the errors which we must ordinarily expect in observations with this instrument:

(*a*). Instrumental errors, such as eccentricity, errors of graduation, index error, &c. Errors of this class often exceed one minute of arc, even in a first-class instrument.

(*b*). Errors in noting time. No observer at sea pretends to note the time closer than one second. If we assume this low limit and multiply the co-efficient 3.5 already found, we have an error of nearly one mile.

(*c*). Errors arising from imperfect sea horizon.

(*d*). Errors arising from the use of approximate data.

(*e*). Errors depending on the latitude of the ship and the time of observation. By combination in the same direction, errors of this class may be very large, and, for the most part, they escape the attention of the navigator. The most favorable time for an observation of the sun for longitude is when it is exactly east or west. Here an error of one minute of arc in the observed altitude produces an error of the same amount in the resulting time, but if the observation is made 40 minutes from the meridian, an error of one minute in altitude may produce an error of 6 miles in the resulting position.

(*f*). Errors arising from the error in the estimated run of the ship between the morning and the noon observations.

The data for assigning a limit to the errors of observation with the sextant are as follows:

I. *Observations on Shore.* From a discussion of the observations by Williams in 1793, by Paine in 1831, by various observers at Willets Point in 1869, 1871 and 1872, by Hall and Tupman at Malta and Syracuse, and by Newcomb and Harkness at Des Moines, we find that for *latitude* the average error of observation with the sextant is 8″, that the average range between the greatest and the least results of a given series is 36″, the latter value having a range between 14″ and 59″. The coefficient comes out 4.4. For *time* the average error is 1.1^s, the range is 4.7^s, and the coefficient is 4.4.

II. *Observations at Sea.*

Under this head three distinct investigations have been made, as follows;

(*a.*) From an examination of the results obtained by chronometric longitude expeditions, we find that for a voyage of 15 days the average error is 5.3^s; the range between the greatest and the least results in each series is 18.0^s; the latter value has a range between 1.5^s and 55.0^s, and the coefficient is 3.4.

(*b.*) The longitudes of 36 stations have been determined by various British naval expeditions. The chronometers were rated at the Greenwich Observatory before starting, and the observations for time at the terminal stations were made in the usual way with the sextant. Evidently more than usual care was taken both with the observations and reductions. We find that the average difference between the results obtained by different chronometers is 4.4 miles with a range of 15.1 miles. The average range between the different results for longitude is 5.0 miles with a range of 31.6 miles. The average number of chronometers was 11, and the average duration of voyage was 11 days.

(*c.*) During the spring and summer of 1880 Officer W. H. Bacon, of the Cunard steamer "Scythia," kindly undertook for me a series of systematic observations from which the relative errors could be determined with considerable certainty. A complete series for a single day consisted of five sights at intervals of fifteen minutes, about 8 o'clock in the morning, five sights in the neighborhood of 11 o'clock, and five sights at the corresponding hours in the afternoon. Observations were also made when the ship was in known positions as often as possible.

This series of observations has an exceptional value on account of the conscientious fidelity with which the programme was adhered to and of the skill with which they were made. The relative errors were determined by comparing each position with the mean of the series, the rate being determined both from the morning and afternoon observations and from the log.

The results obtained are found in the following table:

Limits in Miles.	Average Error from Observations at 9^h and 3^h.	Average Error from Log at 9^h and 3^h.	Average Error from Observations at 11^h and 1^h.	Average Error from Log at 11^h and 1^h.	Difference between Observation and Log at 9^h and 3^h.	Difference between Observation and Log at 11^h and 1^h.
	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.
0.0- 0.5	1	0	0	0	7	6
0.5- 1.0	9	6	2	3	1	2
1.0- 1.5	8	13	3	5	3	3
1.5- 2.0	4	5	3	3	3	2
2.0- 2.5	6	4	6	5	2	3
2.5- 3.0	2	1	3	4	1	0
3.0- 3.5	2	2	6	5	7	2
3.5- 4.0	4	1	4	5	1	2
4.0- 5.0	1	3	6	5	4	4
5.0- 6.0	0	0	2	1	1	5
6.0- 7.0	0	0	2	1	2	2
7.0- 8.0	1	1	0	1	1	1
8.0- 9.0	2	0	1	1	0	2
9.0-10.0	0	1	0	0	1	2
10.0-11.0	0	0	0	0	1	1
11.0-12.0	0	0	0	0	2	1
12.0 +	1	1	0	0	0	0

QUERY.

A SUBSCRIBER would like to know the best method of mounting Triple phosphate crystals (dry) so as to tack them to the slide without interfering with definition. —Replies invited.

ON THE ACTION OF BACTERIA ON VARIOUS GASES.*

By F. Hatton.

The experiments were made to ascertain the nature of the action exerted by various gases on the life and increase of bacteria, and to observe what influence the bacteria had on the percentage composition of the gases. The bacteria were obtained by shaking fresh meat with distilled water. The aqueous extract was filtered and exposed to the air for twenty-four to thirty-six hours; it was always found to be full of bacteria. A small flask was half filled with mercury, filled up with the bacteria solution, and inverted in a mercury trough. The gas under examination was then passed up, a small glass vessel was introduced under the mouth of the flask, and the whole removed from the trough. The liquid was examined daily as to the condition of the bacteria, the sample being removed by a piece of bent glass tubing having an india rubber joint. After about a week the gas was pumped out by means of a Sprengle and analyzed. Atmospheric air was first tried. The bacteria lived well during the fifteen days of the experiment (T. 15° to 22°). A large absorption of oxygen took place, but it was not replaced by carbonic anhydride; in a second experiment (T. 25° to 26·50) 20 per cent. of the oxygen disappeared, and only 17 per cent. of CO_2 was formed. Pure hydrogen after fourteen days had no action on the bacteria; the gas contained 0.34 per cent. CO_2, 98.94 per cent. H. Pure oxygen after ten days was converted into CO_2 29.98 per cent., O 70.02 per cent. A mixture of CO 46.94 per cent., CO_2 1.27, O 1.27, N 50.51, was next tried after fourteen days; the gas contained CO_2 17.77, CO 0.55, H 7.58, CH_4 2.50, N 71.57. In all of the above cases the bacteria flourished well. Cyanogen was next tried. The solution of meat turned gradually to a thick black fluid. On the fifth day very few bacteria could be seen. From this time, however, they increased, and on the twelfth day were comparatively numerous. On the fifteenth day the gas was analyzed; it contained CN 5.35, CO_2 57.59, O 2.24, N 34.79; a second experiment gave similar results. It appears, therefore, that cyanogen is fatal to bacteria as long as it exists as such, but that it soon decomposes into ammonic oxalate, &c., and that the bacteria then revive, especially in sunlight. Sulphurous anhydride was next tried; the bacteria lived during the fifteen days: the gas contained CO_2 7.87, O 0.00, N 2.13, SO_2 90.10. Similar results were obtained with nitrogen, nitrous oxide, nitric oxide, carbonic anhydride, a mixture of H and O obtained by the electrolysis of water and coal gas. In all cases the bacteria lived well during the experment. The author next experimented with a solution of urea (0.98 per cent.) and phosphate of potash (0.4 per cent.), sowing it with bacteria. The bacteria lived well during the fourteen days of the experiment; small quantities of gas were evolved containing 0.53 per cent. CO_2, 2.64 per cent. O, and 96.82 per cent. N. An experiment was made with spongy iron, air, and bacteria. On the fourth day, all the bacteria had vanished; the air was analysed on the fifth day, and consisted of CO_2 0.26, O 0.00, and N 99.74 per cent. Experiments were also made with acetylene, salicylic acid, strychnine (10 per cent.), morphine, narcotine, and brucine; none of these substances had any effect on the bacteria. On the other hand, phenol, spongy iron, alcohol, and potassium permanganate were very destructive to these microscopic growths.

Mr. W. M. Hamlet said that these experiments confirmed some observations of his own. He had found that bacteria could exist in almost anything—in carbonic oxide, hydrogen, 1 per cent. creosote, phenol, methylamin, methylic alcohol, chloroform. Moreover, Crace-Calvert had shown that they could live in strong carbolic acid. In

* Read before Chemical Society, March 3, 81. This paper obtained for the author the Frankland Prize of £50 at the Institute of Chemistry.

reply to Mr. WARINGTON the speaker said that the acetic acid fermentation went on in the presence of chloroform.

Mr. KINGZETT called attention to the fact that the oxygen was completely used up when the meat infusion was placed in contact with air. He did not think the experiments represented the action of bacteria on gases or of gases on bacteria, but rather the effects of various gases on the mode and extent of ordinary putrefaction.

Dr. FRANKLAND expressed his satisfaction with the results obtained by the author in his laborious research. He must confess that these results had surprised him not a little. The fact that bacteria, which were real organisms and could not be shielded under the term putrefaction, lived and flourished in SO_2, CO, CN, &c., seemed to him very extraordinary, and the question arose whether the germs to which infectious diseases were probably due were not similarly endowed with a power of great resistance to ordinary influences.

Mr. F. J. M. PAGE said that Dr. Baxter had proved that with some fever-producing liquids, their virulence was destroyed by chlorine and sulphuric acid, and that he had seen some experiments at the Brown Institution which led to the same conclusion; so it seemed that, at all events in some cases, the virulence of infective liquids was due to organic matter, essentially different from the bacteria observed by Mr. Hatton.

NOTES ON CHICKEN CHOLERA.

We observe in a recent number of the *Chemical News* that C. T. Kingzett, F. C. S., points out, that, in explaining the protective influence of repeated inoculations with the attenuated virus of chicken cholera, against the more virulent forms of this disease, Pasteur finds it "impossible to resist the idea that the microscopic germ which causes the disease, finds in the body of the animal conditions suitable to its development, and that to satisfy the necessities of its life the germ alters certain substances, or destroys them, which comes to the same thing, whether it assimilates them or whether it consumes them with oxygen borrowed from the blood."

So, again, in cases where complete immunity has been attained, the birds "no longer contain food for the germ."

More striking still is the following passage in reference to chickens which are born proof against cholera:— "Animals in this condition may be said to be born vaccinated for this disease, because the fœtal evolution has not placed in their bodies the proper food of the parasite, or because substances which would serve as such food have disappeared while they were yet young.

Now whether or not we may be prepared to regard the said parasite as the direct cause of the disease, it is remarkable that the reasoning of Pasteur should have culminated in the conclusion upon which Liebig insisted with considerable power.

If we turn to Gregory's (3rd) edition of Liebig's "Animal Chemistry" (p. 205) we find the following passage:—"The condition which determines, in a second individual, his liability to the contagion, is the presence in his body of a substance which by itself, or by means of the vital force acting in the organism, offers no resistance to the cause of change in form and composition operating on it. If this substance be a necessary constituent of the body, then the disease must be communicable to all persons; if it be an accidental constituent, then only those persons will be attacked by the disease in whom it is present in the proper quantity and of the proper composition. The course of the disease is the destruction and removal of this substance: it is the establishment of an equilibrium between the cause acting in the organism which determines the normal performance of its functions and a foreign power by whose influence these functions are altered."

I repeat that to me it seems somewhat remarkable that the investigations and reasoning of two such eminent (and in many matters diametrically opposed) thinkers should have culminated in the same conclusion as regards the conditions of the living body which subject it to, or protect it from, infection.

While, however, it can be readily understood how a profuse growth of parasites could quickly alter or destroy a comparatively large amount of substance—as, for instance, happens in ordinary putrefaction—it does not appear to me so easy to accept Pasteur's reasoning as to his so-called vaccination.

In this inflicted process an attenuated virus is introduced into the body of a chicken which becomes ill but does not die. It does not die because, if Pasteur be correct, the parasites do not sufficiently multiply. Why do they not multiply? It cannot be on account of the insufficiency of the pabulum, for in the large majority of cases where death results this seems to arise from the very profusion of the growth of the parasite when more freely introduced.

Can it be expected, therefore, that even, say, in three successive inoculations the substance which I have here spoken of as pabulum can be entirely removed or destroyed by the very limited number of parasites which are introduced by the inoculations, and which so soon perish in the body? I think this cannot be expected; but if it may be, then the particular substance or substances upon which the parasites prey must be extremely limited in quantity. After all, we are faced with the enormous difficulty of ascertaining the nature of such substance, and the further equally great difficulty of understanding why an undiscovered and undetermined substance should be entirely absent from the bodies in some animals and present in varying proportions in others.

Here we come in contact with the weakest point in the parasitic theory. The immunity from a second attack of an infectious disease of the class in question is simply inexplicable under the parasitic theory. We are forced back to an alternative theory, and that is one of which we at present only recognize the beginnings.

A NEW CORTICAL CENTRE.*

By GRAEME M. HAMMOND, M.D., NEW YORK.

Physician to the Department for Diseases of the Nervous System in the Metropolitan Throat Hospital.

Some six years ago there appeared in the *Centralblatt*, Nos. 37 and 38, a short communication by Betz, embodying an account of certain nerve-cells found by him in the cortex of a region of the brain which he newly named the paracentral lobule. This paper has probably aroused more general attention among neurologists than any other paper of recent times dealing with the structure of the cerebral hemispheres, and this, on account of the anatomical confirmation which the discovery seemed to furnish, of the localization doctrine based on the electrical stimulation of the cortex carried out by Hitzig and Fritsche.

After localizing these cells chiefly in the paracentral lobule and the upper ends of the pre- and post-central gyri of man, stating them to be very few in number in the lower halves of these gyri, Betz proceeds to say, "the constancy of the occurrence of these cells, not only as regards the cortical layer, but also the special convolutions in which they are found, led me to direct my attention to that portion of the brain of animals, and particularly of the dog, on which latter Hitzig and Fritsche obtained such brilliant physiological results. I refer to that lobule which bounds the sulcus cruciatus. Now I found in this very lobule in the dog, cells in similar nests and of a similar shape. With the dog as in man they are distributed in the fourth layer."

Engaged in a study of the ganglionic masses of the forebrain of the cat, an animal on which the experiments of Hitzig and Fritsche have been repeated, and in which

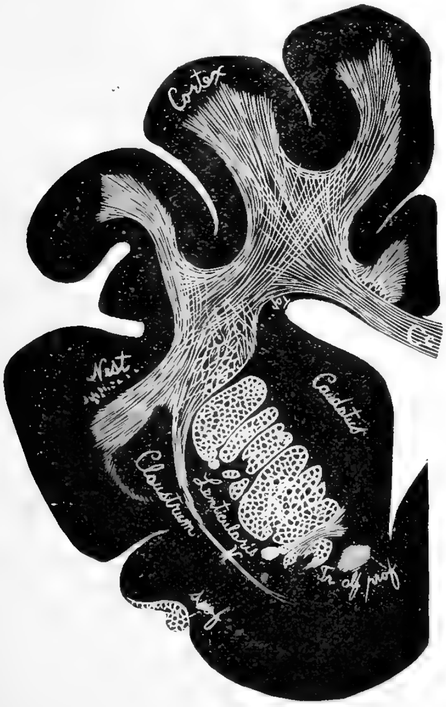
* Read before the New York Neurological Society, February 1, 1881.

the centres have been localized in regions homologous to those of the dog, and in which, furthermore, the architecture of the cortical surface is fundamentally the same, I proceeded to review the question of the localization of the giant-cells.

On the one hand, Betz seemed to argue that the giant-cells, which he claims to have discovered, were motor, because they were found at those points in the dog's brain where Hitzig and Fritsche, by supposed localized electrization, had produced contractions of special groups of muscles. Again, on the other hand, it is apparent that those interested in defending that narrow localization theory, which is such a prominent feature in the teachings of Charcot and Ferrier, have found one of their strongest supports in the anatomical discoveries of Betz.

Let us suppose then, for the sake of argument, that it be granted that larger cells mean motor centres for larger muscles; taking up the localizationists on their own ground we will examine the location of these giant-cells in a cat's brain, which only differs in a single exception from the dog's, and is therefore a fit subject for study. In fact, the cat's brain has the advantage of being somewhat simpler.

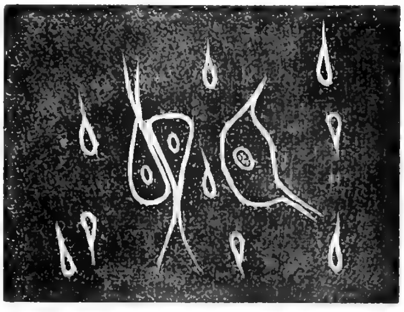
The results I have obtained are based upon the study of the cortical area of the two hemispheres of one cat. One hemisphere was cut as a whole into some seventy-five sections, from different altitudes transversely to the cerebral axis. The other was separated into eleven segments, and each segment cut into a number of thin sections. The series of sections derived from the first hemisphere served as a sort of topographical guide for the location of anything that might be found in the second.



I found that the giant-cells are not confined to localized areas as Betz claims. I find that they are not as numerous near the sulcus cruciatus as they are much posterior to that region. I have even found giant-cells not very far from the base of the brain, but I found the largest group of the largest cells in a place not yet indicated on any of the charts of the localizationists as an unquestionable motor point. In the accompanying woodcut the position of the nest of cells that I have discovered is accurately demonstrated. These cells are ovoid, circular, and sub-pyramidal in shape, and possess a round nucleolated nucleus situated about the centre of the cell. Each cell has from two to six visible processes. The ovoid cells are much the larger, their long diameter measuring from 0.08 mm. to 0.12 mm.; and their short diameter from 0.05 mm. to 0.06 mm. The circular and subpyramidal cells measure from 0.07 mm. to 0.08 mm. in diameter. The nuclei of all the varieties are of the same size, and measure 0.03 mm. in diameter. I only succeeded in finding them in one locality, but found them very numerous in that area. They are situated in the first primary arched gyrus, between the Sylvian and anterior Sylvian fissures. Ferrier, in his "Functions of the Brain" indicates a "centre" on the frontal division of the fourth external convolution, where, he says, he has observed, on irritating this centre, "a divergence of the lips so as to partially open the mouth." This centre approaches nearer in position to the one I have discovered than any other. With this study I was enabled to locate the chief foci for condensation of the giant-cells, of the shape known to Betz and Mierzejewski. These are pyramidal in shape, with a central round nucleus, and measure from 0.09 mm. to 0.12 mm. in length, and from 0.03 mm. to 0.04 mm. in width at the base. Their nuclei measure 0.02 mm. in diameter. The following woodcut shows how two of Betz's largest cells can be placed so that their conjoined areas are only equal to the areas of one of the ovoid cells such as I have described.

I regard the term "area of large cells" as inaccurate. The large cells are scattered more or less widely over the brain-surface, and it would be better to speak of "foci" when they are concentrated in larger numbers than elsewhere.

The giant-cell of Betz is not a new discovery. It is not a thing by itself distinct from the other pyramidal cells of the cortex. On the contrary, both in the human cor-



tex and in the cat, every transition from the average-sized cell of the third frontal layer to the giant-cell can be traced. I would also call attention to the fact that Betz states "these cells to be in nests" and not uniformly distributed in one layer, but I have seen, in one section from the paracentral lobule of the human brain, giant-cells arranged in regular order like soldiers on parade, for a distance of one-third of an inch.

Taking the deductions, which have been based upon the existence of these cells, on their merits, we find that those who have relied on this demonstration for the support of the theory of motor centres, are reduced to a number of predicaments. 1. That the largest giant-cells have been found in the brain of carnivora where no motor centre has been clearly demonstrated, and near which only small muscles are supposed to receive their cortical innervation. 2. That if, after all, this is a motor centre, that the method of localization was incompetent to detect it. I have limited myself this evening to this single fact. I need not say that the giant-cell was known to Meynert, although its locality was not accurately described by him. He claimed that the larger gyri of the frontal lobe contained the largest cells. On the other hand, cells as large as the giant-cells can be seen through the entire occipital lobe, according to this observer, in

the two white strata, and were described by him by the name of "solitary cells." I trust at no distant date to review the entire question of the distribution of large cortical cells with measurements and to submit them to the society.

For the present I think the existence of the large cortical cell group which I have described, shows conclusively that before the existence of large cells can be considered a demonstration of the correctness of functional localization, a more extended study must be made.

THE UNITY OF NATURE.

By the Duke of Argyll.

VII.

ON THE MORAL CHARACTER OF MAN CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(Continued).

It may be well, before proceeding farther in this branch of our inquiry, to retrace for a little the path we have been following, and to identify the conclusions to which we have been led.

In the first place, we have seen that the sense of obligation considered in itself—that is to say, considered apart from the particular actions to which it is attached—is a simple and elementary conception of the mind, insomuch that in every attempt to analyze it, or to explain its origin and growth, this absurdity can always be detected,—that the analysis or explanation universally assumes the previous existence of that very conception for which it professes to account.

In the second place, we have seen that, just as Reason, or the logical faculty, begins its work with the direct perception of some simple and elementary truths, of which no other account can be given than that they are intuitively perceived, or, in other words, that they are what is called "self-evident," so in like manner the Moral Sense begins its work with certain elementary perceptions and feelings in respect to conduct, which arise out of the very nature of things, and come instinctively to all men. The earliest of these feelings is the obligation of obedience to that first Authority the rightfulness of which over us is not a question but a fact. The next of these feelings is the obligation of acting towards other men as we know we should like them to act towards ourselves. The first of these feelings of obligation is inseparably associated with the fact that all men are born helpless, absolutely dependent and subject to Parents. The second of these feelings of obligation is similarly founded on our conscious community of nature with other men, and on the consequent universal applicability to them of our own estimates of good and evil.

In the third place, we have seen that this association of the higher powers of Man with rudimentary data which are supplied by the facts of Nature, is in perfect harmony with that condition of things which prevails throughout Creation,—the condition, namely, that every creature is provided from the first with just so much of instinct and of impulse as is requisite to propel and guide it in the kind and to the measure of development of which its organism is susceptible, leading it with unfailing regularity to the fulfillment of the law of its own being, and to the successful discharge of the functions assigned to it in the world.

In the fourth place, we have seen that the only really exceptional fact connected with Man is—not that he has faculties of a much higher kind than other creatures, nor that these faculties are susceptible of a corresponding kind and measure of development—but that in Man alone this development has a persistent tendency to take a wrong direction, leading not towards, but away from, the perfecting of his powers.

In the last place, we have seen that as a matter of fact, and as a result of this tendency, a very large portion of Mankind, embracing almost all the savage races, and large numbers of men among the most civilized communities, are a prey to habits, practices, and dispositions which are monstrous and unnatural—one test of this unnatural character being that nothing analogous is to be found among the lower animals in those spheres of impulse and of action in which they have a common nature with our own; and another test being that these practices, habits, and dispositions are always directly injurious and often even fatal to the race. Forbidden thus and denounced by the highest of all authorities, which is the authority of Natural Law, these habits and practices stand before us as unquestionable exceptions to the unity of Nature, and as conspicuous violations of the general harmony of Creation.

When, however, we have come to see that such is really the character of these results, we cannot be satisfied with the mere recognition of their existence as a fact. We seek an explanation and a cause. We seek for this, moreover, in a very different sense from that in which we seek for an explanation and a cause of those facts which have the opposite character of being according to law and in harmony with the analogies of Nature.

With facts of this last kind, when we have found the place into which they fit in the order of things, we can and we do rest satisfied as facts which are really ultimate—that is to say, as facts for which no other explanation is required than that they are part of the Order of Nature, and are due to that one great cause, or to that combination of causes, from which the whole harmony and unity of Nature is derived. But when we are dealing with facts which cannot be brought within this category,—which cannot be referred to this Order, but which are, on the contrary, an evident departure from it,—then we must feel that these facts require an explanation and a cause as special and exceptional as the results themselves.

There is, indeed, one theory in respect to those mysterious aberrations of the human character, which, although widely prevalent, can only be accepted as an explanation by those who fail to see in what the real difficulty consists. That theory is, that the vicious and destructive habits and tendencies prevailing among men, are not aberrant phenomena at all, but are original conditions of our nature, —that the very worst of them have been primitive and universal, so that the lowest forms of savage life are the nearest representatives of the primordial condition of the race.

Now, assuming for the present that this were true, it would follow that the anomaly and exception which Man presents among the unities of Nature is much more violent and more profound than on any other supposition. For it would represent the contrast between his instincts and those of the lower animals as greatest and widest at the very moment when he first appeared among the creatures which, in respect to these instincts, are so superior to himself. And it is to be observed that this argument applies equally to every conceivable theory or belief as to the origin of Man. It is equally true whether he was a special creation, or an unusual birth, or the result of a long series of unusual births each marked by some new accession to the aggregate of faculties which distinguish him from the lower animals. As regards the anomaly he presents, it matters not which of these theories of his origin be held. If his birth, or his creation, or his development, whatever its methods may have been, took place after the analogy of the lower animals, then, along with his higher powers of mind, there would have been corresponding instincts associated with them to guide and direct those powers in their proper use. It is in this essential condition of all created things that Man, especially in his savage state, presents an absolute contrast with the brutes. It is no explanation, but, on the contrary, an insuperable increase of the difficulty,

to suppose that this contrast was widest and most absolute when Man made his first appearance in the world. It would be to assume that, for a most special and most exceptional result, there was no special or exceptional cause. If Man was, indeed, born with an innate propensity to maltreat his women, to murder his children, to kill and eat his fellow, to turn the physical functions of his nature into uses which are destructive to his race, then, indeed, it would be literally true that

" Dragons of the prime,
That tare each other in their slime,
Were mellow music matched with him."

It would be true, because there were no Dragons of the prime, even as there are no reptiles of the present age —there is no creature, however terrible or loathsome its aspect may be to us, among all the myriads of created things—which does not pass through all the stages of its development with perfect accuracy to the end, or which, having reached that end, fails to exhibit a corresponding harmony between its propensities and its powers, or between both of these and the functions it has to perform in the economy of Creation. So absolute and so perfect is this harmony, that men have dreamed that somehow it is self-caused, the need and the requirement of a given function producing its appropriate organ, and the organ again reacting on the requirement and the need. Whatever may be the confusion of thought involved in this idea, it is at least an emphatic testimony to the fact of an order and an adjustment of the most perfect kind prevailing in the work of what is called Evolution, and suggesting some cause which is of necessary and universal operation. The nearer, therefore, we may suppose the origin of Man may have been to the origin of the brutes, the nearer also would his condition have been to the fulfillment of a law which is of universal application among them. Under the fulfillment of that law the higher gifts and powers with which Man is endowed would have run smoothly their appointed course, would have unfolded as a bud unfolds to flower,—as a flower ripens into fruit,—and would have presented results absolutely different from those which are actually presented either by the savage or by what is called the civilized condition of Mankind.

And here it may be well to define, as clearly as we can, what we mean by civilization, because the word is very loosely used, and because the conceptions it involves are necessarily complex. Usually it is associated in our minds with all that is highest in the social, moral, and political condition of the Christian nations as represented in our own country and in our own time. Thus, for example, respect for human life, and tenderness towards every form of human suffering, is one of the most marked features of the best modern culture. But we know that this sentiment, and many others which are related to it, were comparatively feeble in the case of other societies which, nevertheless, we acknowledge to have been very highly civilized. We must, therefore, attach some more definite and restricted meaning to the word, and we must agree to understand by civilization only those characteristic conditions which have been common to all peoples whom we have been accustomed to recognize as among the governing nations of the world. And when we come to consider what these characteristics are, we find that though complex, they are yet capable of being brought within a tolerably clear and simple definition. The Latin word *civis*, from which our word civilization comes, still represents the fundamental conception which is involved. The citizen of an imperial City,—the subject of an imperial Ruler,—the member of a great State,—this was the condition which constituted the Roman idea of the rank and status of civilization. No doubt many things are involved in this condition, and many other things have come to be associated with it. But the essential elements of the civilized condition, as thus defined or understood, can readily be separated from others which are not essential. An extended knowledge of the useful arts, and the possession of such a settled system of law and government as enables men to live in great political communities, these are the essential features of what we understand by civilization. Other characteristics may co-exist with these, but nothing more is necessarily involved in a proper understanding, or even in the usual application of the word. In particular, we cannnot affirm that a civilized condition involves necessarily any of the higher moral elements of character. It is true, indeed, that no great State, nor even any great City, can have been founded and built up without courage and patriotism. Accordingly these were perhaps the most esteemed virtues of antiquity. But these are by no means confined to civilized men, and are, indeed, often conspicuous in the savage and in the barbarian. Courage, in at least its lower forms, is one of the commonest of all qualities; and patriotism, under the like limitation, may almost be said to be an universal passion. It is in itself simply a natural consequence of the social instinct; common to Man and to many of the lower animals—that instinct which leads us to identify our own passions and our own sympathies with any brotherhood to which we may belong,—whatever the associating tie of that brotherhood may be,—whether it be morally good, bad, or indifferent. Like every other instinct, it rises on its moral character in proportion as it is guided by reason and by conscience, and in proportion as, through these, it becomes identified with duty and with self-devotion. But the idea of civilization is in itself separate from the idea of virtue. Men of great refinement of manners may be, and often are, exceedingly corrupt. And what is true of individuals is true of communities. The highest civilizations of the heathen world were marked by a very low code of morals, and by a practice even lower than their code. But the intellect was thoroughly cultivated. Knowledge of the useful arts, taste in the fine arts, and elaborate systems both of civil polity and of military organization, combined to make, first Greek, and then Roman, civilization, in such matters the basis of our own.

It is, therefore, only necessary to consider for a moment these essential characteristics of what we mean by civilization, to see that it is a conception altogether incongruous with any possible idea we can form of the condition of our first parents, or, indeed, of their offspring for many generations. An extended knowledge of the useful arts is of necessity the result of accumulation. Highly organized systems of polity were both needless and impossible before settled and populous communities had arisen. Government was a simple matter when the "world's gray fathers" exercised over their own children the first and the most indisputable of all authorities.

It is unfortunate that the two words which are habitually used to indicate the condition opposite to that of civilization are words both of which have come to mean a great deal more than mere ignorance of the useful arts, or a merely rudimentary state of law and government. Those two words are barbarism and savagery. Each of these has come to be associated with the idea of special vices of character and of habit, such as cruelty and ferocity. But "barbarian," in the classical language from which it came to us, had no such meaning. It was applied indiscriminately by the Greeks to all nations, and to all conditions of society other than their own, and did not necessarily imply any fault or failure other than that of not belonging to the race, and not partaking of the culture which was then, in many respects at least, the highest in the world. St. Paul refers to all men who spoke in any tongue unknown to the Christian communities as men who were "to them barbarians." But he did not associate this term with any moral faults, such as violence or ferocity; on the contrary, in his narrative of his shipwreck on the coast of Malta, he calls the

natives of that island "barbarous people" in the same sentence in which he tells us of their kindness and hospitality. This simple and purely negative meaning of the word barbarian has been lost to us, and it has become inseparably associated with characteristics which are indeed common among uncivilized nations, but are by no means confined to them. The epithet "savage," of course, still more distinctly means something quite different from rude, or primitive or uncultivated. The element of cruelty or of ferocity is invariably present to the mind where we speak of savagery, although there are some races—as for example, the Eskimo—who are totally uncivilized, but who, in this sense, are by no means savage.

And this may well remind us that, as we have found it necessary to define to ourselves the condition which we are to understand by the word civilization, so it is not less essential to define and limit the times to which we are to apply the word primeval. For this word also is habitually used with even greater laxity of meaning. It is often employed as synonymous with primitive, and this again is applied not only to all times which are prehistoric, but all conditions even in our own age which are rude or savage. There is an assumption that,the farther we go back in time, there was not only less and less extensive knowledge of the useful arts,—not only simpler and simpler systems of life and polity,—but also that there were deeper and deeper depths of the special characteristics of the modern savage. We have, however, only to consider what some of these characteristics are, to be convinced that although they may have arisen in early times, they cannot possibly have existed in the times which were the earliest of all. Things may have been done, and habits may have prevailed, when the multiplication and dispersion of Mankind had proceeded to a considerable extent, which cannot possibly have been done, and which cannot possibly have prevailed when as yet there was only a single pair of beings "worthy to be called" man and woman, nor even when as yet all the children of that pair knew themselves to be of one family and blood. The word primeval ought, if it is to have any definite meaning at all, to be confined to this earliest time alone. It has already been pointed out, that on the supposition that the condition of primeval man approximated to the condition of the lower animals, that condition could not have been nearer to, but must, on the contrary, have been very much farther removed from the condition of the modern savage. If, for example, there ever was a time when there existed on one spot of earth, or even on more spots than one, a single pair of human beings, it is impossible that they should have murdered their offspring, or that they should have killed and eaten each other. Accordingly it is admitted that cannibalism and infanticide, two of the commonest practices of savage and of barbarous life, cannot have been primeval. But this is a conclusion of immense significance. It hints to us, if it does no more, that what is true of one savage practice may possibly be true of others.

(To be Continued.)

ASTRONOMY.

COMPARISON STARS:—Under this heading Mr. Dreyer, in the last number of *Urania*, makes a most excellent "Suggestion to Astronomers" upon a matter which, of late, attracted some little attention. It is to be hoped that other observers will follow the example set at the Dunsink Observatory. Mr. Dreyer's "suggestion" is as follows:

"In spite of the numerous star-catalogues in the hands of observers of minor planets and comets, it frequently happens that a well-determined place for a comparison star cannot be found in any catalogue. Many stars have therefore to be re-observed, and much time is no doubt lost by a number of observers, each having to determine the places of a few stars, which, if put together in one working list could be observed by one person with but little trouble.

It would evidently be an advantage if an astronomer, having at his disposal a good transit circle, would, for a time, endeavor to determine the places of all the comparison stars recently used and requiring re-observation.

In accordance with this scheme, I shall, until further notice (with the concurrence of Dr. Ball) be glad to determine with the Dunsink Transit Circle the places of any comparison stars north of —20° Declination not found in modern catalogues, and recently used in observations of minor planets or comets. The mean places, based on the Fundamental Catalogue of the 'Astronomische Gesellschaft,' will be worked out and published as soon as practicable."

THE SOLAR PARALLAX.

M. Faye has recently communicated to the Paris Academy of Sciences (*Comptes Rendus Tome XCII., No. 8*), an interesting paper upon the actual state of our knowledge of the sun's parallax. Remarking that there is no other constant in science whose determination depends upon such a large number of entirely independent results, he subdivides the various values assigned for the sun's mean parallax, as follows:

Geometrical Methods 8.82"	8.85"	by Mars (Cassini's method)	Newcomb
	8.78	by Venus, 1769 (Halley's method)	Powalky
	8.81	by Venus, 1874 "	Tupman
	8.87	by Flora, (Galle's method)	Galle
	8.79	by Juno "	Lindsay
Mechanical Methods 8.83"	8.81	by the lunar inequality (Laplace's method)	———
	8.85	by the monthly equation of the earth	Leverrier
	8.83	by the perturberations of Mars and Venus	Leverrier
Physical Methods 8.81"	8.799	velocity of light (Fizeau's method)	Cornu
	8.813	" " (Foucault's method)	Michelson

In regard to the value 8.85" obtained by Cassini's method, M. Faye says that Mars has always given values for the solar parallax somewhat too large. The first value 8."81 obtained by mechanical methods was calculated by adopting for the coefficient of the inequality 125.2", the mean between the result of Airy from the Greenwich observations, and that of Newcomb from the Washington observations, taking for the moon's parallax 57′ 2.7", and for her mass $\frac{1}{80.8}$. By the second of the "mechanical methods" Leverrier found 8."95, which was afterwards reduced to 8.85" by Stone upon correcting two slight errors in the computation. The value from the perturbations of Venus and Mars, assigned by Leverrier was 8.86", but one of the numbers requiring a small correction, it is reduced to 8.83". Michelson having overcome all the difficulties in Foucault's method, found for the velocity of light 2,999.40 kilom. ± 100 kilom. Using Struve's constant of aberration the corresponding values of the parallax are 8.799" and 8.813", as above. The general mean is 8.82", to which M. Faye attributes a probable error of ± 0.016". Although each of the values may be effected by systematic error, nevertheless, since the causes of error are varied, and without the least possible connection, these errors must be to a great degree eliminated, as well as the accidental errors.

The following conclusions are reached:

1. That the physical methods are superior to all others, and should be adopted.

2. That the value of the solar parallax, 8.813" (by physical methods), is now determined to about $\frac{1}{100}$ of a second.

3. That the seven astronomical methods converge more and more towards that value, and tend to confirm it, without equalling it in precision.

This fact does not diminish, however, the great importance of observations upon the coming transit of Venus, to which we can now bring to our aid the most effective of photographic apparatus. W. C. W.

WASHINGTON, D. C., April 14, 1881.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

PRIMITIVE STAGES OF COSMICAL EVOLUTION.

Some correspondence which has recently appeared in your columns seems to render it proper to offer a brief synopsis of my conception of the primitive modes of existence of cosmical matter. Supposing luminous cosmical matter to be intensely heated, there is no good reason for assuming this to be absolutely a first condition. It is undoubtedly a remote and primitive condition, which may be assumed as a stage from which cosmical development proceeds. But, in the light of recent science, we may reasonably seek for earlier stages at which cosmical matter existed in a cold and non-luminous state. In these stages we may conceive it either as atomically subdivided and dissociated, or as partly in a condition of molar aggregation, and perhaps of chemical combination. I am inclined to think, with Laplace and Kant, that space is abundantly stocked with cosmical matter in a crude and unformed state. I see no ground for assigning any limit to the state of subdivision in which some of it may exist. I see no satisfactory ground for assuming as Macvicar and others have done, that it emerges from an ethereal condition, nor for denying it. I do not imagine it to possess a temperature different from that of the space in which it floats—if, indeed space, which is not conditioned by matter, can be said to possess a property, which we only know as an affecton of matter.

But there is a principle of gravitation in the universe, however we may explain it, which is ever beginning the aggregation of the ultimate elements of cosmical matter, and ever uniting these aggregations into masses larger and larger. We have made the acquaintance of some of these masses, large and small, in meteoric stones and millions of meteoroids, which become kindled only by friction with our atmosphere. It has been shown that several meteoroidal trains identify themselves with well-known cometary existences. It has been suggested that a rain of cosmical matter has pelted the minute satellites of Mars until the angular orbital motion of the inner one exceeds the axial motion of the planet. The collisions of molecules and masses must result in the evolution of heat. This, within certain limits of temperature, would promote the chemical union of atoms previously dissociated. But the impact of larger masses and groups of masses and molecules would undoubtedly develop sufficient heat to vaporize matter, and even to reproduce a state of dissociation. Such events would characterize the history of any vast region of space in which cosmical atoms should have become relatively approximated. They are in process of falling together. This is the condensation of the cosmical swarm. It may proceed, as Sir William Herschel conceived, until the successive stages of nebular condensation have been passed, when the swarm arrives at the condition of a sun. But it is well-nigh impossible for two cosmical masses to come together without producing rotation. The constituent portions of a nebula must probably rotate, and the nebula as a whole must, in most cases, exist in a state of rotation. Such a nebula was the starting point of Laplace, and was recognized as an actual contingency by Sir William Herschel.

But I do not conceive a fully formed and characteristic nebula as necessarily in a state of complete gaseity. The perpetual collision of hard parts might vaporize sufficient matter to occupy the intervening spaces and afford the characteristic spectral results.

As long as the nebula remains in process of condensation through central gravitation, the evolution of heat must continue. But there is a juncture at which rise of temperature must cease. That is reached when the elastic forces equilibrate the attractive. From this point, condensation developes heat, but some more is lost than is acquired, and the mass continually subsides in temperature. It is only the loss of an excess of heat which now permits the progressing condensation. Cooling is the limiting condition of condensation; and to assert a rising or a constant temperature is to assume that a condition may be surpassed by the effect of that which it conditions and measures. It is a virtual denial of the conservation of energy and of the equation of cause and effect.

In this view of cosmical beginnings, nebular heat is preceded by motion, and the cause of motion is what we call gravity. If, as Le Sage maintained, gravity is only the effect of the impact of a storm of ultramundane corpuscles, then our explanation ends at last in motion for which we cannot invoke gravity as an ultimate physical cause. Metaphysically speaking, such must be the issue of all explanations. There is a necessary *ultima thule* in the realm of thought. We are no nearer an absolute explanation at one stage of cosmical development than at any other The remotest term reached must always stand scientifically unexplained.

I began by supposing luminous nebular matter intensely heated. There are many indications that such is its condition. The bright spectral lines and the analogy of the envelopes of the solar and stellar bodies are strongly suggestive. The rational continuity of cosmical development, leading our thoughts backward from an incrusted world through all conceivable stages, to an incandescent vapor or possibly gas, enforces the conviction of high nebular temperature. But, on the contrary, the very limited number of nebular bright lines spectroscopically revealed proclaims a fundamental condition widely different from that in the sun and fixed stars. It might be suggested that this indicates not only elemental dissociation, but an ulterior resolution, as Lockyer maintains, into one or two sole sorts of world-stuff; but it may also be suggested that the phenomenon is so divergent from the results of any terrestrial verifications that we are left without any substantial ground for inference. The nuclei of comets give also a few bright spectral lines. When the comet is near its perihelion it is not difficult to admit, in some cases, that the spectrum reveals a volatilized condition; but when a body so tenuous as to transmit star-light has retired from perihelion, it is difficult to believe that a gaseous condition is still the effect of high incandescence. The identification of cometary and cold meteoroidal trains, if it has truly been done, throws doubt on the assumed heat of even the nuclear portions of a comet remote from the sun; and yet even here it is supposable that perpetual collision of cold hard parts disengages sufficient heat to create a common gaseous medium. Finally, the meteoric streak of light left sometimes fifteen to thirty minutes after the fall and dissipation of the meteor, cannot be a case of heated luminosity. Heat vaporizes the meteoroid; but it is then a train of minutely divided particles exposed to almost instantaneous refrigeration. A few grains of matter strewn along a path twenty miles in extent, in the cold atmosphere, cannot retain luminosity as a consequence of high temperature. We might cite the streamers of the *aurora borealis*, and the Geisslerian discharges, and Crooke's radiant matter, and the general phenomena of phosphorescence as further reminders that intense heat is not the only cause of luminosity, and suggestions that nebular light *may* not be exclusively the light of thermal incandescence.

ALEXANDER WINCHELL.

UNIVERSITY OF MICHIGAN, *April* 8, 1881.

BOOKS RECEIVED.

SIGHT: An Exposition of the Principles of Monocular and Binocular Vision, by JOSEPH LE CONTE, LL.D., Professor of Geology and Natural History in the University of California.—D. Appleton & Co. New York, 1881.

This is another of those books specially prepared to bring wiihin the reach of the general reader, some of the most interesting but complicated problems of science.

In the present case Professor Le Conte has attempted the double task of writing a work for the general public, which should also be found profitable reading for even the most advanced specialist. This would appear to be an embarrassing operation even with a popular subject, but with one so purely technical as optics the difficulties of the author must have been greatly increased.

To accomplish this programme with success, Professor Le Conte has divided his subject into two parts; the first treating, in an elementary manner, the anatomy and construction of the human eye, and the various theories involved in vision; and also a second part addressed to the specialist, in which some of the disputed points in Binocular vision are treated in detail.

Professor Le Conte, in speaking of the wonderful mechanism of the human eye, describes it as a masterpiece of Nature, whereas it is well known that many authors have been far less enthusiastic in its praise.

To show how differently two authors may treat the same subject even from the same point of view, let us compare what Professor Le Conte and Dr. H. Newell Martin says, as to the comparison of the human eye with a microscope objective, for curiously enough they both select the same object for this purpose, the former in the work now under review, and the latter in "The Human Body," a book recently received in our office, a notice of which is in preparation.

Professor Le Conte remarks, * "We see, then, that the mode of adjustment of the eye is somewhat like that of the Microscope." "Like the Microscope, but how infinitely superior."

Whereas, Dr. Martin observes: "The eye, though it answers admirably as a physiological instrument, is by no means as perfect optically; not nearly so good, for example, as a good Microscope objective." † Again Professor Le Conte, while indeed speaking of "defects of the eye as an instrument," refers such imperfections to a condition of disease or malformation, or at least to eyes which are not "normal or perfect," and specially mentions Myopy or Brachymetropy, Presbyopy, Hypermetropy and Astigmatism. Whereas, Dr. Martin points out defects of the human eye, which appear to be the normal condition of human vision, and exist "even in the best of eyes"—such as the "reflecting surfaces of our eyes not even being truly spherical, especially in the case of the cornea," and that "few persons are able to see equally clearly, at once, two lines crossing one another at right angles," this defect, when existing in a marked degree, causing serious trouble, amounting to disease, and known as "*astigmatism*."

In regard to the first part of the work, we may state that rarely has the subject been before treated in a manner so likely to realize in the mind of the student all that it is necessary to know in regard to the vision of man. The author's descriptions are so clear that to misconstrue them is impossible. Excellent illustrations are given, the majority being diagrams prepared by Professor Le Conte.

In regard to the more technical parts of the work relating to Binocular Vision, we find a difficulty in giving a resumé without diagrams, which it is not possible to present with this notice, but we trust we have aroused sufficient interest in this book to induce those studying the question to make direct use of Professor Le Conte's work, where the whole subject is explained.

Professor Le Conte's own views, however, on Binocular vision may be given, which are expressed in the following words:

"All objects or points of objects, either beyond or nearer than the point of sight, are doubled but *differently*. The former homonymously, the latter heteronymously. The double images in the former case are united by *less* convergence, in the latter case by *greater* convergence of the optic axes. Now the observer knows *instinctively and without trial*, in any case of double images, whether they will be united by greater or less optic convergence, and therefore never makes a mistake, or attempts to unite by making a wrong movement of the optic axes. In other words, *the eye (or the mind) instinctively distinguishes homonymous from heteronymous images, referring the former to objects*, successive combination of the different parts of the object or scene, or pictures, as maintained by Brücke." ‡

Professor Le Conte claims that this work "meets a real want, and fills a real gap in scientific literature;" in this assertion we heartily concur.

* Sight, &c., page 45.

† The Human Body, page 502.

‡ Page 151.

CORRESPONDENCE.

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A PROBLEM IN OPTICS.

To the Editor of "SCIENCE:"

Will some of your correspondents answer the following questions, not from any tables given in books, but by original computation or observation. Given a plate of crown glass, index of refraction 1.525, with parallel surfaces, a ray of light incident on the under-surface 38° from the normal.

What will be the direction of the ray *in the glass*? What will be its direction of emergence from the upper surface into air? What its direction into water? What percentage of the light will be lost by reflection from the under surface of the plate? What will be lost from the upper surface?

If the light is incident 45° from the normal, what is the answer to each of the above questions?

As each color of the spectrum has a different degree of refraction the medium ray should as usual be taken for computation.

The thickness of the plate cannot affect the refraction *in* the glass, only the point where the ray reaches the upper surface. CARL REDDOTS.

OZONE is absorbed in a solution of arsenious acid to which a little neutral potassium iodide has been added. The excess of arsenious acid is estimated with standard iodine solution. 1. Ozone is a constituent of the higher atmosphere to a much larger extent than near the earth's surface. 2. Ozone is destroyed by contact with the gases and organic matter in a moist atmosphere, as near the earth's surface, and that the collection of ozone from the air is attended by the destruction of it to a large extent. 3. The absorptive power of the ozone in the air is quite sufficient to account for the limitation of the solar spectrum. 4. The blue tint of the atmosphere is largely due to ozone.—W. N. HARTLEY.

ERRATA.

How to obtain the Brain of the Cat, (Wilder).—Correction: Page 158, second column, line 7, "grains," should be "grams;" page 159, near middle of 2nd column, "succesily," should be "successively;" page 161, the number of Flower's paper is 3.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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The growth of abstract science in this country is perhaps no better illustrated than by the advance which has been made of late years in the various departments of mathematics. It is only a few years since Prof. Peirce was about the only person in the United States who held a position among the original mathematicians of the world, while to-day there are in this country a number of persons whose writings are destined to rank among the classics, and a journal of mathematics of the highest rank is published under the auspices of the Johns Hopkins University and sustained almost entirely by American contributors. Among the best of the abstract writers referred to is Mr. William Ferrel, who has been hitherto best known by his tidal researches, but is now engaged in investigations on the mathematical principles of meteorology. His latest work, just published by the Coast Survey, is now before us, and although nominally consisting only of researches on Cyclones, Waterspouts and Tornadoes, is in reality a valuable contribution to the theory of storms in general.

The Board of Directors of the Ohio Mechanics' Institute have organized a "*Department of Science and Arts*" for the purpose of increasing the usefulness of the Institution. A Section of Mechanics and Engineering under the chairmanship of Professor H. T. Eddy, and one of Chemistry under Professor F. W. Clark, have been arranged. Meetings for the public discussion of scientific subjects will be held once a month, and various other arrangements are in progress which will contribute to the success of the present attempt to provide increased facilities for technical and scientific education for the youths of Cincinnati.

SCIENTIFIC SOCIETIES OF WASHINGTON.

The Biological Society, Washington.—Since our last report the following papers have been read: "Roan Mountain, North Carolina, and its Flora," by Prof. J. W. Chickering, Jr.; "Notes on the Flowering of *Solanum rostratum* and Cassia chamaecrista, with illustrations," by Prof. J. E. Todd; "A Critical Review of Günther's Ichthyology," by Prof. Theodore Gill; "On the Mortality of Marine Animals in the Gulf of Mexico," by Mr. Ernst Ingersoll: "A Statistical View of the Flora of the District of Columbia," by Prof. Lester F. Ward. It is to be regretted that the absence of our Washington correspondent from the meetings deprives us of abstracts of these valuable papers.

The Anthropological Society.—The Constitution of this society, now in its third year, makes it obligatory upon the President to prepare at the commencement of each year, a summary of the transactions of the organization during the past year. At the close of the first year, the President overlooked this fact, but made ample amends at the commencement of the third year by preparing a pamphlet containing both annual addresses, and copious abstracts of all the papers that had ever been read.

Since our last report, the following papers have been read: "The Savage Mind in the Presence of Civilization," by Prof. Otis T. Mason; "Prehistoric Trephining," by Dr. Robert Fletcher; "Some Superstitions of the Sioux Indians," by Dr. H. Yaddow; "The Chief's Son and the Thunders: An Omaha Myth," by Rev. J. Owen Dorsey.

The design of the first named paper was two-fold: first, to show that the presence of other peoples better furnished and skilled in some respect had always operated as a stimulus in the onward march of civilization; and second to draw attention to the fact that in the treatment of the Indians, Chinese, and Negroes, the phenomena of the past history of civilization were being re-presented. The two latter papers were recitals of exceedingly interesting Indian myths. Dr. Fletcher, who is associated with Dr. Billings in publishing "Index Medicus," having collected all that could be gathered on the subject of prehistoric-trephining, from two years reading, gave an elaborate summary of his investigations.

The Philosophical Society of Washington.—The Spectrophone.—At the 198th meeting of the Philosophical Society of Washington, Prof. Alexander Graham Bell communicated the announcement of his discovery of the Spectrophone, the latest outgrowth of the Photophone.

In a paper read before the American Association for the Advancement of Science, in which he announced the discovery of the photophone, Mr. Bell ventured the prediction that probably all matter would be found to posses sonorous properties of the same nature as those manifested by the discs used in that instrument. More recent investigations in Europe with gases and liquids have fully verified this prediction. Any liquid or gas placed in a test tube and exposed to the action of a beam of light condensed upon it by a lens can be made, by means of an interrupter, to emit musical tones. This has been shown by Prof. Tyndall in his memoir, to the Royal Society, on Radiant Heat. Some substances thus emit feeble sounds, others stronger ones. Iodine vapor, Nitrogen Oxide and Bromine give very loud sounds. It is found that those substances which emit loud sounds are those which absorb heat in a high degree, and among these lamp-black is especially remarkable. It has been questioned whether such sounds are provoked by the luminous rays or by the dark ones. M. Mercadier expressed the belief that the inciting rays are the red and dark ones. This led Mr. Bell, with the assistance of Mr. Sumner Taintor, to experiment with the sonorous properties of Carbon Disulphide, actuated by the light of the Spectrum.

When lamp-black is exposed to the action of the light of the spectrum it is found to give a sonorous response to all of its rays as far as the middle of the violet, and perhaps beyond. The intensity of the sound, however, varies remarkably in different parts of the spectrum. Taking the rays successively from different parts, from the violet towards the red, the sounds begin very feebly and increase in intensity, reaching a maximum in the ultra red. Beyond that point they suddenly cease. The increase of intensity is very gradual, the decrease very sudden.

Other substances have been experimented with, and while exhibiting similar properties, each has a range of its own. Porous and fibrous substances give loud sounds. Thus, common wool or worsted are found to be very sonorous, but the sounds are obtained wholly from the visible parts of the spectrum and have the maximum intensity in the green. In all substances tried success has resulted, but nearly all have a very short range.

In experimenting with more homogeneous substances of simpler constitution, still more definite results are obtained. The rays of the spectrum are passed through sulphuric ether. Outside of the ultra red is a very narrow band which cause sounds while the other parts fail to produce them. Hydrogen peroxide gives sounds at several places wholly within the visible parts of the spectrum, and these places are found to coincide with the positions of the known absorption bands of that substance. The same is found to be true of Nitrogen Oxide and a solution of Ammonia, Sulphate of Copper, and many other substances. The general law deduced is that *sounds are produced in any substance by the rays which it absorbs.*

Thus a kind of spectrum analysis can be obtained through the intermediation of sound. The principal value of the spectrophone, Mr. Bell believes, will be found in the investigation of absorption bands in the ultra red part of the spectrum.

Mr. William B. Taylor inquired whether the sounds observed from the two absorption bands of ammonia, sulphate of copper, were octaves. Mr. Bell replied that this question had not as yet been investigated.

Mr. G. Brown Goode read portions of a paper on the sword-fish and its allies, which paper will be published in full in the next annual report of the U. S. Fish Commission.

ON THE MODERN DEVELOPMENT OF FARADAY'S CONCEPTION OF ELECTRICITY.*

By Professor Helmholtz.

The majority of Faraday's own researches were connected, directly or indirectly, with questions regarding the nature of electricity, and his most important and most renowned discoveries lay in this field. The facts which he has found are universally known. Nevertheless, the fundamental conceptions by which Faraday has been led to these much-admired discoveries have not been received with much consideration. His principal aim was to express in his new conceptions only facts, with the least possible use of hypothetical substances and forces. This was really a progress in general scientific method, destined to purify science from the last remnants of metaphysics. Now that the mathematical interpretations of Faraday's conceptions regarding the nature of electric and magnetic force has been given by Clerk Maxwell, we see how great a degree of exactness and precision was really hidden behind his words, which to his contemporaries appeared so vague or obscure; and it is astonishing in the highest to see what a large number of general theories the methodical deduction of which requires the highest powers of mathematical analysis, he has found by a kind of intuition, with the security of instinct, without the help of a single mathematical formula.

The electrical researches of Faraday, although embracing a great number of apparently minute and disconnected questions, all of which he has treated with the same careful attention and conscientiousness, are really always aiming at two fundamental problems of natural philosophy, the one more regarding the nature of physical forces, or of forces working at a distance; the other, in the same way, regarding chemical forces, or those which act from molecule to molecule, and the relation between these and the first.

The great fundamental problem which Faraday called up anew for discussion was the existence of forces working directly at a distance without any intervening medium. During the last and the beginning of the present century the model after the likeness of which nearly all physical theories had been formed was the force of gravitation acting between the sun, the planets, and their satellites. It is known how, with much caution and even reluctance, Sir Isaac Newton himself proposed his grand hypothesis, which was destined to become the first great and imposing example, illustrating the power of true scientific method.

But then came Oerstedt's discovery of the motions of magnets under the influence of electric currents. The force acting in these phenemena had a new and very singular character. It seemed as if it would drive a single isolated pole of a magnet in a circle around the wire conducting the current, on and on without end, never coming to rest. Faraday saw that a motion of this kind could not be produeed by any force of attraction or repulsion, working from point to point. If the current is able to increase the velocity of the magnet, the magnet must react on the current. So he made the experiment, and discovered induced currents; he traced them out through all the various conditions under which they ought to appear. He concluded that somewhere in a part of the space traversed by magnetic force there exists a peculiar state of tension, and that every change of this tension produces electromotive force. This unknown hypothetical state he called provisionally the electrotonic state, and he was occupied for years and years in finding out what was this electrotonic state. He discovered at first, in 1838, the dielectric polarisation of electric insulators, subject to electric forces. Such bodies show, under the influence of electric forces, phenomena perfectly analogous to those exhibited by soft iron under the influence of the magnetic force. Eleven years later, in 1849, he was able to demonstrate that all ponderable matter is magnetized under the influence of sufficiently intense magnetic force, and at the same time he discovered the phenomena of diamagnetism, which indicated that even space, devoid of all ponderable matter, is magnetizable; and now with quite a wonderful sagacity and intellectual precision Faraday performed in his brain the work of a great mathematician without using a single mathematical formula. He saw with his mind's eye that by these systems of tensions and pressures produced by the dielectric and magnetic polarisation of space which surrounds electrified bodies, magnets or wires conducting electric currents, all the phenomena of electro-static, magnetic, eléctro-magnetic attraction, repulsion, and induction could be explained, without referring at all to forces acting directly at a distance. This was the part of his path where so few could follow him; perhaps a Clerk Maxwell, a second man of the same power and independence of intellect, was necessary to reconstruct in the normal methods of science the great building, the plan of which Faraday had conceived in his mind and attempted to make visible to his contemporaries.

Nevertheless the adherents of direct action at a distance have not yet ceased to search for solutions of the

* The Faraday Lecture, delivered before the Fellows of the Chemical Society in the Theatre of the Royal Institution, on Tuesday, April 5th, 1881, by Professor Helmholtz. Abstract revised by the author.

electro-magnetic problem. The present development of science, however, shows, as I think, a state of things very favorable to the hope that Faraday's fundamental conceptions may in the immediate future receive general assent. His theory, indeed, is the only existing one which is at the same time in perfect harmony with the facts observed, and which at least does not lead into any contradiction against the general axioms of dynamics.

It is not at all necessary to accept any definite opinion about the ultimate nature of the agent which we call electricity.

Faraday himself avoided as much as he could giving any affirmative assertion regarding this problem, although he did not conceal his disinclination to believe in the existence of two opposite electric fluids.

For our own discussion of the electro-chemical phenomena, to which we shall turn now, I beg permission to use the language of the old dualistic theory, because we shall have to speak principally on relations of quantity.

I now turn to the second fundamental problem aimed at by Faraday, the connection between electric and chemical force. Already, before Faraday went to work, an elaborate electro-chemical theory had been established by the renowned Swedish chemist, Berzelius, which formed the connecting-link of the great work of his life, the systematisation of the chemical knowledge of his time. His starting point was the series into which Volta had arranged the metals according to the electric tension which they exhibit after contact with each other, a fundamental point which Faraday's experiment contradicted with the supposition that the quantity of electricity collected in each atom was dependent on their mutual electro-chemical differences, which he considered as the cause of their apparently greater chemical affinity. But although the fundamental conceptions of Berzelius's theory have been forsaken, chemists have not ceased to speak of positive and negative constituents of a compound body. Nobody can overlook that such a contrast of qualities, as was expressed in Berzelius's theory, really exists, well developed at the extremities, less evident in the middle terms of the series, playing an important part in all chemical actions, although often subordinated to other influences.

When Faraday began to study the phenomena of decomposition by the galvanic current, which of course were considered by Berzelius as one of the firmest supports of his theory, he put a very simple question ; the first question, indeed, which every chemist speculating about electrolysis ought to have answered. He asked, What is the quantity of electrolytic decomposition if the same quantity of electricity is sent through several electrolytic cells? By this investigation he discovered that most important law, generally known under his name, but called by him the law of definite electrolytic action.

Faraday concluded from his experiments that a definite quantity of electricity cannot pass a voltametric cell containing acidulated water between electrodes of platinum without setting free at the negative electrode a corresponding definite amount of hydrogen, and at the positive electrode the equivalent quantity of oxygen, one atom of oxygen for every pair of atoms of hydrogen. If instead of hydrogen any other element capable of substituting hydrogen is separated from the electrolyte, this is done also in a quantity exactly equivalent to the quantity of hydrogen which would have been evolved by the same electric current.

Since that time our experimental methods and our knowledge of the laws of electrical phenomena have made enormous progress, and a great many obstacles have now been removed which entangled every one of Faraday's steps, and obliged him to fight with the confused ideas and ill-applied theoretical conceptions of some of his contemporaries. We need not hesitate to say that the more experimental methods were refined, the more the exactness and generality of Faraday's law was confirmed.

In the beginning Berzelius and the adherents of Volta's original theory of galvanism, based on the effects of metallic contact, raised many objections against Faraday's law. By the combination of Nobili's astatic pairs of magnetic needles with Schweigger's multiplicator, a coil of copper wire with numerous circumvolutions, galvanometers became so delicate that the electro-chemical equivalent of the smaller currents they indicated was imperceptible for all chemical methods. With the newest galvanometers you can very well observe currents which would want to last a century before decomposing one milligram of water, the smallest quantity which is usually weighed on chemical balances. You see that if such a current lasts only some seconds or some minutes, there is not the slightest hope to discover its products of decomposition by chemical analysis. And even if it should last a long time the feeble quantities of hydrogen collected at the negative electrode can vanish, because they combine with the traces of atmospheric oxygen absorbed by the liquid. Under such conditions a feeble current may continue as long as you like without producing any visible trace of electrolysis, even not of galvanic polarisation, the appearance of which can be used as an indication of previous electrolysis. Galvanic polarisation, as you know, is an altered state of the metallic plates which have been used as electrodes during the decomposition of an electrolyte. Polarised electrodes, when connected by a galvanometer, give a current which they did not give before being polarised. By this current the plates are discharged again and returned to their original state of equality.

This depolarising current is indeed a most delicate means of discovering previous decomposition. I have really ascertained that under favorable conditions one can observe the polarisation produced during some seconds by a current which decomposes one milligram of water in a century.

Products of decomposition cannot appear at the electrodes without motions of the constituent molecules of the electrolyte throughout the whole length of the liquid. This subject has been studied very carefully, and for a great number of liquids, by Prof. Hittorff, of Münster, and Prof. G. Wiedemann, of Leipsic.

Prof. F. Kohlrausch, of Würzburg, has brought to light the very important fact that in diluted solutions of salts, including hydrates of acids and hydrates of caustic alkalies, every atom under the influence of currents of the same density moves on with its own peculiar velocity, independently of other atoms moving at the same time in the same or in opposite directions. The total amount of chemical motion in every section of the fluid is represented by the sum of the equivalents of the cation gone forwards and of the anion gone backwards, in the same way as in the dualistic theory of electricity, and the total amount of electricity flowing through a section of the conductor corresponds to the sum of positive electricity going forwards and negative electricity going backwards.

This established, Faraday's law tells us that through each section of an electrolytic conductor we have always equivalent electrical and chemical motion. The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion, and accompanies it during all its motions through the interior of the electrolytic fluid. This we may call the electric charge of the atom.

Now the most startling result, perhaps, of Faraday's law is this : If we accept the hypothesis that the elementary substances are composed of atoms we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity. As long as it moves about on the electrolytic liquid each atom remains united

with its electric equivalent or equivalents. At the surface of the electrodes decomposition can take place if there is sufficient electromotive power, and then the atoms give off their electric charges and become electrically neutral.

Now arises the question, Are all these relations between electricity and chemical combination limited to that class of bodies which we know as electrolytes? In order to produce a current of sufficient strength to collect enough of the products of decomposition without producing too much heat in the electrolyte, the substance which we try to decompose ought not to have too much resistance against the current. But this resistance may be very great, and the motion of the ions may be very slow, so slow indeed that we should need to allow it to go on for hundreds of years before we should be able to collect even traces of the products of decomposition; nevertheless all the essential attributes of the process of electrolysis could subsist. If you connect an electrified conductor with one of the electrodes of a cell filled with oil of turpentine, the other with the earth, you will find that the electricity of the conductor is discharged unmistakably more rapidly through the oil of turpentine than if you take it away and fill the cell only with air.

Also in this case we may observe polarisation of the electrodes as a symptom of previous electrolysis. Another sign of electrolytic conduction is that liquids brought between two different metals produce an electromotive force. This is never done by metals of equal temperature, or other conductors which, like metals, let electricity pass without being decomposed.

The same effect is also observed even with a great many rigid bodies, although we have very few solid bodies which allow us to observe this electrolytic conduction with the galvanometer, and even these only to temperatures near to their melting-point. It is nearly impossible to shelter the quadrants of a delicate electrometer against being charged by the insulating bodies by which they are supported.

In all the cases which I have quoted one might suspect that traces of humidity absorbed by the substance or adhering to their surface were the electrolytes. I show you therefore this little Daniell's cell, in which the porous septum has been substituted by a thin stratum of glass. Externally all is symmetrical at both poles; there is nothing in contact with the air but a closed surface of glass, through which two wires of platinum penetrate. The whole charges the electrometer exactly like a Daniell's cell of very great resistance, and this it would not do if the septum of glass did not behave like an electrolyte. All these facts show that electrolytic conduction is not at all limited to solutions of acids or salts.

Hitherto we have studied the motions of ponderable matter as well as of electricity, going on in an electrolyte. Let us study now the forces which are able to produce these motions. It has always appeared somewhat startling to everybody who knows the mighty power of chemical forces, the enormous quantity of heat and of mechanical work which they are able to produce, and who compares with it the exceedingly small electric attraction which the poles of a battery of two Daniell's cells show. Nevertheless this little apparatus is able to decompose water.

The quantity of electricity which can be conveyed by a very small quantity of hydrogen, when measured by its electrostatic forces, is exceedingly great. Faraday saw this, and has endeavored in various ways to give at least an approximate determination. The most powerful batteries of Leyden jars, discharged through a voltameter, give scarcely any visible traces of gases. At present we can give definite numbers. The result is that the electricity of 1 m.grm. of water, separated and communicated to two balls, 1 kilometre distant, would produce an attraction between them, equal to the weight of 25,000 kilos.

The total force exerted by the attraction of an electrified body upon another charged with opposite electricity is always proportional to the quantity of electricity contained in the attracting as on the attracted body, and therefore even the feeble electric tension of two Daniell's elements, acting through an electrolytic cell upon the enormous quantities of electricity with which the constituent ions of water are charged, is mighty enough to separate these elements and to keep them separated.

We now turn to investigate what motions of the ponderable molecules require the action of these forces. Let us begin with the case where the conducting liquid is surrounded everywhere by insulated bodies. Then no electricity can enter, none can go out through its surface, but positive electricity can be driven to one side, negative to the other, by the attracting and repelling forces of external electrified bodies. This process going on as well in every metallic conductor is called "electrostatic induction." Liquid conductors behave quite like metals under these conditions. Prof. Wüllner has proved that even our best insulators, exposed to electric forces for a long time, are charged at last quite in the same way as metals would be charged in an instant. There can be no doubt that even electromotive forces going down to less than 1-100 Daniell produce perfect electrical equilibrium in the interior of an electrolytic liquid.

Another somewhat modified instance of the same effects is afforded by a voltametric cell containing two electrodes of platinum, which are connected with a Daniell's cell, the electromotive force of which is insufficient to decompose the electrolyte. Under this condition the ions carried to the electrodes cannot give off their electric charges. The whole apparatus behaves, as was first accentuated by Sir W. Thomson, like a condenser of enormous capacity.

Observing the polarizing and depolarizing currents in a cell containing two electrodes of platinum, hermetically sealed and freed of all air, we can observe these phenomena with the most feeble electromotive forces of 1-1000 Daniell, and I found that down to this limit the capacity of the platinum surfaces proved to be constant. By taking greater surfaces of platinum I suppose it will be possible to reach a limit much lower than that. If any chemical force existed besides that of the electrical charges which could bind all the pairs of opposite ions together, and require any amount of work to be vanquished, an inferior limit to the electromotive forces ought to exist, which forces are able to attract the atoms to the electrodes and to charge these as condensers. No phenomenon indicating such a limit has as yet been discovered, and we must conclude, therefore, that no other force resists the motions of the ions through the interior of the liquid than the mutual attractions of their electric charges.

On the contrary, as soon as an ion is to be separated from its electrical charge we find that the electrical forces of the battery meet with a powerful resistance, the overpowering of which requires a good deal of work to be done. Usually the ions, losing their electric charges, are separated at the same time from the liquid; some of them are evolved as gases, others are deposited as rigid strata on the surface of the electrodes, like galvanoplastic copper. But the union of two constituents having powerful affinity to form a chemical compound, as you know very well, produces always a great amount of heat, and heat is equivalent to work. On the contrary, decomposition of the compound substances requires work, because it restores the energy of the chemical forces, which has been spent by the act of combination.

Metals uniting with oxygen or halogens produce heat in the same way, some of them, like potassium, sodium, zinc, even more heat than an equivalent quantity of hydrogen; less oxidisible metals, like copper, silver, platinum, less. We find, therefore, that heat is generated when zinc drives copper out of its combination with the

compound halogen of sulphuric acid, as is the case in a Daniell's cell.

If a galvanic current passes through any conductor, a metallic wire, or an electrolytic fluid, it evolves heat. Mr. Prescott Joule was the first who proved experimentally that if no other work is done by the current the total amount of heat evolved in a galvanic circuit during a certain time is exactly equal to that which ought to have been generated by the chemical actions which have been performed during that time. But this heat is not evolved at the surface of the electrodes, where these chemical actions take place, but it is evolved in all the parts of the circuit, proportionally to the galvanic resistance of every part. From this it is evident that the heat evolved is an immediate effect, not of the chemical action, but of the galvanic current, and that the chemical work of the battery has been spent in producing only the electric action.

If we apply Faraday's law, a definite amount of electricity passing through the circuit corresponds to a definite amount of chemical decomposition going on in every electrolytic cell of the same circuit. According to the theory of electricity the work done by such a definite quantity of electricity which passes, producing a current, is proportionate to the electromotive force acting between both ends of the conductor. You see, therefore, that the electromotive force of a galvanic circuit must be, and is, indeed, proportionate to the heat generated by the sum of all the chemical actions going on in all the electrolytic cells during the passage of the same quantity of electricity. In cells of the galvanic battery chemical forces are brought into action able to produce work; in cells in which decomposition is occurring work must be done against opposing chemical forces; the rest of the work done appears as heat evolved by the current, as far as it is not used up to produce motions of magnets or other equivalents of work.

Hitherto we have supposed that the ion with its electric charge is separated from the fluid. But the ponderable atoms can give off their electricity to the electrode, and remain in the liquid, being now electrically neutral. This makes almost no difference in the value of the electromotive force. For instance, if chlorine is separated at the anode, it will remain at first absorbed by the liquid; if the solution becomes saturated, or if we make a vacuum over the liquid, the gas will rise in bubbles. The electromotive power remains unaltered. The same may be observed with all the other gases. You see in this case that the change of electrically negative chlorine into neutral chlorine is the process which requires so great an amount of work, even if the ponderable matter of the atoms remains where it was.

The more the surface of the positive electrode is covered with negative atoms of the anion, and the negative with the positive ones of the cation, the more the attracting force of the electrodes exerted upon the ions of the liquid is diminished by this second stratum of opposite electricity covering them. On the contrary, the force with which the positive electricity of an atom of hydrogen is attracted towards the negatively charged metal increases in proportion as more negative electricity collects before it on the metal, and the more negative electricity collects behind it in the fluid.

Such is the mechanism by which electric force is concentrated and increased in its intensity to such a degree that it becomes able to overpower the mightiest chemical affinities we know of. If this can be done by a polarized surface, acting like a condenser, charged by a very moderate electromotive force, can the attractions between the enormous electric charges of anions and cations play an unimportant and indifferent part in chemical affinity?

You see, therefore, if we use the language of the dualistic theory and treat positive and negative electricities as two substances, the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom with different force. Potassium, sodium, zinc, must have strong attraction to a positive charge; oxygen, chlorine, bromine to a negative charge.

Faraday very often recurs to this to express his conviction that the forces termed chemical affinity and electricity are one and the same. I have endeavored to give you a survey of the facts in their mutual connection, avoiding, as far as possible, introducing other hypotheses, except the atomic theory of modern chemistry. I think the facts leave no doubt that the very mightiest among the chemical forces are of electric origin. The atoms cling to their electric charges and the opposite electric charges cling to the atoms. But I don't suppose that other molecular forces are excluded, working directly from atom to atom. Several of our leading chemists have begun lately to distinguish two classes of compounds, molecular aggregates and typical compounds. The latter are united by atomic affinities, the former not. Electrolytes belong to the latter class.

If we conclude from the facts that every unit of affinity of every atom is charged always with one equivalent either of positive or of negative electricity, they can form compounds, being electrically neutral, only if every unit charged positively unites under the influence of a mighty electric attraction with another unit charged negatively. You see that this ought to produce compounds in which every unit of affinity of every atom is connected with one and only with one other unit of another atom. This is, as you will see immediately, indeed, the modern chemical theory of quantivalence, comprising all the saturated compounds. The fact that even elementary substances, with few exceptions, have molecules composed of two atoms, makes it probable that even in these cases electric neutralization is produced by the combination of two atoms, each charged with its electric equivalent, not by neutralization of every single unit of affinity.

But I abstain from entering into mere specialties, as for instance, the question of unsaturated compounds; perhaps I have gone already too far. I would not have dared to do it if I did not feel myself sheltered by the authority of that great man who was guided by a never-erring instinct of truth. I thought that the best I could do for his memory was to recall to the minds of the men, by the energy and intelligence of whom chemistry has undergone its modern astonishing development, what important treasures of knowledge lie still hidden in the works of that wonderful genius. I am not sufficiently acquainted with chemistry to be confident that I have given the right interpretation, that interpretation which Faraday himself would have given perhaps, if he had known the law of chemical quantivalence, if he had had the experimental means of ascertaining how large the extent, how unexceptional the accuracy of his law really is; and if he had known the precise formulation of the law of energy applied to chemical work, and of the laws which determine the distribution of electric forces in space as well as in ponderable bodies transmitting electric current or forming condensers. I shall consider my work of to-day well rewarded if I have succeeded in kindling anew the interest of chemists for the electro-chemical part of their science.

MANUFACTURE OF SODA FROM SULPHATE.—Salt-cake is produced in quantity in California in the manufacture of nitric acid. As coal and lime-stone are dear in California, Le Blanc's process is not economical. The author therefore proposes to mix a solution of salt cake with calcium sulphite and pass in sulphurous acid. Soluble calcium bisulphite is formed, and by decomposition calcium sulphate and sodium bisulphite. The two salts are separated by filtration, and the sodium bisulphite is treated with milk of lime. The result is a solution of caustic soda, retaining a certain quantity of sodium sulphite and sulphate, which is evaporated down in the usual manner, and calcium sulphite, which is used again in the process.—J. PUTZKOW,

THE YELLOWSTONE NATIONAL PARK.

ANNUAL REPORT OF THE SUPERINTENDENT OF THE YELLOWSTONE NATIONAL PARK—to the Secretary of the Interior, for the year 1880.—Washington, 1881.

The chief feature of interest found in this report is a description of what is termed the Hoodoo Region, which is a terribly broken and eroded portion of the head branches of the East Fork of the Yellowstone and the Passamaria or Stinking Water Fork of the Big Horn, and which until this occasion had never been visited by tourists or government explorers; all previous information having been derived from a small party of prospecting miners, two of whom were killed by the Indians.

Mr. P. W. Norris, the Superintendent of the Park, having arrived on the ground with his party, some made sketches of the many weird wonders of erosion, copies of which may be found on another page of this issue, and Mr. Norris with an attendant took the elevations of the adjacent peaks, including Hoodoo Mountain. The latter was found to be 10,700 feet high (anneroid-barometer measurement).

This mountain is about a mile in length, and must present a most extraordinary appearance, and while probably itself not a crater, it is evidently of volcanic origin, and was eroded in its present form. Its southern face is still changing, here extending from 500 to 1500 feet below the summit; the frosts and storms of untold ages in an Alpine climate have worn about a dozen labyrinths of countless deep, narrow, tortuous channels amid the long, slender, tottering pillars, shafts and spires of conglomerate breccia and other remaining volcanic rocks. In shape they are described as being unlike any elsewhere known, being a cross between the usual spire and steeple form, and the slender-based, and flat, tottering, table-topped sandstone monuments, near the Garden of the Gods, in Colorado. And while lacking the symmetry and beauty of these, surpass both in wild weird fascination. Here the sharp-cornered fragments of rocks of nearly every size, form and formation, and shade of coloring, by a peculiar volcanic cement attached sideways, endwise, and upon the tops, sides, and apparently unsupported upon each other, represent every form, garb and posture of gigantic human beings, as well as of birds, beasts and reptiles. In fact nearly every form, animate or inanimate, real or chimerical ever actually seen or conjured by the imagination may here be observed. With this description and the illustrations we have given on page 187, some idea may be formed of the wild unearthly appearance of these eroded Hoodoos of the Goblin Land. These monuments are from 50 to 300 feet in height, with narrow tortuous passages between them, with sometimes tunnels, where the Big Horn sheep hide in safety; while the ceaseless but ever changing moans of the wild winds seem to chant fitting requiems to these gnome-like monuments of the legendary Indian Gods.

We have not sufficient space to allow us even to briefly follow Superintendent Norris in his very interesting description of the many wonders of this extraordinary region—the Yellowstone Lake, Geysers, cold, hot and medicinal springs, pulsating Geysers, terrace building springs, fossil forests, natural bridges, gold and silver mines, and many objects of scientific interest. Among the animals still to be found in the Yellowstone Park, mention is made of the bison, or mountain buffalo, which differs considerably from the bison of the plains; also the moose, elk, white tailed deer, black tailed deer, antelope, big horn sheep, bears, mountain lion or cougar, wolves, foxes, skunks, badgers, rock dog, porcupine, rabbits, rats, mice, moles, squirrels, chipmunks, beavers, otters, etc., etc.

We note the presence in trout, found in the cold water tributaries, of a "worm" named by Dr. Leidy "*Dibothrium cordiceps*." They are described "as long, slender, white worms, found in the intestines and flesh of the countless large and beautiful trout of the Yellowstone Lake, named by Professor Cope, *Salmo pleuriticus*." They are said to be entirely different to the worms found in European trout. The Superintendent does not appear to have succeeded in tracing the cause of this parasite, but states that they are only met with in fish found in the Yellowstone Lake. Here the trout exist in great numbers in water bubbling with hot gases; and the angler, without changing his position, or removing the fish from the hook, can rapidly boil them in seething pools.

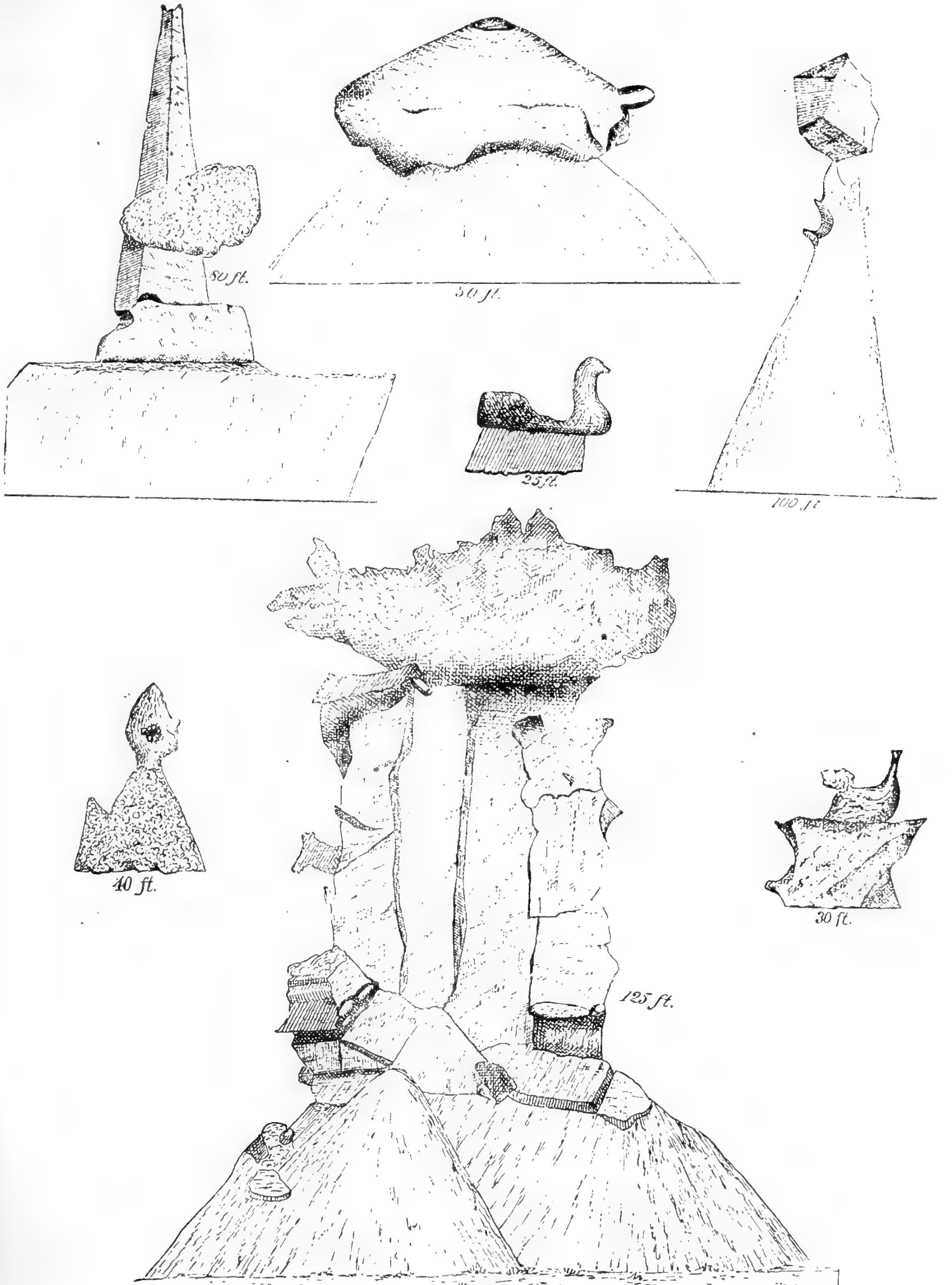
This National Park of the United States was visited by over 2000 tourists during the season previous to this report, all of whom returned in safety, although much inconvenience was experienced from the condition of the roads. An appropriation is now annually made for the improvement of the Park; and no one reading this report can fail to come to the conclusion that Mr. P. W. Norris is a gentleman highly qualified for the position of Superintendent, and brings an enthusiastic devotion to bear on his arduous duties in developing this "peerless wonder-land of earth."

Statement showing the *mean temperature* at BOSTON, MASS., for *each month* and *year* from *January*, 1871, to *December*, 1880, inclusive, as recorded at the *station of observation* of the *Signal Service, U. S. Army*, at that place.

[Compiled from the records on file at the office of the Chief Signal Officer, U. S. A., Washington, D. C.]

YEAR.	MEAN TEMPERATURE.												
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Year.
1871	27.2	29.4	42.8	46.7	57.4	66.2	71.0	71.8	59.7	54.2	39.8	38.4	49.5
1872	27.5	28.5	26.4	46.3	57.2	67.6	74.4	71.8	63.8	52.1	40.7	24.1	48.4
1873	26.3	27.6	34.1	44.6	56.9	67.2	72.9	68.8	61.7	53.0	33.4	32.9	48.3
1874	31.2	27.9	35.1	39.1	56.1	65.8	72.2	67.7	63.8	53.4	41.5	31.9	48.3
1875	20.9	22.8	31.7	42.2	57.3	66.5	71.7	69.7	58.8	49.3	34.4	29.8	46.3
1876	30.5	27.5	32.9	43.2	53.9	67.6	73.1	69.5	58.9	48.0	40.8	22.3	47.3
1877	24.2	33.6	35.1	44.3	54.9	66.5	69.9	70.7	63.9	51.3	43.8	36.1	49.5
1878	28.3	31.0	39.5	47.2	55.3	64.2	72.7	68.1	62.9	55.3	39.9	29.6	49.5
1879	24.5	24.5	33.8	42.4	59.4	64.2	69.9	67.7	60.8	56.6	39.2	32.6	48.0
1880	35.0	32.2	33.1	45.9	62.7	67.7	71.1	68.9	64.1	50.8	37.5	26.2	49.6

WAR DEPARTMENT,
OFFICE OF CHIEF SIGNAL OFFICER,
WASHINGTON, D. C., *April* 12, 1881.



HOODOOS OR REMNANTS OF EROSION IN THE GOBLIN LABYRINTHS.

YELLOW STONE NATIONAL PARK.

NOTE ON THE SENSORY TRACT OF THE BRAIN.

By Edward C. Spitzka, M. D., New York City.

It is well known that Meynert ([1]) and those who followed that distinguished anatomist, believed that the tract through which the conscious sensory impressions reach the cortex, extends from the columns of Goll and Burdach, of the cord and lower oblongata, through the so-called superior or sensory decussation, to the anterior pyramids; that thence the tract runs with the anterior pyramids in their outermost third through the pons and pes pedunculi, courses between the thalamus and lenticular nucleus in the posterior third of the internal capsule, and arching back, terminates in the cortex of the occipital lobe. Flechsig showed that what Meynert interpreted as the sensory pyramidal decussation, has no connection with the anterior pyramids, but, on the contrary, enters the lemniscus layer, or interolivary strand, whose relations to the corpora quadrigemina had been explained by Meynert, although he was befogged as to its lower relations, owing to the aforesaid confounding with the anterior pyramids proper.

Now, Flechsig ([2]) distinctly states in his work that the explanation he has been able to furnish of the real nature of the superior decussation, demonstrates the non-exist ence of a direct tract from that decussation to the cortex.

The true tract has, however, been known to exist, although the relations have not been properly interpreted. The lemniscus layer is not only a detachment from the corpora quadrigemina, but also distinctly incorporates a peculiar bundle, described by Henle as a fasciculus, from the pes to the tegmentum([3]). This tract continues, in at least a part of the fibres, from the columns of Goll and Burdach to the pes pedunculi and thence, no doubt, to the cortex of the brain. The circuit for the conscious sensory impressions transmitted by the cord, and proposed by Meynert, therefore becomes re-established, with a modification, namely, that the sensory tract does not run through the pyramids and pons, but immediately above them, and after entering the *pes pedunculi* probably takes the course claimed by Meynert.

That there is a close relation between the pyramidal tracts and the by-track from the superior decussation to the *pes pedunculi*, is proven by an interesting observation which I have been able to make on the elephant's brain. In this animal([4]) the entire pyramidal tract takes the course of the by-track, that is, there are no vertical fibres in the pons. The crus is continued bodily above the latter (which is composed exclusively of transverse fibres) to take the usual course on the ventral and medial aspect of the olivary nucleus.

This fact strengthens the proposition of Meynert, that there intervenes a third projection series between that of the tegmentum and that of the pes pedunculi, for which he proposes the name of the *stratum intermedium* ([5]). In man, I believe this stratum intermedium to be the main tract for the conveyance of conscious sensory impressions from the general sensory periphery, while in other animals, at least in the elephant, it is at the same time the voluntary motor tract.

That the sensory fibres occupy the most posterior portion of the internal capsule, while they compose the most dorsal in the pes pedunculi, shows that the fibres of the latter must pursue a spirally twisted course before entering the brain. Such an arrangement seems to be indicated, indeed, in the outer contours of the crus. In an early human embryo, of about the third month, I find a well marked columnar elevation running from the outer part of the crus through the pons, where it touches its fellow of the opposite side, and then passes between the olives([6]). This I regard as the embryonically distinct stratum intermedium.

([1]). Das Gehirn der Saügethiere, in Stricker's Histology.
([2]). Die Leitungsbahnen des Gehirnes und Rückenmarks. 1875.
([3]). Lehrbuch der Anatomie des Menschen. 1872.
([4]). "Science," February 7, 1881. ([5]). Archiv fuer Psychiatrie. 1874.
([6]). Demonstrated before the N. Y. Neurological Society, March 1, 1881.

ASTRONOMIAL MEMORANDA.

A small pamphlet containing notes, corrections, etc., to the "Handbook of Double Stars," has been recently prepared by Messrs. Crossly, Gledhill and Wilson. In the introduction, the editors say: "The corrections have been thrown into two classes: the first contains those which from their importance demand immediate attention in order to save waste of time. These the reader is requested to insert at once. In the second list will be found a large number of corrections which may be entered as the stars are observed or read.

A very copious set of additional notes has also been drawn up, embodying, so far as we know them, the most recent and improved orbits, measures and discoveries.

It seems probable that the asteroid, No. 220, discovered by Palisa on the 23d of March, is identical with No. 139, Juewa. Juewa was discovered by the late Prof. Watson while engaged upon one of the transit of Venus parties in 1874 at Pekin. The asteroid was observed by Rümker at Hamburg, on November 8th of the same year, but since that date it has not been seen.

Nature for March 17, contains the following note upon the largest refractor in the world. "A very interesting scientific work, the most important of its kind yet attempted in the kingdom, has just been completed. It is the great refracting telescope, constructed by Mr. Grubb, of Rathmines, Dublin, for the Austro-Hungarian Government, and it is to be placed in the Observatory at Vienna. A commission appointed by the Government to examine the work, transmitted yesterday to the Austro-Hungarian Embassy, in London, a report expressing their full approval of the manner in which the task has been completed. It is a matter of no little pride to Ireland that she has produced the largest refracting as well as the largest reflecting telescope in the world." The object glass of this instrument is 27 inches in diameter or 1 inch larger than that of the Washington Refractor made by Clark.

W. C. W.

Washington, D. C., *April* 21, 1881.

INTRA-MERCURIAL PLANETS.

In "Science" of February 26, appeared an article on the above subject by "W. C. W.," which I have read with considerable personal interest, wherein we are led to infer, from purely negative testimony alone, that no such objects were seen during the total eclipse of Aug. 29, 1878, either by the late Prof. Watson or myself. Unfortunately, Prof. Watson's tongue and pen are now silent, and no one exists to defend his observations. What he has written on the subject the astronomical world is familiar with. It is about my own I wish to speak, and in defending them against the negative testimony which your correspondent brings, I hope to be able to convince the reader that because the observers whom he cites saw no planets, it is very far from proving their non-existence.

If the reader will refer to the article itself, he will find delineated on a chart the ground swept over by six observers, but he fails to tell us how short a time was devoted to a search west of the sun, and especially in the immediate region of the two objects seen by me, and near which one of Watson's objects was, viz., near θ Cancri. As not one in a thousand of your readers will have the privilege of reading the reports of those six observers, just published by the Naval Observatory, and, are therefore incapable of forming a correct conclusion on the subject, I have thought it advisable to quote what they really say, and, to remark, that when negative testimony is arrayed against positive, it is very important that its weight, if it has any, be carefully considered.

First, let the fact be stated, that during the total phase of the eclipse which lasted but 162 seconds, two experienced observers, with telescopes in every way well adapted for the work, state with positiveness that each saw two objects not down on any star chart, and, that they were not there when the sun had sufficiently withdrawn to allow the locality to be re-observed. On the other hand, three observers who searched west of the sun, one in a cloudy sky, and two of the others poorly equipped, and, devoting but a few seconds to the search, saw nothing, not even θ Cancri, a star of the fifth magnitude, near where one of Watson's and both of my objects were seen. The weakness of this negative testimony will be apparent from a few extracts from their reports.

Mr. Wheeler (telescope 5 inch, power 100) says, he observed the second and third contacts (beginning and end of totality), also the Corona on both sides of the sun, saw with the naked eye Venus, Mercury and Regulus, observed carefully the several prominences, etc., and then says, "An unsatisfactory attempt was made to sweep for Vulcan. The time given to it was limited, as I was expected to observe all the contacts, and time was consumed in recording the second, and again in bringing the telescope into position for observing the third contact." Now when it is considered that he undoubtedly occupied several seconds in looking at the grand sight with the naked eye, and, that the power used was altogether too high, and of course, the field very small, the time devoted to the search for Vulcan could have been but a few seconds. Is it therefore surprising that Mr. Wheeler saw nothing of the objects seen by me? Only those familiar with the use of telescopes know how perplexingly difficult it is to bring a well-known object in the field of a telescope, using a power of 100.

Mr. Bowman (telescope 3 inch, power 30) says he searched *north* and west of the sun (my objects, also Watson's, were southwest), and that some time was lost (during totality) in exchanging the diagonal tube for the straight one, swept to the westward 5° or 6° in the declination of the sun, and then returning, shifted the declination just far enough *north* to clear the Corona and swept to the westward again, then returned to the R. A. of the sun and shifted to the proper declination just in time to observe the third contact. When it is considered how much precious time was lost in observing and recording in his note-book the time of second contact, changing tubes, and probably observing the eclipse for several seconds with his naked eye, which he could hardly refrain from doing, is it at all wonderful that Mr. Bowman saw nothing of my objects or Watson's either?

Prof. Todd (telescope 4 inch, power 20) says, "I searched 15° each side of the sun, but the sky was *cloudy*, so much so that I was unable to see Delta Cancri," (a 4th mag. star). He does not say how much time he spent searching west of the sun. It certainly could have been but a moment, and, in the region where my objects were, but a few seconds. He, too, observed the second contact, also the Corona, saw Mercury, Venus, Mars, and Procyon. Again I ask is it at all surprising that Prof. Todd saw nothing of the objects seen by me?

Prof. Pritchett (telescope 3½ inch, power 90) says he first observed the grand scene with a naked eye, then swept along the ecliptic several degrees *each* side of the sun, observed all the phenomena of the eclipse, the second contact, Corona, the prominences, and the question arises how many seconds he searched with a very small field west of the sun for the "Ghost of Vulcan," as he facetiously calls it. Still again I ask is it at all wonderful that Prof. Pritchett saw nothing of the objects seen by me? Would it not, in fact, have been very surprising had he seen them at all?

Your correspondent has given in his diagram the outlines of the regions swept over by the above observers, saying: "The place of one of Watson's stars was covered by Wheeler, Bowman and Pritchett, and the place of Swift's two stars was examined by Bowman and Wheeler, and that one of the stars appears in the corner of Pritchett's sweep." Now all this is calculated to convey a wrong impression, for it is not likely that either of them knew within from 1° to 3° the exact boundaries of their hastily-made sweeps; neither do I pretend to be exact about the location of the stars I saw, although I made three estimates of their deviation and distance from the sun, by sighting along the outside of the telescope tube.

They are wrongly placed in the diagram. They were nearer where Theta is, and probably somewhat west of it, which would place it outside of the sweeps of all the observers. I should strongly suspect that one of them was θ, were it not that Watson, who says he saw that star, says nothing about another equally bright some 7′ from it, both ranging with the sun's centre.

Neither in his published statements, or letters to me, does he allude to this vital point. It was as impossible for him to have seen one and not the other, as for one to see Epsilon 4 Lyræ, without, at the same time, seeing Epsilon 5.

Again, he says, as far as relative position is concerned, my objects resemble closely d^2 Cancri, and B. A. C. 2810, on the *east* side of the sun. I hope he does not mean to be understood as inferring that it was on the east, instead of the west, of the sun I was searching.

Finally, he says, the existence of an intra-mercurial planet is not yet admitted by the majority of astronomers. This may be true, but I hope their opinion is based on stronger evidence than that adduced by "W. C. W."

LEWIS SWIFT.

ROCHESTER, N. Y., *April* 11, 1881.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of SCIENCE :—

The article on "Discrepancies in Recent Science" in a late number of this journal demands some attention, not because the Nebula Theory is seriously threatened by it, but because it properly calls attentions to some physical inferences that have been drawn from other phenomena and applied to the Nebula Theory, especially in the domain of heat. It is assumed by the writers quoted in that article, that *luminousness implies high temperature* and also that the rarity of the gaseous material of the nebula is the immediate result of the high temperature of the constituent atoms. Neither of these assumptions is correct. The trouble comes chiefly from the writer's failure to make the proper distinction between *energy* and *heat*, and I apprehend, also, in the failure to see clearly what the nature of heat is. Most of the books treat of this in a very loose way, and most of the statements on the subject by Mr. Charles Morris are wrong. How far wrong may be seen by comparing his statements with the following quotation from "The Mechanical Theory of Heat," by Clausius, Chap. 1st, Sec. X, p. 24: "*All heat existing in a body is appreciable by the touch and by the thermometer; the heat which disappears under the above changes of condition (fusion and vaporization) exist no longer as heat, but has been converted into work, and the heat which makes its appearance under the opposite changes (solidification and condensation) does not come from any concealed source, but is newly produced by work done on the body.*" We have all along been familiar with the conception of *heat as a mode of motion*, but not with the character of the motion except as "a brisk agitation of the molecules" or "a rapid vibration of the atoms;" but there are two kinds of vibratory motions possible to atoms, one of the character of pendulous motion or a

change of position in space of the centre of gravity of the atom, and the other the change of form of the atom itself; the first of these is known as free path motion, and the second as heat. The evidence for this may be briefly given.

First—It is certain that a heated body loses its heat by radiation, that is, it imparts its motion to the ether which transmits it in every direction as undulations having certain wave lengths and amplitudes. Second—It is certain that the energy of such undulations depends upon the amplitude of such undulations, and if the amplitude of the undulation was measured by the free path of the atom, then the radiant energy of the atom would vary as its free path, or in other words the rarer a gas is the greater its radiant energy. Now when the spectrum of a gas, say hydrogen, is examined, it is seen to be composed of lines having definite wave lengths, and wave length is dependent solely upon the rate of vibration. If this rate depended upon the number of impacts per second of the atoms or molecules of a gas, then these atoms would need to be always at exactly the same distance apart and the velocity of free path motion invariable, which conditions are physically impossible among free atoms, otherwise the spectrum we should obtain would be a continuous spectrum such as solid incandescent bodies give. But the spectrum of hydrogen for a given temperature is the same whether the gas be at ordinary pressure or very rare. This necessitates the conclusion that the heated atom which is thus radiating energy is vibrating quite independent of its position in space or of its free path motion, and the energy embodied in such vibratory motion is often spoken of as *internal energy*. When a swiftly moving bullet strikes a target, both bullet and target are heated and oftentimes a flash of light may be seen at the instant of impact. The free path motion has been changed into atomic vibrations, which at the first instant had a period capable of giving the sensation of light, but if the bullet be picked up at once it may not be uncomfortably hot. Now imagine two atoms in space urged by gravitation towards each other until they strike each other; each will be set vibrating, that is they will both be heated by impact, and until they were thus made to vibrate they would have no temperature at all; their energy would be represented by their free path motion; the greater their distance apart, when they began to approach, the greater would be their velocity at impact, and the period of vibration of each after impact would depend upon the character of the atoms themselves. One might have such a period as to give out undulations that might affect the eyes and we would say it was luminous while the other one might not, luminosity being dependent upon the rate of vibration, not upon the energy of vibration or the amplitude.

There are many phenomena, that are familiar enough, which show that luminosity does not depend upon high temperature. The decaying stump that shines at night, has a temperature not appreciably higher than surrounding objects; the swift moving molecules in a Crookes tube, that spend their energy upon the walls of the tube, cause the latter to glow, and the molecules themselves shine as they move in their long, free paths, but the tube is not uncomfortably hot, much less *very hot*. It is true that by increasing the energy of the moving atoms, the tube may be made red hot, but the point here is, that this is not essential for luminosity.

If then, in the process of universe building, we start with dissociated atoms, without any temperature,—at absolute zero, and let gravitation alone act among them, the first motions will be free path motions, and there will be no such thing as heat until atomic impact has begun; the energy that was at first represented solely by gravitation will now be partly changed into heat and radiation proper will begin, and the actual loss of energy to the involved atom will be greater than what would be due solely to gravitative approach; there might be luminousness with very little temperature, and one might speak of it as "fire mist," and as "glowing vapor," and yet not threaten the "Law of Interaction of Forces." Neither does the Nebula Theory fall, if originally matter was not hot, but cold.

TUFTS COLLEGE, MASS.

A. E. DOLBEAR.

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of "SCIENCE:"

In his communication to your excellent journal (Vol. II., p. 142), Mr. Larkin has very correctly stated the discrepancy which is contained in the designation "fire-mist," as applied to the initiatory stage of nebular cosmogeny, the "Chaos" of Laplace—*sit venia verbo!* If the Nebular Hypothesis is a true representation of the history of our solar system (or all solar and other systems, for that matter) then, certainly, *heat* could have been present only after motion, and very lively motion at that, had been going on for quite a number of—well, let us say, billions of years, or pretty nearly that.

As soon as motion, *i. e.*, aggregation (and rotation) had begun, then, by the impact of the more distant portions of matter on those nearer the centre of the solar nucleus, heat was produced equivalent to the motion thus arrested. The primordial "Chaos," therefore, was cold and dark, if it ever did exist at all.

Mr. Larkin, consequently, is correct: There *is* a discrepancy!

Not so, Mr. Morris, whose objection is stated, *in nuce*, by himself (Vol. II., No. 41) in these words:

"Temperature and heat are very different things."

"It is one thing to contain heat and another thing to be in what we call a heated state."

To prove this he mentions the generally accepted facts "that a mass of water at 32° contains far more heat than an equal mass of ice at the same temperature; and a mass of water gas, (steam?) at 212° contains far more heat than an equal mass of water at that temperature."

The foregoing facts illustrate the phenomenon of "latent heat" or heat not appreciable by the thermometer. But *latent heat is not heat!* It is a misnomer that should have been eradicated from scientific nomenclature long ago. The heat which melts a pound of ice is employed in *performing* a certain amount of *work* by *overcoming* the *cohesion* of the solid ice. Its subsequent liquid state is the result of this work of heat. This heat has disappeared, is no more heat; exactly as the muscular force of the locksmith's arm disappears (is latent) at night, because by eight hours of filing he has overcome the cohesion of a quantity of iron. We can not look for the work and the force spent on it at the same time.

The greater mobility of the liquid and the diminished cohesion are the equivalent of the heat that has "become latent," *i. e.*, disappeared, absolutely, utterly and entirely, as heat. In changing water back again into ice, from the liquid into the solid state, the same amount of heat must be liberated, withdrawn, or allowed to escape, as was necessary to melt it.

Water, therefore, does not contain more *heat* than ice at 32° F.; it contains more mobility, energy, potentiality —in short, more *motion*, but not motion of the heat kind.

The same relations exist between water and steam at 212° F. Here the peculiar property of the gaseous condition allows us to appreciate the nature of the difference between water and steam much more precisely than that between water and ice. "Latent heat" is here simply *expansion*, and as expansion is the work of heat it is not heat. This we can prove by confining steam or any gas in a vessel with a movable wall. If the gas just fills the receptacle and we now apply heat, a thermometer will show a rise of temperature in the interior of the vessel.

As soon as the heat reaches a certain point, so that the

expansive force of the gas equals the resistance by the weight or friction of the movable wall, the latter will move and our thermometer, indicating the temperature in the interior of the vessel, will record *a fall of temperature.* Heat has "become latent," has disappeared, because it has done work, has moved the wall.

It is, therefore, not in accordance with the facts, if Mr. Morris states that a thing may be in a heated state and yet not contain heat. Nor is it true that "as density diminishes the heat capacity increases." The true state of things is evident from the examples given. Since diminution of density is effected by heat, is the work of heat, the gas thus expanded does not contain more, but less heat. The attenuation, the change of its state of cohesion, is the action of a certain amount of heat, and this heat has "become latent," has disappeared as heat, but it nevertheless exists in the expanded gas as a greater range of mobility, as diminished density. On condensation this heat again reappears, *i. e.* the range of mobility of the gas is diminished, and the motion, potentiality, energy or whatever name Mr. Morris should prefer to apply to this *causa efficiens*, is transformed into heat. This is, by the way, the very process that is supposed to have been going on in the Laplace "Chaos," and, therefore, proves again the discrepancy between the principle of the conservation of energy and the assumption that the primordial nebula was a "fire-mist," as Mr. Larkin has correctly explained.

As to Mr. Morris' conception of the action of gravitation, it seems still more erroneous. This gentleman says: "The earth must fall towards the body with the same energy that the body displays in falling towards the earth." Now, the two fundamental laws of gravitation, as first discovered by Newton are: Attraction acts in direct proportion to mass and in indirect proportion to the square of the distance.

The statement of Mr. Morris, therefore, is absolutely false.

Nor is this all. The possibility that gravity can act lies in the space given for the fall of a body acted upon. If, therefore, a body should fall, it must be raised first to allow it space in which to fall. If by some force 100 pounds are raised to the height of one foot, this body, if unsupported, will by its fall develop the very same force as was employed to raise it previously, viz.: 100 foot-pounds. In striking on some resistance, say the surface of the earth, it will develop an amount of heat, equivalent to its mechanical force 100 foot-pounds = 100 calorics or (small) heat units. Mr. Morris ignores the element of space, in so far as it must always have been previously furnished. He says:

"The motion that exists in a falling body was not created for the purpose. It existed in the falling body in some other form and has simply been transformed, not created."

But nobody ever contended that it was created. The possibility of its fall was given to the body by some elevating force, and it is the very same force which, having existed as potentiality to fall, as long as the body was supported, is transformed into motion, into falling, as soon as the support is withdrawn. And this fall is therefore not due to "internal forces" but to gravity and the space furnished by previous raising.

But is the discrepancy laid bare by Mr. Larkin the only one existing in regard to the Nebular Hypothesis? Nay, is this hypothesis really the true history of the world, and of our planetary system in particular? Is it as free from uncertainties and as little at variance with observed facts, as an hypothesis, which has found such ready credence and such universal approval, should be?

I believe it has no claim to such confidence as has been bestowed upon it, and since I have gone so far, I might just as well state a few reasons for my conviction, which I am glad to say is shared, or rather also held, by one of our first astronomers. This I shall do further on, but at present would beg to call the attention of your readers to two other discrepancies which exist between the principle of the conservation of energy and the Nebular Hypothesis—assuming, for argument's sake, the truth of the latter.

They may be best stated by two questions, to wit:

1. What existed before "Chaos," and how was "Chaos" brought about?

2. How long and by what influence did "Chaos" remain at rest, and what cause acted on it to force it into formative action?

I have never been able to get a satisfactory answer to these questions, nor have I been successful in answering them myself. They seem to convey the impression that a most glaring discrepancy with "conservation of energy" exists in this matter, of which every reader may become convinced on mature reflection.

For, in the light of the principle of the conservation of energy, motionless matter is an impossibility, as it is totally inconceivable to our understanding in every aspect. Again: Why should attraction—if we suppose that it was dormant while the primordial nebula was forming—suddenly begin to act? What force, what cause, instigated this action at a particular moment?

It has been said, and truly, I think, that it is a very cheap and easy matter to write upon the Nebular Hypothesis. But is it also an easy thing to write *against* this hypothesis? From the fact that it is rarely done, it would appear that it is not; and yet there are dozens of facts and arguments to be brought against it. Of these I will only name the more prominent ones, without going into detail:

The eccentricity of all planetary and lunar orbits.

The various deviations of the planes of all the planetary and lunar orbits from each other.

The retrograde motion of the moons of Uranus and of that of Neptune.

The composition of the rings of Saturn.

The greater velocity of revolution of the innermost portions of the rings than that of Saturn's surface.

The greater velocity of revolution of the inner moon of Mars than that of the latter's surface.

The immense number of comets and meteorites; their great eccentricities; the considerable number that have retrograde motion; the absence of any planetary nebula, so-called, which would allow of being interpreted as an initiatory stage of the formation of a solar system.

The multiple stars, etc.

The elaboration of the various facts above stated and several others* would far exceed the proper limits of this communication—which have, indeed, been rather overstepped already. I may be allowed, however, to add what Prof. Asaph Hall, to whom the paper described in the foot-note was submitted, wrote in reply:

"To me the Nebular Hypothesis is a very doubtful thing. The facts you mention are against it. Possibly its supporters may fudge it so that it will last a little longer, but it is always unsafe to rest on a theory based largely on our ignorance."

As may be imagined, I was very agreeably surprised by this card and asked Prof. Hall's permission to make use of it in case occasion should offer. This permission the excellent gentleman first declined to give; his reasons for so doing were stated in the following language:

"Such questions as the Nebular Hypothesis will only be decided by the slow growth of observation and knowledge and not by the opinion of this man or that. I have but little faith in it. . . . ."

After some further expostulation on my part, this permission was kindly given in a letter, written December 9, 1879. Not, however, until now have I made any use of

* This I have done in a paper published in the November and December issues of the *Gaea* of the 1878, edited by Dr. Hermann Klein, at Cologne, Germany.

it, except to refer to it in a general way.* I am glad that the discussion arising from Mr. Larkin's letter has furnished such opportunity, and avail myself of it to give it to "SCIENCE" for publication. GEO. W. RACHEL.

NEW YORK, *April* 11, 1881.

MICROSCOPICAL NOTES.

Recent investigations respecting the pathological relations of diphtheria, and the discovery of a micrococcal organism in the false membrane, have made it almost certain that the morbid poison which gives rise to the disease is a parasitic organism. M. Talamon now states that he has succeeded in finding this organism in eight cases. In the condition of complete development they presented a characteristic mycelium and spores. The former are tubes with partitions from two to five thousandths of a millimetre in length. These under favorable circumstances, elongate and bifurcate, the bifurcations being characteristic in consequence of their incurved branches, like the sides of a lyre. In other conditions the mycelia do not become elongated, although they multiply so rapidly as to cover the surface of the cultivated liquid; they remain short and assume irregular forms, and give rise to numerous straight rods. The spores are of two kinds, round or oval, which may be termed the spores of germination, and rectangular spores or conidia. The latter characterize the species. They form small rectangles of various sizes, their length being sometimes fifteen thousandths of a millimetre. They may be isolated or united in festoons or zigzag chains. At first homogeneous they soon become filled with small round granules, highly refracting, and of the size of ordinary micrococci. The round or oval spores are those which by their elongation constitute the mycelium. They appear as clear points, from three to five thousandths of a millimetre in diameter, in the middle of a mass of granular material.

Animals and birds inoculated with these organisms all died developing the characteristic false membrane.

These facts are very important, and open up an entirely new field of investigation, and M. Talamon already hints that he has a clue as to the source from which the organism is derived in the case of human infection. We trust that those of our subscribers who possess microscopes will follow up the researches of M. Talamon, which promise results of the highest value to science and to humanity.

NOTES.

THE bicarbonate of soda prepared by the Solvay process contains from two to three per cent. ammonia, and is therefore not suitable for pharmaceutical use, and for certain technological purposes.

A NOVEL FERRIC HYDRATE.—C. Graebe has received from the Baden Aniline and Soda works a reddish crystalline substance deposited in cast-iron vessels in which potash has been melted. It has the same composition as hematite and gœthite, but its specific gravity is only 2.93.

CHLORINATED DERIVATIVES OF CARBAZOL.—On treating carbazol suspended in acetic acid with chlorine, the liquid turns blue, yellowish, greenish, and, lastly, red. If the reaction is then interrupted the product is trichloro-carbazol in white needles, melting at 185°. If the process is continued for ten or twelve hours, hexachloro-carbazol is produced, fusible at 225°.—W. KNECHT.

PRESENCE OF ALCOHOL IN THE SOIL, THE WATER AND THE ATMOSPHERE.—A. Muntz has previously shown that it is possible to detect exceedingly slight traces of alcohol by converting it into iodoform. On concentrating the alcohol in a small volume of water by means of fractionated distillation, and using the microscope to verify the presence of iodoform, he was able to recognize with ease 1-300,000th of alcohol mixed with water. He has since improved the process so as to detect quantities even smaller than 1-1,000,000. During the last four years he has applied this method to river, spring, and sea-water, as well as to rain and snow. The results obtained leave no doubt of the presence of a neutral body, more volatile than water, and yielding iodoform. He thinks alcohol is the hydro-carburetted body present in the atmosphere, indicated by the researches of Boussingault and De Saussure. Soils rich in organic matter yield alcohol in such proportions that its essential properties may easily be verified.

* "SCIENCE," Vol. I, p. 246, foot-note to the paper on Friedrich Mohr's Life and Works; *Scientific American Supplement* No. 266, p. 4,241, in a paper on "The Actual Figure of the Earth."

NEW SYNTHESIS OF DESOXY-BENZOIN AND CRYSENE.—MM. Graebe and Bungener have obtained desoxy-benzoin by causing the chloride of phenyl-acetic acid to react upon benzol in presence of aluminium chloride. By the same reaction, naphthalin being substituted for benzol, they produce benzyl-naphthyl-keton. They then reduce with hydriodic acid and phosphorus at 150° to 160°, and pass the vapors of the carbide thus obtained through a red-hot tube, when 4 atoms H are removed and chrysene remains.

CERTAIN PHENOMENA OF OPTICS AND OF VISION.—M. Trève mentions the fact that the flame of a lamp appears brighter, and that a vertical shaft, a post, or mast is seen more distinctly through a vertical than through a horizontal slit, whilst a house, a landscape, or the disk of the sun or moon is perceived more clearly through a horizontal slit. He finds similar differences in photographs according as the light passes from the object to the plate through a vertical or a horizontal slit, and ascribes the results to the action of diffused light.

COPAL varnish for mounting objects for the microscope has been suggested by Mr. Julien Derby of the Quekett Club, who states that Mr. Van Heurck, of Antwerp, who first used it, has met with much success in mounting diatoms with that medium. This varnish is used about the consistency of oil and should be of that brand known as "pale copal." It has about the same refractive index as balsam, and is free from bubbles. Drop the copal over the object and slightly heat over a spirit lamp. In some cases a cover can be dispensed with, as it soon takes the consistency of amber, and is hard enough to sustain wiping and brushing with a soft brush with impunity.

WIDENING OF THE RAYS OF HYDROGEN.—The nebulous expansion of the spectral rays of hydrogen, noticed on increasing the pressure of this gas in a Geissler tube, is still ascribed to the influence of the pressure, though Dr. Shuster, Secchi, and others have shown that it is not possible to alter the pressure of a gas without at the same time affecting the resistance of the medium, and in consequence the temperature of the spark which traverses it. C. Fievez has undertaken to examine separately the influence of the different agents, temperature, pressure, direction of the current, etc., which have been suggested as contributing to produce this phenomenon. He finds that the widening of the hydrogen rays is correlative to the rise of temperature. We may affirm that the temperature of a celestial body is higher than that of another if its hydrogen rays are broader.

RECIPROCAL DISPLACEMENTS OF THE HYDRACIDS.—The action of the hydracids upon the salts formed by the halogens is in general the inverse of that of the elements themselves. Thus hydriodic acid expels hydrochloric acid from the metalic chlorides and hydrobromic acid from the bromides, whilst hydrobromic acid also liberates hydrochloric acid from the chlorides. The chlorides in general are decomposed by hydrobromic acid, and this decomposition preponderates according to the thermic value of the principal action. But the bromides may also be decomposed, though less readily, by hydrochloric acid. This inverse action previously pointed out by M. Hautefeuille in the salts of silver at a red heat, and by the author in the moist way, has lately been observed anew by M. Potilizine, but it is in no way contrary to thermo-chemical principles. It results from the existence of secondary compounds, partially dissociated, which intervene with their peculiar heat of formation. The theory of these reciprocal actions and equilibria is always the same. In every case we have to do with a principal re-action, foreseen by the thermic theory, and a perturbation equally foreseen by the same theory, of which it is a necessary confirmation.—M. BERTHELOT.

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Since we last referred to Mr. Edison and his incandescent lamp, the subject has been advanced another step and the final stage of complete and unqualified success achieved; permission has been granted to the Edison Light Company, to place surface conducting wires under the streets of New York City, and in the course of the next two or three months, one large district of that city will be enjoying the full benefits of Mr. Edison's system of electrical illumination.

Taking a retrospective review of public utterances on this question during the last eighteen months, we now extend our condolence to a certain class of professed scientific experts who have maintained, from first to last, the impracticability of Edison's well-devised plans.

Never in the annals of scientific discovery has a grosser attempt been made to pervert the truth, and mislead public opinion.

As one instance among many, let us take up what is offered as a standard work of reference on this subject: "The Electric Light, its Production and Use, embodying plain directions for the working of galvanic batteries, electric lamps, dynamo-electric machines, etc.," by J. W. Urquhart, C. E., edited by F. C. Webb, M. I. C. E., M. S. T. E., London, 1880. Under the heading of "Edison's Lamps" we find "much interest has been taken in the sensational and often absurd announcements, concerning apparatus in course of perfection by Mr. T. A. Edison, of Menlo Park, New York (?), and it was in *some quarters* thought, that when he had set himself about the task of inventing an efficient subdivision of the electric light circuit, *something* would in all probability be done."

"There is little probability, however, that this lamp (the horse-shoe carbon) will prove constant. Burnt paper in various forms has been repeatedly tried before, and it is assuredly not constant, in the best possible vacuum obtainable." "We may indeed rest assured, that upon further reflection, Mr. Edison will abandon this imperfect burner" The same authors in speaking of the "Sawyer lamp," describe it as "the best incandescent lamp of this kind that has been invented."

Such being the teachings of an educational work, written by professed teachers on this subject, let them be compared with the actual results achieved, and the relative positions of the two men at this moment. Seldom has the irony of events demonstrated more forcibly that the honest work of a man is proof against the assaults of fraudulent or ignorant critics, and that the leveling influence of time always reveals the truth.

On the various attempts to imitate Edison's lamp we shall offer but a few words, for most practical inventors are usually plagued by men who endeavor to duplicate their successful inventions. If "imitation is the sincerest of flattery" we suppose Mr. Maxim is merely desirous of paying Edison a high compliment. Concerning Mr. Swan, of Newcastle, England, who professes to have perfected a horse-shoe carbon lamp, apparently identical to that of Mr. Edison's, we would draw attention to the significant fact, that in Messrs. Urquhart and Webb's work on the "Electric Light," dated as recently as April, 1880, and published in Mr. Swan's own country, not a single reference is made to the Swan electric lamp—in fact, his name does not occur in the book. This would appear to be conclusive evidence that neither Mr. Swan, nor his lamp, were known in England up to that date—unless he is included among the nameless crowd, spoken of by the authors, who had "repeatedly used burnt paper in various forms," and *who failed to secure constant results, even "in the best possible vacuum obtainable."*

CONGRESO INTERNACIONAL DE AMERICANISTAS.

We are in receipt of a pamphlet printed at Madrid, containing the official announcement of the above Congress, and instructions for those desirous of attending it. This is the fourth meeting of an International Congress for the discussion of American Archæology, and will take place on the 25th, 26th, 27th, and 28th of September next.

The object to be attained by this body is to contribute to the progress of Ethnographical, Linguistic and Historical researches relative to the two Americas, especially for the period prior to Christopher Columbus, and to bring together such persons as are interested in such studies.

Among the delegates from the United States we notice the names of Professor Spencer F. Baird, of Washington, Professor R. B. Anderson, of the University of Wisconsin; Professor J. Putnam Duncan, of the Academy of National Sciences, Davenport, Iowa, and Albert S. Gatschet, Esq., of 304 E street, N. W. Washington, D. C., to whose courtesy we are indebted for a copy of these official instructions.

Those desirous of attending this Congress, or of forwarding papers, should put themselves in communication with one of the above named gentlemen. Residents of New York City are welcome to read the prospectus at the office of "Science."

The Spanish railway authorities have consented to reduce the fares of those attending this Congress, and other concessions have been arranged. We direct the attention of those who have read early notices of this Congress to the fact that the first day of meeting has been changed from the 18th to the 22nd of September. This change has been made for the convenience of those who would attend two other International Congresses which meet at about the same time, one at Berlin and another at Venice.

THE UNITY OF NATURE.

By the Duke of Argyll.

VII.

ON THE MORAL CHARACTER OF MAN CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(*Continued*).

It breaks down the presumption that whatever is most savage is therefore probably the most ancient. And then, when we come to think of it, this idea, from being vague and general, rises into suggestions which are definite and specific. On the great fundamental subject of the relation of the sexes, conclusions not less important than those respecting cannibalism and infanticide are forced upon our conviction. We have seen that the cruel treatment of the female sex is almost universal among savages, and that it is entirely unknown among the lower animals. It is in the highest degree improbable and unnatural to suppose that this habit can have been primeval. But the same considerations carry us a great deal farther. They raise a presumption in favor of the latter origin of other habits and customs which are not confined to the savage state, but have prevailed, and do now prevail, among nations comparatively civilized. There can have been no polygamy when as yet there was only a single pair, or when there were several single pairs widely separated from each other. The presumption, if not the certainty, therefore is, that primeval Man must have been monogamous. It is a presumption supported by the general equality of the sexes in respect to the numbers born, with only just such an excess of the male sex as tends to maintain that equality against the greater risks to life arising out of manly pursuits and duties. Thus the facts of Nature point to polygamy as in all probability a departure from the habits of primeval times. Like considerations set aside, as in a still higher degree unnatural and improbable, the primeval rank of other customs of which the historians of human culture tell us, and probably tell us truly, that there are many surviving traces among the existing customs of men. Thus "marriage by capture" cannot have been primeval. It may be very ancient: but it cannot possibly have arisen until the family of Man had so multiplied and scattered, that it had become divided into tribes accustomed to act with violence towards each other. And then as regards a custom still more barbarous and savage, namely, that of polyandry, and that which is now euphemistically called "communal marriage," apart from the strong presumption in favor of primeval monogamy, they are stamped by many separate considerations as corruptions and as departures from primeval habits. In the first place, all such customs are fatally injurious to the propagation of the race. In the second place, they are unknown in the animal world. In the third place, their origin can be assigned, in many cases, if not with certainty at least with the highest probability, to one cause, and that is the previously-acquired habit of female infanticide. But as regards this last habit, besides the certainty that it cannot have been primeval, we know that it has often arisen from customs such as the exorbitant cost of marriage portions, which can only have grown up under long developed and highly artificial conditions of society.

But powerful as all these separate considerations are to raise at least adverse presumptions against the primeval rank of the worst and commonest characteristics of savage life, the force of these considerations is much increased when we find that they are closely connected together, and that they all lead up to the recognition of a principle and a law. That principle is no other than the principle of Development; that law is no other than the law of Evolution. It is a curious misunderstanding of what that law really is, to suppose that it leads only in one direction. It leads in every direction in which there is at work any one of the "potential energies" of Nature. Development is the growth of germs, and according to the nature of the germ so is the nature of the growth. The flowers and fruits which minister to the use of Man have each their own seed, and so have the briars and thorns which choke them. Evil has its germs as well as good, and the evolution of them is accompanied by effects to which it is impossible to assign a limit. Movement is the condition of all being, in moral as well as in material things. Just as one thing leads to another in knowledge and in virtue, so does one thing lead to another in ignorance and vice. Those gradual processes of change which arise out of action and reaction between the external condition and the internal nature of Man have an energy in them of infinite complexity and power. We stand here on the firm ground of observation and experience. In the shortest space of time, far within the limits even of a single life, we are accustomed to see such processes effectual both to elevate and degrade. The weak become weaker and the bad become worse. "To him that hath more is given, and from him that hath not is taken even that which he seemeth to have." And this law, in the region of character and of morals, is but the counterpart of the law which prevails in the physical regions of Nature, where also Development has its double aspect. It cannot bring one organism to the top without sinking another organism to the bottom. That vast variety of natural causes which have been grouped and almost personified under the phrase "Natural Selection," are causes which necessarily include both favorable and unfavorable conditions. Natural Rejection, therefore, is the inseparable correlative of Natural Selection. In the battle of life the the triumph of one individual, or of one species, is the result of causes which bring about the failure of another. But there is this great distinction between the lower animals and man,—that in their case failure involves death and complete extinction, whilst in his case it is compatible with prolonged survival. So far as mere existence is concerned, the almost infinite plasticity and adaptability of his nature enable him to accommodate himself to the hardest lot, and to the most unfavorable conditions. Man is the only animal whose possible distribution is not limited to narrow, or comparatively narrow, areas, in consequence of exclusive dependence upon particular conditions of climate and of productions. Some such conditions of a highly favorable kind may, and indeed must, have governed the selection of his birthplace and of his infancy. But when once born and fairly launched upon his course, it was in his nature to be able to prevail over all or over most of the limitations which are imposed upon the lower animals. But it is this very power of adaptation to unfavorable circumstances which involves of necessity the possibility of his development taking an equally unfavorable direction. If he can rise to any level, so also can he descend to any depth. It is not merely that faculties, for the exercise of which there is no call and no opportunity, remain dormant, but it is also, that if such faculties have already been exercised,

they may and often do become so stunted that nothing but the rudiments remain.

With such immense possibilities of change inherent in the nature of man, we have to consider the great element of Time. Strangely enough, it seems to be very commonly assumed that the establishment of a great antiquity for the human race has some natural, if not some necessary, connection with the theory that primeval Man stood on some level far lower even than any existing savage. And no doubt this connection would be a real one if it were true that during some long series of ages Development had not only been always working, but had always been working upwards. But if it be capable of working, and if it has been actually working, also in the opposite direction, then the element of time in its bearing upon conditions of modern savagery must have had a very different operation. For here it is to be remembered that the savage of the present day is as far removed in time from the common origin of our race as the man who now exhibits the highest type of moral and intellectual culture. Whether that time is represented by six thousand, or ten thousand, or a hundred thousand years, it is the same for both. If therefore the number of years since the origin of Man be taken as a multiplier in the processes of elevation, it must be taken equally as a multiplier in the processes of degradation. Not even on the theory which some hold, that the human species has spread from more than one centre of birth or of creation, can this conclusion be affected. For even on this hypothesis of separate origins, there is no reason whatever to suppose that the races which are now generally civilized are of more recent origin than those which are generally savage. Presumably, therefore, all the ages which have been at work in the development of civilization have been at work equally in the development of savagery. It is not possible in the case of savagery, any more than in the case of civilization, that all those ages have been without effect. Nor is it possible that the changes they have wrought have been all in one direction. The conclusion is, that neither savagery nor civilization, as we now see them, can represent the primeval condition of Man. Both of them are the work of time. Both of them are the product of Evolution.

When, however, this conclusion has been reached, we naturally seek for some understanding--some definite conception—of the circumstances and conditions under which development in Man has taken a wrong direction. No similar explanation is required of the origin of civilization. This is the development of Man's powers in the natural direction. Great interest, indeed, attaches to the steps by which knowledge has been increased, and by which invention has been added to invention. But there is no mystery to be encountered here—no dark or distressing problem to be solved. This kind and direction of development is all according to the constitution and course of things. It is in harmony with all the analogies of Creation. Very different is the sense of painful wonder with which we seek an explanation of the wretched condition of Man in many regions of the globe, and, still more, with which we seek the origin of the cause of all the hideous customs which are everywhere prevalent among savage men, and which often, in their ingenuity of evil, and in the sweep of their destructive force, leave it a wonder that the race survives at all.

There are, however, some considerations, and some facts, on which we may very safely advance at least a few steps towards the explanation we desire. Two great causes of change, two great elements of Development or Evolution, have been specified above—namely, the external conditions and the internal nature of Man. Let us look at them for a little separately, in so far as they can be separated at all.[1]

[1] The argument which follows was urged in a former work on "Primeval Man." It has been here re-written and re-considered with reference to various objections and replies

It is certain that external or physical conditions have a very powerful, and sometimes a very rapid, effect both on the body and on the mind of Man. The operation of this law has been seen and noted even in the midst of the most highly civilized communities. There are kinds of labor which have been found to exert a rapid influence in degrading the human frame, and in deteriorating the human character. So marked has been this effect, that it has commanded the attention of Parliaments, and the course of legislation has been turned aside to meet the dangers it involved. Moreover, our experience in this matter has been very various. Different kinds of employment, involving different kinds of unfavorable influence, have each tended to develop its own kind of mischief, and to establish its own type of degradation, The particular conditions which are unfavorable may be infinitely various. The evils which arise out of the abuses of civilized life can never be identical with the evils to which the earlier races of Mankind may have been exposed. But the power of external conditions in modifying the form, and in molding the character of men, is stamped as a general law of universal application.

In connection with this law, the first great fact which calls for our attention is the actual distribution of Mankind in relation to the physical geography of the globe. That distribution is nearly universal. From the earliest times when civilized men began to explore distant regions, they found everywhere other races of men already established. And this has held true down to the latest acquisitions of discovery. When the New World was discovered by Columbus, he found that it must have been a very old world indeed to the human species. Not only every great continent, but, with rare exceptions, even every habitable island has been found peopled by the genus HOMO. The explorers might find, and in many cases did actually find, everything else in Nature different from the country of their birth. Not a beast, or bird, or plant,—not an insect, or a reptile, or a fish, might be the same as those of which they had any previous knowledge. The whole face of Nature might be new and strange—but always with this one solitary exception, that everywhere Man was compelled to recognize himself—represented, indeed, often by people of strange aspect and of strange speech, but by people nevertheless exhibiting all the unmistakable characters of the human race.

In ancient times, before the birth of physical science, this fact might not appear so singular and exceptional as it really is. Before Man had begun to form any definite conceptions as to his own origin, or as to his place in Nature, it was easy to suppose in some vague way that the inhabitants of distant regions were "Aborigines," or as the Greeks called them "Autocthonoi"—that they were somehow native to the soil, and had sprung from it. But this conception belongs essentially to that stage and time when tradition has been lost, and before reasoning has begun. Those who refuse to accept the Jewish Scriptures as in any sense authoritative, must at least recognize them as the records of a very ancient and a very sublime Cosmogony. That Cosmogony rests upon these four leading ideas—first, that the globe has been brought to its present condition through days of change; secondly, that from a state which can only be described as chaos, it came to be divided into sea, and land, and atmosphere; thirdly, that the lower animals were born first,—Man being the last as he is the highest product of Creation; fourthly, that he appeared first at one place only in the world, and that from one pair has all the earth been overspread.

It is remarkable that in this general outline of events, and especially in the unity of Man's origin, the progress of discovery, and those later speculations which have outrun discovery, are in strict accordance with the tradition recorded by the Jewish Prophets. There are, indeed, some, scientific men who think that different races

of men represent different species—or, at least, that if Man be defined as one species, it is a species which has spread from more than one place of origin. But those who hold to this idea are men who stand outside the general current of scientific thought. The tendency of that thought is more and more to demand unity and simplicity in our conception of the methods of creation, and of the order of events through which the birth of species has been brought about. So strong is the tendency, and so intimately connected is it with the intellectual conceptions on which the modern theory of Development has been founded, that Mr. Darwin himself, and Mr. Wallace, who may be said to be joint-author with him of that theory, both lay it down as a fundamental postulate, that each new organic Form has originated, and could only originate, at one place. This doctrine is by no means a necessity of thought, nor is it a necessary consequence of the theory of Development. It rests mainly on the doctrine of chances, and that doctrine may be wholly inapplicable to events which are governed not by accident but by law. It is, however, a postulate of the particular form of that theory which Mr. Darwin has adopted. It is not always easy to reconcile this postulate with the existing distribution over the globe of animal forms. But it is not absolutely inconsistent with the facts so far as we know them; and it is interesting to observe how universally and tacitly it is assumed in all the current explanations of the history of Creation. On this point, therefore, of the unity of Man's origin, those who bow to the authority of the most ancient and the most venerable of traditions, and those who accept the most imposing and the most popular of modern scientific theories, are found standing on common ground, and accepting the same result.

And when we come to consider a very curious subject, namely, the configuration of the habitable continents of the globe, we find that this configuration stands in a very intelligible relation to the dispersion of Mankind from a single center. If, indeed, we could suppose that the earliest condition of our race was a condition of advanced knowledge in the useful arts, there would be no difficulty to solve. The great oceans of the world are now the easiest highways of travel, and, consequently, of dispersion. The art and the science of navigation has made them so. But we cannot imagine that this art or this science was known to our forefathers of a very early age. Various means of crossing narrow waters, from the use of solid logs of wood to the use of the same logs when hollowed out, and so to the use of canoes and boats, were in all probability among the very earliest of human inventions. But not the less would it have been impossible with these inventions to cross the Atlantic, or the Indian Ocean, or even many of the more limited tracts of sea which now separate so many habitable regions. Some other solution must be found for the problem presented by the fact that the earliest navigators who traversed those seas and oceans have always found the lands on the other side already colonized, and in some cases thickly inhabited by races and nations which had made considerable advances in civilization. Yet, this problem presents no serious difficulty in accepting the unity of the human race, when it is regarded in the light of physical geography. The distribution of the larger tracts of land and sea upon our planet is very singular indeed. Attached to the southern Pole there is no mass of land which stretches so far north as to enter the latitudes which are even moderately temperate. In the centre of the Antarctic Circle there is probably a great continent. But it is a continent where volcanic fires burst here and there through surfaces which are bound in perpetual ice. Round that vast Circle roll the continuous waves of an Ocean vexed by furious storms, and laden with the gigantic wrecks of immeasurable fields and cliffs of ice. In the northern hemisphere, round the Arctic Circle, on the contrary, everything is different. There land-masses begin, which stretch southward without a break through all the temperate and through all the torrid zones on both sides of the Equator. Then, again, all these great continents of the globe, as they extend towards the south, become narrower and narrower, and so tend to become more and more widely separated from each other by vast oceanic spaces. Towards the north, on the contrary, all these continents converge, and at one point, Behring's Straits, they approach so near each other that only a space of some forty miles of sea intervenes between them. The result is, that in the northern hemisphere there is either a continued connection by land, or a connection severed only by comparatively narrow channels, between all the great inhabited continents of the world. The consequences of this as bearing on the dispersion of Mankind are obvious at a glance. If, for example, Man may be supposed to have been born in any part of Western or Central Asia, it is easy to see how his earliest migrations might lead him without serious difficulty into every one of the lands in which his children have been actually found. The Indian peninsula was at his feet. A natural bridge, as it were, would enable him to penetrate the Arabian deserts, and would conduct him by the glorious valley of the Nile into the heart of the continent of Africa. Eastwards he had before him the fertile tracts of China, and beyond the narrow passage of Behring's Straits lay that vast continent which, when rediscovered from the West, was called the New World. Again, beyond the southern spurs of the great Asiatic Continent there lay an archipelago of magnificent islands, with comparatively narrow seas between them, and connected by a continuous chain with the continental islands of Australasia. The sea-faring habits which would spring up among an insular population,—especially in an archipelago where every volcanic cone and every coral reef rising above the waves was rich in the products of a bounteous vegetation,—would soon lead to a rapid development of the arts of navigation. When these were once acquired, there is no difficulty in accounting for the gradual dispersion of the human race among the beautiful islands of the Pacific. Across its comparatively peaceful waters it is not improbable that even rude navigators may have made their way at various times to people the western shores of the continent of America.

It is true indeed that the science of geology teaches us that the distribution of sea and land has been immensely various in different epochs of the unmeasured ages which have been occupied in the formation of our existing world. And it may be urged from this that no argument on the methods of dispersion can be based with safety upon that distribution as it now is. There is not much force, however, in this plea. For it is equally true that the evidence afforded by geology is in favor of the very great antiquity of the principal land-masses, and of the great oceanic hollows which now divide them. The antiquity of these is almost certainly much greater than the antiquity of Man. The fauna and the flora of the principal continents indicate them to have been separated since a period in the development, or in the creation of species, long anterior to any probable estimate of the time of Man's appearance. Even if that appearance dates from the Miocene epoch in geology,—which is an extreme supposition,—no great difference in the problem of the dispersion of our species would arise. Since that time indeed it is certain that great subsidences and elevation of land have taken place. But although these changes have greatly altered the outlines of sea and land along the shores of Europe and America, there is no reason to believe that they could have materially affected, either injuriously or otherwise, the earlier migrations of Mankind.

But although the peculiar physical geography of the globe makes it easy to understand how, from a single centre, it must have been quite possible for a creature with the peculiar powers and faculties of Man to distribute himself, as he has actually been found distributed over

every habitable region of the world, it is most important to observe the very adverse conditions to which, in the course of this distribution, particular portions of the human family must have been, and to which we do now find them actually exposed.

The "New World"—the American continent—is that which presents the most uninterrupted stretch of habitable land from the highest northern to the lowest southern latitude. No part of it was without human inhabitants when the civilized children of the Old World first came upon it, and when, from its mountain tops, they first "stared on the Pacific." On its extreme north there was the Eskimo or Inuit race, maintaining human life under conditions of extremest hardship, even amid the perpetual ice of the Polar regions. On the extreme south—at the opposite extremity of the great American continent—there were the inhabitants of Cape Horn and of the island off it, both of which project their desolate rocks into another of the most inhospitable climates of the world. Let us take this case first—because it is a typical one, and because it happens that we have from a master-hand a description of these people, and a suggestion of the questions which they raise. The natives of Tierra del Fuego are one of the most degraded among the races of mankind. How could they be otherwise? "Their country," says Mr. Darwin, "is a broken mass of wild rocks, lofty hills, and useless forests; and these are viewed through mists and endless storms. The habitable land is reduced to the stones of the beach. In search of food they are compelled to wander unceasingly from spot to spot; and so steep is the coast that they can only move about in their wretched canoes." They are habitual cannibals, killing and eating their old women before they kill their dogs, for the sufficient reason, as explained by themselves, "Doggies catch others: old women, no." Of some of these people who came round the *Beagle* in their canoes the same author says: "These were the most wretched and miserable creatures I anywhere beheld. They were quite naked, and even one full-grown woman was absolutely so. It was raining heavily and the fresh water, together with the spray, trickled down her body. In another harbor not far distant, a woman who was suckling a new-born child, came one day alongside the vessel and there remained out of mere curiosity, whilst the sleet fell and thawed on her naked bosom, and on the skin of her naked baby. These poor wretches were stunted in their growth, their hideous faces bedaubed with white paint, their skins filthy and greasy, their hair entangled, their voices discordant, and their gestures violent. Viewing such men, one can hardly make one's self believe that they are fellow-creatures and inhabitants of the same world." Such are the facts, or one aspect of the facts, connected with this people. But there are other facts, or another aspect of the same facts, not less important which we have on the same evidence. Beneath this crust of savagery lay all the perfect attributes of humanity—ready to be developed the moment the unfavorable conditions of Fuegian life were exchanged for conditions which were different. Captain Fitzroy had, in 1830, carried off some of these poor people to England, where they were taught the arts and the habits of civilization. Of one of those who was taken back to his own country in the *Beagle*, Mr. Darwin tells us that "his intellect was good," and of another that he had a "nice disposition."

Let us look now at the questions which the low condition of the Fuegians suggests to Mr. Darwin. "Whilst beholding these savages, one asks whence have they come? What could have tempted, or what change compelled, a tribe of men to leave the fine region of the North, to travel down the Cordillera or backbone of America, to invent and build canoes which are not used by the tribes of Chili, Peru, and Brazil, and then to enter one of the most inhospitable countries within the limits of the globe?"

These questions of Mr. Darwin, it will be observed, assume that Man is not indigenous in Tierra del Fuego. They assume that he has come from elsewhere into that savage country. They assume farther that his access to it has been by land. They assume that the progenitors of the Fuegians who first came there were not skilled navigators like the crew of the *Beagle*, able to traverse the Atlantic or the Pacific in their widest and stormiest expanse. These assumptions are surely safe. But these being accepted, it follows that the ancestors of the Fuegians must have come from the North, and have passed down the whole length, or a great part of the length, of the American continent. In other words, they must have come from regions which are highly favored into regions of extremest rigor. If external circumstances have any influence upon the condition of Man, this great change cannot have been without effect. Accordingly, Mr. Darwin at once, instinctively as it were, connects the utter savagery of the Fuegians with the wretched conditions of their present home. "How little," he says, "can the higher powers of the mind be brought into play! What is there for imagination to picture, for reason to compare, for judgment to decide upon." It is in perfect accordance with this view that on every side of them, and in proportion as we pass northwards from their wretched country, we find that the tribes of South America are less wretched, and better acquainted with the simpler arts. None of the depressing and stupefying conditions which attach to the present home of the Fuegians can be alleged of the regions in which some distant ancestors of the Fuegians must have lived. In Chili, in Peru, in Brazil, in Mexico, there are boundless tracts in which every condition of nature, soil, climate, and productions, are comparatively as favorable to men as they are unfavorable on the desolate shores of Cape Horn and Tierra del Fuego. Yet one or other of these many well-favored regions must have been on the line of march by which the Fuegian shores were reached. One and all of them present attractions which must have induced a long encampment, and must have made them the home of many generations. Why was that march ever resumed in a direction so uninviting and pursued in a destination so desolate and so miserable?

But the moment we come to ask this question in respect to the Fuegians, we find that it is a question which arises equally out of the position and life of many other portions of the human family. The northern extremity of the American continent presents exactly the same problem as the southern. If it is impossible to suppose that Man was first created, or born, or developed in Tierra del Fuego, it is not less impossible to suppose that he had made his first appearance on the frozen shores of Baffin's Bay. Watching at the blow-hole of a seal for many hours in a temperature 75° below the freezing point, is the constant work of the Inuit hunter. And when at last his prey is struck, it is his greatest luxury to feast upon the raw blood and blubber. To civilized man it is hardly possible to conceive a life so wretched, and in some aspects at least so brutal, as the life led by this race during the continual night of the Arctic winter. Not even the most extravagant theorist as regards the possible plurality of human origins can believe that there was a separate Eskimo Adam. Man, therefore, is as certainly an immigrant into the dreary regions round the Pole as he is an immigrant to the desolations of Cape Horn. But the whole conditions of his life there are necessarily determined by the rigors of the climate. They are conditions in which civilization, as it has been here defined, is impossible. And the importance of that definition is singularly apparent in the case of the Eskimo. Although essentially uncivilized, he is not, in the ordinary sense of the word, a savage. Many of the characteristics usually associated with that word are altogether wanting in the Eskimo. They are a gentle, inoffensive, hospitable, and truthful race. They are therefore a conspicuous example of the fallacy of supposing that there is any necessary connection between a backward condition of knowledge in the useful arts, and violent dispositions, or

ferocious and cruel habits. Men are not necessarily savage because they may use flint hatchets, or because they may point their arrows and their spears with bone. Nevertheless, the condition of the Eskimo, although not savage, is almost the type of the merely uncivilized condition of Mankind. It is a condition in which not more than a few families can ever live together, and in which, therefore, large communities cannot be formed. A few simple and some very curious rules of ownership are all that can represent among them the great law-giving instinct which lives in Man. Agriculture cannot be practiced, nor even the pasturing of flocks and herds. Without fuel, beyond the oil which feeds their feeble lamps, or a few stray logs of drift timber, the Eskimo can have no access to the metals, which in such a country could not be reduced from their ores, even if these ores were themselves obtainable. The useful arts are, therefore, strictly limited to the devising and making of canoes and weapons of the chase. There is no domestic animal except the dog, and dogs too, like their masters, must have been brought from elsewhere. These are all conditions which exclude the first elements of what we understand by civilization. But every one of these conditions must have been different with the progenitors of the Eskimo. If they were immigrants into the regions within the Arctic Circle, they must have come from the more temperate regions of the South. They must have been surrounded there by all the natural advantages of which their descendants are now deprived. To what extent these ancestors of the Eskimo may have profited by their very different and more favored position, we cannot know. They may have practiced such simple agriculture as was practiced by the most ancient races which have left their traces in the Swiss Lake dwellings. They may have been nomads, living on their flocks and herds, as the Laplanders and Siberians actually are who in the Old World live in latitudes only a little farther south. They may have been people who, like the ancient but unknown Mound-builders in the Southern and Western States of America, had developed a comparatively high civilization. But one thing is certain, that they must have lived a life wholly different from the life of the Eskimo, and that they must have had completely different habits. Whatever arts the father knew, suited to more genial climates, could not fail to be forgotten by the children, in a country where the practice of them was impossible.

The same question, therefore, which Darwin asks in respect to the inhabitants of the extreme south of the American continent, arises in respect to the inhabitants of its extreme north—What can have induced any people to travel along that continent in a direction more and more inhospitable, and at last to settle in a country where nearly one-half the year is night, and where, even during the short summer, both sea and land are mainly occupied by ice and snow?

But, again, we are reminded that there are other cases of a similar kind. The African continent does not extend so far south as to reach a severe southern latitude. In that continent, accordingly, beyond the frequent occurrence of deserts, there is nothing seriously to impede the migrations of Man from its northern towards its southern extremity; nor is there anything there to subject them when they had reached it to the worst conditions. Accordingly we do not find that the predominant native races of Southern Africa rank low in the scale of humanity. Those among them, however, which are or were the lowest in that scale, were precisely those who occupied the most favorable portion of the country and are known as Bushmen. Of these it is well ascertained that they are not a distinct race, but of kindred origin with the Hottentots, who were by no means so degraded. On the whole, therefore, the question how men could ever have been induced to live where we actually find them, does not press for an answer so much in respect to any part of the continent of Africa, with the exception of a few tribes whose present habitat is exceptionally unfavorable.

There is, however, another case of difficulty in respect to the distribution of Mankind, which in some respects is even more remarkable than the case of the Fuegians, or the case of the Eskimo. We have seen that the great Asiatic continent, though it does not itself extend beyond latitudes which are favorable to human settlement, is practically prolonged through a continuous chain of islands into the regions of Australasia. Every part of those regions was found to be inhabited when they were discovered by civilized man; and it is universally admitted that the natives of Australia, and the natives of Tasmania, are or were (for the Tasmanians are now extinct) among the very lowest of all the families of Man. Now the physical conditions of the great islands of Australasia are in many respects the most remarkable on the surface of the globe. Their peculiar fauna and flora prove them to be of great antiquity as islands in the geological history of the earth. That is to say—their beasts, and their birds, and their vegetation are so widely separate from those of all other regions, that during long ages of the total time which has elapsed since they first appeared above the ocean, they must have been as separate as they are now from all other habitable lands. Their beasts are, indeed, related—closely related—to forms which have existed during certain epochs in many other portions of the earth's surface. But those epochs are so distant, that we are carried back in our search for creatures like them to the times of the Secondary Rocks—to the horizon of the Oolite. Speaking of the poverty and of the extremely isolated character of the Australian Mammalia, Mr. Wallace says: "This class affords us the most certain proofs that no part of the country has been united to the Asiatic continent since the latter part of the Mezozoic period of geology."[2] Of the vast series of creatures which elsewhere have been created, or born, or developed, since that epoch, including all the higher members of the Mammalian Class, not one existed in Australasia until they were introduced by Europeans. Among the grasses there were none which by cultivation could be developed into cereals. Among the beasts there was not one which was capable of domestication. There were no apes or monkeys; no oxen, antelopes, or deer; no elephants, rhinoceroses, or pigs; no cats, wolves or bears; none even of the smaller civits or weasels: no hedgehogs or shrews; no hares, squirrels, or porcupines, or dormice."[3] There was not even a native dog; and the only approach to, or representative of, that wonderful animal, was a low, marsupial beast, which is a mere biting machine, incapable of affection for a master, and incapable even of recognizing the hand that feeds it. In the whole of Australia, with the exception of a few mice, there was not one single mammal which did not belong to this low Marsupial Class, whilst some others belonged to a class still lower in the scale of organization, the class called Monotremata. Strange forms astonished our first explorers, such as the Ornithorynchus and the Echidna—forms which combined features elsewhere widely separated in the animal kingdom—the bills of Birds, the spines of Porcupines, the fur of Otters, and the feet of Moles. Nothing analogous to these relics of an extinct fauna had been known to survive in any other part of the world. Yet in the midst of this strange assemblage of creatures, without any representative of the animals which elsewhere surround him, the familiar form of Man appeared, low, indeed in his condition, but with all the inalienable characteristics of his race. It is true, that everywhere the gap which separates Man from the lower animals is enormous. Nothing bridges, or comes near to bridging it. It is a gap which has been well

[2] "Australasia," by Alfred R. Wallace, p. 51.

[3] "Australasia," by Alfred R. Wallace, p. 51.

called a gulf. But in Australasia the breadth and depth of this gulf is rendered more conspicuous by the association of Man with a series of animals absolutely wanting in those higher members of the Mammalian Class which elsewhere minister to his wants, and the use of which is among the first elements of a civilized condition. Alone everywhere, and separate from other beings, Man is most conspicuously alone in those strange and distant lands where his high organization is in contact with nothing nearer to itself than the low marsupial brain.

To those who connect the origin of Man with the theory of Development or Evolution, in any shape or in any form, these peculiar circumstances respecting the fauna of Australasia indicate beyond all doubt that Man is not there indigenous. They stamp him as an immigrant in those regions—a wanderer from other lands. Nor will this conclusion be less assuredly held by those who believe that in some special sense Man has been created. There is something more than an incongruity in supposing that there was a separate Tasmanian Adam. The belief that the creation of Man has been a special work is not inconsistent with the belief that in the time, and in the circumstances, and in the method of this work, it had a definite relation to the previous course and history of Creation—so that Man did not appear until all these lower animals had been born, which were destined to minister to his necessities, and to afford him the means and opportunities for that kind of development which is peculiarly his own. On the contrary, this doctrine of the previous creation of the lower animals, which is, perhaps, more firmly established on the facts of science than any other respecting the origin of Man, is a doctrine fitting closely into the fundamental conceptions which inspire the belief that Man has been produced by operations as exceptional as their result. And so it is, that when we see men inhabiting lands destitute of all the higher Mammalia, which are elsewhere his servants or companions—destitute even of those productions of the vegetable kingdom, which alone repay the cultivation of the soil, we conclude with certainty that he is there a wanderer from some distant lands, where the work of creation had been carried farther, and where the conditions of surrounding Nature were such as to afford him the conditions of a home.

We see, then, that the question asked by Mr. Darwin, in respect to the Fuegians, is a question arising equally in respect to all the races who inhabit regions of the globe, which from any cause present conditions highly unfavorable to Man. Just as Mr. Darwin asked, what could have induced tribes to travel down the American continent to a climate so rigorous as Cape Horn?—just as we have asked, on the same principle, what could have induced men to travel along the same continent in an opposite direction till they reached and settled within the Arctic Circle?—so now we have to ask, what could have induced men to travel from Asia, or from the rich and splendid islands of the Eastern Archipelago, and to take up their abode in Australasia?

In every one of these cases the change has been greatly for the worse. It has been a change not only involving comparative disadvantages, but positive disabilities—affecting the fundamental elements of civilization, and subjecting those who underwent that change to deteriorating influences of the most powerful kind.

It follows from these considerations as a necessary consequence that the present condition of the Australian, or the recent condition of the Tasmanian, cannot possibly be any trustworthy indication of the condition of their ancestors, when they lived in more favored regions. The same argument applies to them which, as we have seen, applies to the Fuegians and the Eskimo. If all these families of Mankind are the descendants of men, who at some former time inhabited countries wholly different in climate, and in productions, and in all the facilities which these afford for the development of the special faculties of the race, it is in the highest degree improbable that a change of habitat so great should have been without a corresponding effect upon those over whom it passed. Nor is it a matter of doubt or mere speculation that this effect must have been in the highest degree unfavorable. The conclusion, therefore, to which we are led is, that such races as those which inhabit Australasia, are indeed the results of development, or of evolution—but of the development of unfavorable conditions, and of the evolution of the natural effects of these. Instead of assuming them to be the nearest living representative of primeval Man we should be more safe in assuming them to represent the widest departure from that earliest condition of our race which, on the theory of Development, must of necessity have been associated at first with the most highly favorable conditions or external Nature.

DOLBEAR ON THE NATURE AND CONSTITUTION OF MATTER.

A CRITIQUE.

There appeared in "SCIENCE" a series of three papers[1] by Professor A. E. Dolbear which contain such new and somewhat startling ideas on the nature and constitution of matter that an interesting controversy was to be expected. Nearly six months have, however, passed without any objections having been raised to any of the Professor's statements, some of which seem to me quite strange and of rather peculiar mathematics withal. I now, with no little hesitation enter a protest against some of these statements. The subject of the constitution of matter is so intricate, so complicated, beset with so many difficulties on the one hand, while on the other our means of dealing with it are so inadequate, our methods of investigation so imperfect that, as Maxwell says, all we can do is to make hypotheses and see how far our facts and phenomena bear them out. This being so, I believe that whenever a particularly bold hypothesis is made and conclusions are drawn therefrom by anyone without having made a most careful comparison with all the principal phenomena of matter, the humblest student of this fascinating department of physical science has a right to command a most vigorous halt, and to examine whether he who assumes to guide is himself sufficiently acquainted with the intricacies and windings of the road not to lead his followers into the dismal swamps of metaphysical vagaries. I therefore claim for myself that right, lest what I have to say might be construed as too presumptuous.

In my review I shall, in the main, touch upon and discuss the points I desire to examine, in the order in which they occur in the Professor's papers. To begin, then, with the first paper, Section II, I shall devote a little attention to the equation $E' = \varepsilon \frac{mv^2}{2}$ which the Professor says expresses the total energy of an atom. It seems an altogether gratuitous assumption to give to the expression for the total energy of an atom the same form that Clausius gives for the total energy of a molecule. In the molecule we have the motion of translation and also the motion or motions of its parts relative to its centre of mass; but of the atom we cannot make the same assertion. Clausius was justified, by mathematical deductions from experimental data, to assume that the total energy of the molecule is proportional to the energy of agitation; but that does by no means justify the assumption that the same form of function also expresses the total energy of the atom, for here all experimental data are wanting. We may, however, reasonably conclude that the form of this function for the atom must differ somewhat from that for the molecule, as the motions of the atom must, of necessity, be much more intricate and complex

[1] "On Some Needed Changes and Additions to Physical Nomenclature," Vol. I., p. 238; "On Matter as a Form of Energy," Vol. II., p. 49, and "On the Amplitude of Vibration of Atoms," Vol. II., p. 146.

than those of the molecule. Granting the correctness of the expression for argument's, sake I must confess that I do not understand how the Professor gets the expression $E' - E = \varepsilon$ given under 3, in his "Table of Forms of Energy." If ε in the expression $E' = \varepsilon \frac{m v^2}{2}$ is anything it certainly must be the ratio $\frac{E'}{E}$ where $E = \frac{m v^2}{2}$ is the energy of agitation of an atom. By subtraction we obtain $E' - E = \varepsilon \frac{m v^2}{2} - \frac{m v^2}{2} = \frac{m v^2}{2} (\varepsilon - 1)$ and not ε as the Professor would lead us to believe. While I regard it simply a gratuitous assumption to give the expression for the total energy of an atom, and that for the total energy of a molecule the same form—because we have no experimental evidence whatever to justify us to believe that the conditions of the atom resemble those of the molecule—I believe that the equation $E' = \varepsilon \frac{m v^2}{2}$ in which ε is *internal* energy is utterly incorrect. ε in this expression is not at all analogous to β in $\frac{1}{2} \beta m v^2$ the expression for the total energy of a molecule as given by Maxwell. Here β is the numerical ratio of the total energy to the energy of agitation, an abstract, while ε is internal energy, a concrete. Here let me ask what is energy times energy. The form $E' - E = \varepsilon$ is undoubtedly correct. From this by substitution we get $E' = \frac{m v^2}{2} + \varepsilon$ and not $\varepsilon \frac{m v^2}{2}$.

The statement "Latent heat, specific heat, and specific inductive capacity, are all involved in (that factor?) ε," is certainly not correct. Latent heat is work performed upon some body, and is, according to Clausius, partly internal and partly external. The external work is performed upon surrounding material systems. The internal work is, in general, composed of two parts—one expended upon the molecules in expanding the body from one state of aggregation to another, the other part is expended upon the parts of the molecule. It is only this last portion which can affect the atom as such, and which can in any way be involved in ε. Similarly we find that specific heat is also work performed, and that, too, of a complex nature. Specific inductive capacity seems to me to belong to an altogether different class of phenomena.

In regard to the ether the Professor makes some very curious statements. He says that he knows nothing of the specific properties of the ether, yet in the same sentence is the statement "ether is not matter," as if this were a generally accepted view. If the ether is not matter, what is it? There are two ways of looking at matter—the subjective or metaphysical, and the objective or physical. Metaphysically defined matter is anything which has extension or occupies space. For the physical definition I quote Maxwell[2]: "Hence, as we have said, we are acquainted with matter only as that which may have energy communicated to it from other matter, and which may, in its turn, communicate energy to other matter." Again, he says: "Energy cannot exist except in connection with matter." Whether, then, we accept the metaphysician's definition or the physicist's, we must regard ether as matter; for it certainly has extension and occupies space, and it certainly receives from other matter, transmits and imparts to other matter energy. That Maxwell regarded ether as matter, appears from the following quotation, taken from the same work and page as the preceding: "Hence, . . . we conclude that the *matter* which transmits light is disseminated through the whole of the visible universe." The italics are mine. Professor Dolbear, furthermore, tacitly assumes ether to have mass, as will appear hereafter.

Again, the Professor says: "Furthermore, as atoms differ in mass so will their rates of vibration differ when they possess the same absolute amount of energy. Velocity, in this case, will be equal to amplitude $a\,b$, the space point c passes over during one vibration. If m and m' be two atoms of different masses having equal energy of vibration, then $E = \frac{m v^2}{2} = \frac{m' v'^2}{2}$ and $\frac{m}{m'} = \frac{v'^2}{v^2}$ that is the square of their velocities is inversely as their masses, so that wave-length in the ether will vary as the mass of the atom." This is certainly very curious logic and mathematics. The statement may be true, and the investigations of Lecoq de Boisbaudran even furnish some evidence in its favor, but the mathematical proof offered by the Professor does not justify any such conclusion. v and v' are, according to his own statement, amplitudes of vibration; when, then, the atoms of different masses have equal energy, the proportion $\frac{m}{m'} = \frac{v'^2}{v^2}$ simply proves that the squares of the amplitudes of vibration are inversely as the masses. In what manner the rate of vibration and wave-length in ether follows from this relation of mass to amplitude the Professor does not make clear. In order to make the above conclusion of Professor Dolbear correct, we must have the further condition, $\frac{v'^2}{v^2} = \frac{n'}{n}$ where n and n' are the relative number of vibrations of m and m' in equal times. One of the most fundamental equations of motion is unquestionably $v = \frac{s}{t}$. Hence, as the amplitude $a\,b$ is a space passed over in a given time, we can make it equal to v only by making t unity. Similarly we can make the amplitude of m' equal to v' only by making t unity. If now we wish to compare the velocities and masses of the two atoms we can certainly not use different units of time to determine those velocities; and we get, according to the Professor's statement, the self-contradictory result that two atoms, which make each one vibration in equal times yet have different rates of vibration. To make the problem more general let us take two atoms of masses m and m'. Let them make respectively n and n' vibrations of amplitudes, a and a' in unit of time. The time of one vibration of m will be $\frac{1}{n}$ and of m', $\frac{1}{n'}$. Substituting these values successively for t, and a and a' successively for s in the equation of motion, we have $v = \frac{a}{\frac{1}{n}} = a n$ and $v' = \frac{a'}{\frac{1}{n'}} = a' n'$ combining $\frac{v}{v'} = \frac{a n}{a' n'}$, or the velocities are proportional to the products of the amplitudes by the number of vibrations in unit time. Combining this with the Professor's proportion we have $\frac{m}{m'} = \frac{a'^2 n'^2}{a^2 n^2}$.

To obtain from this the relation $\frac{m}{m'} = \frac{\lambda}{\lambda'}$, λ and λ' being wave-lengths, we must fulfil the condition $\frac{a'^2 n'^2}{a^2 n^2} = \frac{n'}{n}$ or $\frac{n}{n'} = \frac{a'^2}{a^2}$. If, then, two atoms of the masses m and m' have equal energy, and the relation $\frac{n}{n'} = \frac{a'^2}{a^2}$ holds n and n', being the respective number of vibrations in unit time, and a and a' corresponding amplitudes, the relation $\frac{\lambda}{\lambda'} = \frac{m}{m'}$ in which λ and λ' are wave-lengths will follow. For we will then have, as above shown, $\frac{m}{m'} = \frac{n'}{n}$. We also have $\lambda = \frac{v}{n}$ and $\lambda' = \frac{v}{n'}$. From these we obtain $\frac{\lambda}{\lambda'} = \frac{n'}{n}$ and, hence, $\frac{m}{m} = \frac{\lambda}{\lambda'}$.

[2] "Matter and Motion," p. 93.

Whether or not the relation $\frac{n}{n'} = \frac{a'^2}{a^2}$ holds in any particular case can, it would seem, be determined only by experiment. So, too, the fact of the equal absolute energy of vibration of two atoms. Our experimental methods are, however, as yet far from competent to deal with either question, and until they are it is certainly premature to build up speculative hypotheses.

Every student of molecular science knows how great is the temptation to build hypotheses which are to account for all the physical and chemical relations of matter. We can read between the lines of nearly all our recent writers in this department of science their secret belief that chemical phenomena are probably but a complex phase of mechanical phenomena, and that all matter is probably one. Nor are facts justifying such views altogether wanting. Probably no chemist would be bold enough to say in how far such phenomena as, for instance, the solution of ammonia, carbon dioxide, and many other gases in water are of purely chemical and how far of purely physical nature. There are many other phenomena in which similar difficulty would be felt. The phenomena of adhesion and cohesion are such that it does not require a very great stretch of the imagination to suppose that they may be but different phases of what we call chemical union. But to pass from such general and indefinite speculations to suppositions in regard to the mechanical conditions which will account for all these phenomena and all the properties of matter upon purely mechanical principles is a long and, indeed, a bold stride. As the temptation to make this attempt is great, so ought our caution to be great in making the attempt. Professor Dolbear's immediate predecessor in this attempt is Professor Norton. His hypothesis of two atmospheres, one attractive, the other repellant, surrounding each atom, is too artificial, and in being in opposition to the "Kinetic Theory of Gases," is probably too much out of sympathy with the tendency of modern thought to make many converts. Not so, however, with Professor Dolbear's speculations. Their great fundamental simplicity, as well as their thoroughly Kinetic nature, make them dangerous to healthy progress in molecular science unless they can maintain their right of being by accounting for at least the chief and fundamental phenomena of matter. I shall now attempt to apply the touch-stone to them. In Section IV. of his first paper Professor Dolbear advances an hypothesis of chemical union founded on the analogy to a vibrating body which, as is well known by reducing the average density of the atmosphere, causes light bodies to cling to it by atmospheric pressure. We are told that precisely the same conditions exist in the ether near a vibrating atom; that the average density of the surrounding ether is lessened, and that by extraneous pressure another atom vibrating synchronously with the first would attach itself thereto, and the molecule would be formed, etc., etc. I would like to ask how Prof. Dolbear can consistently speak of the density of ether, which, he says, is not matter. Now, in this idea of density there is implicitly the idea of mass, for density, as every one knows, is the mass or amount of matter in unit volume. But, disregarding this inconsistency, it is certainly very bold induction, if induction it can be called, to attribute chemical union to a lessening of density of ether due to atomic vibrations because a vibrating tuning-fork attracts light bodies when brought sufficiently near. In the professor's hypothesis the atoms (vortex-rings) vibrate about a circle as figure of equilibrium, and consequently have four points of maximum displacement or minimum density of the ether. As a consequence of this, each atom must attract other atoms capable of attaching themselves to it at four points. To judge from his diagrams, the Professor believes that atoms unite only in two-dimensional space, *i.e.*, that the centres of all the atoms lie in the same plane. Such a distribution of the atoms would render any closed structure such as a saturated molecule an impossibility, for the peripheral atoms would constantly attract further atoms as long as they vibrate, and other atoms vibrating synchronically with them are present. If, on the other hand, the atoms are arranged in tri-dimensional space, having their centres in planes, say, at right angles to one another, the simplest molecule and the only really stable one would have to contain six atoms whose planes of rotation form the faces of a cube. A further possible supposition is that the atoms would arrange themselves in parallel planes with their centres in a line at right angles to these planes. The first of these suppositions, as already indicated, would not allow the formation of saturated molecules, and it would seem that all chemical union, as we know it, could not exist, for it would evidently be altogether a matter of chance how atoms grouped themselves in regard to numbers, so that we could not always obtain like results of union under precisely like conditions. The second supposition is also inconsistent with chemical facts, for we have molecules of two, three, four and five atoms, as well as others containing hundreds. The third supposition is also untenable, for from Helmholtz's mathematical investigations and Tait's experiments we know that two vortex-rings, when they move axially in the same direction alternately, pass through each other one expanding, the other contracting, while when moving axially in opposite directions they both expand moving slower and slower, but never meet. This is, according to Tait, about all we know experimentally or mathematically in regard to the action of one vortex ring upon another. It is certainly a little strange that Prof. Dolbear, in framing his hypothesis, completely ignores these known facts, and relies on a far-fetched analogy. Serious as are these difficulties, they are by no means the most serious. If experimental evidence is worth anything, we must believe that elementary molecules, with a few exceptions, consist of two atoms, which are, as far as we can judge, exactly alike. Furthermore, we find that in all chemical reactions we can deal with nothing less than the molecule; we know and can deal with the atom only as a part of a molecule, and not as an independent existence. When chemical union takes place between two elements, there is simply an interchange of atoms between the molecules. The difference between the molecules of an element, and those of a compound, is simply this, that the atoms of elementary molecules are all alike, while those of a compound molecule are unlike. I repeat all these fundamental and well-known chemical facts and deductions, to show how singularly inadequate Prof. Dolbear's hypothesis is to account for even the most simple chemical facts. According to his hypothesis, the atoms whose rates of vibration are most exactly alike, must form the most stable molecules. Consequently, the atoms of an element must cling more firmly together than can those of two different elements, and chemical union between the elements becomes impossible. Did the atoms of elements exist as individuals, and not as parts of molecules simply, synchronism of vibrations might be a possible supposition to account for chemical union; but as the case stands, we must reject any such hypothesis as precluding all combination between atoms of different elements. Setting aside even this difficulty, how are we to account by synchronous vibrations for the liberation of energy in the form of heat and light, which accompanies most chemical unions. These forms of energy are, according to the Professor himself, altogether due to vibrations of the atoms and these same vibrations cause the union. Now, how can they both cause the union and be produced by it? Does this not look a little like *perpetuum mobile?* Had the Professor tried to explain adhesion and cohesion by molecular vibration his position would undoubtedly be much stronger. We know that molecules are complex and that there must be motion of their parts relative to the centre of mass of

the molecule. As there is no good reason for supposing the motions of these parts or atoms to be rather in one plane than another, we must admit the possibility of motion in all planes. The vibrations would, however, probably be in three planes at right angles to one another in all molecules of more than three atoms; and would, consequently, have six points of maximum displacement and minimum density of the surrounding ether. Molecules of two and three atoms might possibly vibrate in two or only one plane. As molecules are not vortex-rings, though possibly groups of vortex-rings, the analogy to a vibrating tuning fork becomes much closer than in the case of a vibrating vortex-ring, and we are much more justified in trying to make application of the hypothesis. Prof. Dolbear's analogy thus modified can, I think, be made a very fair working hypothesis to explain adhesion, cohesion and even crystallization. The phenomena of surface tension of liquids and capillary action find a reasonably fair explanation upon this hypothesis, and possibly also those of osmosis, dialysis and occlusion. But even here such an hypothesis meets with many difficulties and we must exercise extreme caution, and must gather further experimental evidence before committing ourselves to its acceptance.

In his second paper the Professor tells us that the vortex-ring theory assumes that matter is a *form of energy*, etc. Never having been so fortunate as to have had access to Sir William Thomson's original memoir, I know his celebrated hypothesis only through interpretations of others. From these interpretations I have always supposed that this hypothesis assumes that all matter is essentially one; and that the elements, as we know them, are portions of this common matter imbued with vortex-motion, thus forming vortex-rings variously knotted, whose energy is non-interchangeable with other forms of energy provided the vortex-rings are formed and exist in a perfect or frictionless fluid. If the fluid is not quite perfect, not quite frictionless, the vortex-rings must gradually be destroyed and their energy must be transformed. The uniform material substratum, if I understand the hypothesis correctly, consists of smaller and simpler vortex-rings which are also the particles or atoms of the ether. If, then, I comprehend the positions, the non-transformability of the energy of the vortex atoms and also their permanence, *i. e.* the persistence of our elements depend upon the perfect fluidity of the ether. Whether the ether is perfectly frictionless or not science is, I think, hardly ready to answer. To call "*matter a form of energy* not interchangeable with other variable forms" is, under the circumstances and from the meaning of the terms employed, to take extraordinary liberties with language. Physically regarded, energy is, to strip the term of all technicalities, matter in motion. Then Professor Dolbear's statement becomes matter, is a form of matter in motion, which is hardly intelligible. Again we are told "The energy of a mass of matter varies as the square of the velocities, but the *properties* of the mass vary with the form of the energy, that is to say the physical properties of a heated body are not identical with those of the same body when it is cool, but possesses the same amount of energy in free path motion." Exactly what this sentence means is, I must confess, beyond my comprehension. One thing, however, seems certain, that it expresses an idea directly opposed to the "Mechanical Theory of Heat" and the "Kinetic Theory of Gases" in the statement that a cool body "possesses the same amount of energy in free path motion" as the same body when heated. If this be so, what becomes of $\frac{vp}{\tau} = \frac{v'p'}{\tau'}$ for gases, and what of the "Thermo-dynamic Scale of Temperature."

In regard to the assumption $\frac{mv^2}{2}$ = atomic weight and the calculations based thereon, I will merely remark that if the groups having the same m or those having the same v showed any family likeness or any gradual variation of properties as do Mendelejeff's periods and groups, then they would be worthy of consideration. As it is, however, they seem mere jugglery with figures. That the atoms of the elements have a "common form differing arithmetically from each other in size and velocity" is utterly inconsistent with the well-known facts and phenomena of quantivalence or valency of atoms. There would have to be two forms at least one for artiad, and one for perissad atoms. I think for the present, at least, we must reject this idea of simplicity and still follow Sir William Thomson.

In the third paper we read, "There is now sufficient evidence for the belief that the Kinetic energy of atoms and molecules consists of two parts, one of which is the energy of translation or free path, the other of a change of form due to vibrations of the parts of the atom or molecule toward or away from its centre of mass. The pressure of a gas is immediately due to the former while the temperature depends solely upon the latter." To the first sentence of this quotation I object, because atoms and molecules are treated as if similar, for which assumption we have no evidence. The second sentence contains the very strange idea that the temperature of a gas is due only to the internal energy of the molecule. Maxwell in his "Theory of Heat" Chap. XXII, under "Specific Heat at Constant Volume" says: "Since the product pv is proportional to the absolute temperature, the energy is proportional to the temperature." By energy Maxwell here means, as appears from the context, what Prof. Dolbear would call total energy. From this it appears that Prof. Dolbear's statement can hardly be correct. If we remember that Maxwell speaks of molecules and Prof. Dolbear of atoms the latter's statement becomes still more doubtful. The assumption that "these two forms of energy must indeed be equal to each other in a gas under uniform conditions," upon which all the Professor's calculations in his third paper are based, can easily be disproved. The Kinetic energy of agitation of a molecule is $\frac{1}{2}\ mv^2$ and the (total) energy is "$\frac{1}{2}\ \beta\ mv^2$ where β is a factor always greater than unity and probably equal to 1.634 for air and several of the more perfect gases." Hence the internal energy is $\frac{1}{2}$ (.634 mv^2.) This, of course, invalidates all the Professor's calculations.

Having extended my remarks far beyond what I originally intended, I shall touch upon only one more point, though I find various other difficulties in the Professor's speculations. The last paragraph of the third paper begins: "As at absolute zero each atom is quite independent of every other atom, that is, matter has not a molecular structure, etc." Now, I would like to ask the Professor how he knows this. Such a state of affairs would indeed make the absolute zero a more than singular point in the curve of the properties of matter.

BUFFALO, N. Y., *April* 20, 1881. WM. H. DOPP.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

INTRA-MERCURIAL PLANETS.

To the Editor of "SCIENCE:"

I wish to say that in the sketch given to "SCIENCE," No. 35, p. 95, the position of Professor Swift's Vulcans is very nearly as they were put down by Professor Swift himself on a map that now hangs in my room at the Naval Observatory.

As to negative evidence there is something to be said on both sides of the question. When extraordinary discoveries are reported they are to be severely examined and carefully criticised. If the observations on which

the discoveries rest are conflicting among themselves, and if the probability of such discoveries is rendered small by long and careful series of independent observations, we are justified in waiting for further evidence before we accept the alleged discoveries as true. The ways in which an observer may be deceived are numerous. In 1878 an astronomer wrote me that he had discovered a satellite of Venus that revolved around the planet in thirty seconds. I expressed some doubt and advised him to examine his telescope and the eye pieces. He did so and was candid enough to inform me that the satellite he had discovered was nothing but a "ghost."

If any astronomer who is familar with astronomical observations and their discussion, will examine the reports on the Vulcans supposed to have been discovered during the eclipse of 1878, and will notice how the reports were changed from time to time, he will find good reasons for doubt. Certainly this matter is not to be settled by assertion. If there are Vulcans of the fourth and fifth magnitudes which attain an angular distance of from three to seven degrees from the sun, they ought to be found easily. A. HALL.

Washington, April 25, 1881.

THE SOLAR PARALLAX.

To the Editor of "SCIENCE":

From the American photographs of the Transit of Venus, as presented in part the first of "Observations of the Transit of Venus, December 8, 9, 1874, Made and Reduced Under the Direction of the Commission Created by Congress," I have obtained, for the value of the solar parallax, $8.883'' \pm 0.034''$ corresponding to a distance between the centres of the sun and the earth equal to 92,028,000 miles. D. P. TODD.

WASHINGTON, *April* 26, 1881.

ASTRONOMY.

MOUNT ETNA OBSESVATORY.—The *Memoirs* of the Italian Spectroscope Society contains an illustration of the Observatory of Mount Etna, showing that work upon the building has progressed as rapidly as could have been expected, when we consider the difficulties to be overcome in the transportation of materials, etc. Every effort is being made to finish the Observatory by 1882, and provide it with a director and staff both of astronomers and meteorologists. W. C. W.

Messrs. Houzeau and Lancaster, the Director and Librarian of the Bruxelles Observatory, are performing an extremely valuable service to astronomers by the preparation of a general bibliography of Astronomy. Two volumes have thus far appeared, the second of which is just published, and is devoted to memoirs which have appeared in scientific periodicals, and in the publications of the various academies. Four topics are included in this volume, Spherical Astronomy, Theoretical Astronomy, Celestial Mechanics, and Physical Astronomy. The only thing which even approximates the completeness of the present work, is the catalogue of the library of the Poulkova Observatory, a new edition of which has been in course of preparation for several years past. The Bruxelles work, however, has the advantage of being a general bibliography, and not limited to the contents of any one library, however extensive. O. S.

MICROSCOPICAL NOTES.

At our suggestion, Mr. Lockwood, of New York City, who has already devoted considerable attention to the application of Photography to the various branches of science, now proposes to make arrangements for photographing Microscopical Preparations.

The objects will be enlarged by very perfect and powerful objectives, and photographed while thus enlarged.

Those possessing microscopes will at once notice the great advantage to be secured by such an arrangement. Few possess the skill to produce a drawing from a microscopic object, while the amount of detail involved in sketching anatomical preparations, can be mastered by few who are not professed artists.

When Mr. Lockwood's arrangements are complete a microscopist, for a moderate amount, will be enabled to have a perfect copy of any microscopic preparation, and as many duplicate as he requires to circulate among specialists, or his friends. Should he desire to publish the result of his researches, Mr. Lockwood can then photograph the object directly on the wood block, ready for the hand of the engraver.

The chief value of the use of Photography in such a case lies in the fact that such drawings, being prepared by the hand of nature, their integrity cannot be impeached, and that any charge of exaggeration or error cannot be maintained.

When Mr. Lockwood's arrangements are complete we will announce the fact in our microscopical column, but in the interval would be glad to hear from those who are likely to avail themselves of these facilities for promoting microscopical research.

NOTES.

Les Mondes proposes to apply the photophone to the study of the *aurora borealis*.

ON THE GALVANIC POLARIZATION PRODUCED BY METALLIC DEPOSITS.—The polarization of copper, employed as negative electrode in a solution of sulphate of zinc, is never null, as Lipmann believes, in cases where the solution contains traces of a salt of copper, and that the deposit of zinc is exceedingly slight and invisible. On the contrary, it has a value which may differ much, and which is so much the greater the smaller the quantity of a copper-salt contained in the solution, and the less the time which has passed from the moment when the polarizing current was interrupted.—D. MACALUSO.

ON THE ELECTROMOTIVE FORCE OF VOLTAIC ARC.—When an electric flux is established between two conductors of the same nature by means of a gaseous medium, which is commonly the vapor thrown off by their substance, the inequality of temperature of those portions of the conductors which are contiguous to such a medium appears to be a general fact. It seems not less probable that the extremity by which the positive electricity arrives, possesses the higher temperature. This is observed in a remarkable degree in the production of the voltaic arc between two carbons, by means of a current of constant direction. The idea of ascribing to this phenomenon a thermo-electric origin is not novel. According to the application of the principle of the equivalence of heat to electric phenomena, an electromotive force acting in the inverse direction of the current, corresponds to a disengagement of heat at the point of junction of two heterogeneous substances.—M. F. P. LE ROUX.

MAGNETIC ACTION UPON THE FLUORESCENT LIGHT PRODUCED BY THE NEGATIVE DISCHARGE IN AN EXHAUSTED SPACE.—If we take a well-exhausted cylindrical tube, with rectilinear electrodes placed in its axis, the fluorescent light formed by the cathodic rays consists, as is well known, of a green cylinder bounded by a circle. This circle undergoes transpositions if a magnet is allowed to act upon the discharge. It can be shown that these, whether simple or complicated cases, may be explained by the following hypothesis:—The cathodic rays, emanating from the negative electrode, pass on in a straight direction, and the current moves from the anode to the sides of the cathodic space, and from thence to the negative electrode. The magnet acts upon these currents according to Ampère's rule.—K. DOMALIP.

BOOKS RECEIVED.

COMPENDIUM OF MICROSCOPICAL TECHNOLOGY; A guide to Physicians and Students in the use of the Microscope, and in the preparation of Histological and Pathological specimens. By CARL SEILER, M. D. Published by D. G. Brinton, Philadelphia, 1881.

The author of this work has a high reputation for preparing mounted specimens for Microscopical study, and therein gives short and clear descriptions of his own methods, which have given such satisfactory results. The reader is, therefore, not perplexed by being instructed in the various methods suggested by many authorities, but a clear line of conduct is indicated for him by Dr. Seiler, which may be relied on as being satisfactory.

The work is written for medical students, and for that reason the usual subject matter found in Manuals of Microscopy is altogether omitted, neither are descriptions given of tissues, and the student is referred for histological details to works devoted to histology.

Without intending to cast any reflection on the body of the work, we are inclined to consider the appendix the most valuable part of Dr. Seiler's book. In it the author presents a short, concise, and, at the same time, comprehensive classification of the more common tumors and other neoplasms in tabular form; these, indeed, will be welcome to the student of pathological histology. The author claims to have exercised great care in its compilation, and to have introduced all the accepted modern views on the subject, so as to bring it up to the standard of the present time.

POPULAR LECTURES ON SCIENTIFIC SUBJECTS. By H. HELMHOLTZ, Professor of Physics in the University of Berlin. Translated by E. ATKINSON, Ph. D., F. C. S. Second Series. D. Appleton & Co. New York, 1881.

The present volume presents a series of addresses and lectures delivered by Professor Helmholtz during a period of six years, from 1871 to 1877. The contents show that the following subjects are treated:

1. An address delivered before the Leibnitz meeting of the Academy of Sciences, 1870. In memory of Gustav Magnus.

2. A lecture on the Origin and Significance of Geometrical Axioms, delivered at Heidelberg, 1870.

3. The substance of a series of lectures on the relation of optics to painting, delivered at Cologne, Berlin and Bonn.

4. Lecture on the Origin of the Planetary System, delivered in 1871.

5. An address delivered in 1877, on the Anniversary of the foundation of the Institute for the Education of Army Surgeons: or, Thought in Medicine.

Perhaps the only popular paper in the series is that "On the Origin of the Planetary System," in which the various hypotheses connected with the subject are explained in simple and familiar language. Professor Helmholtz appears to have handled this subject in a manner which must have been a source of delight to a mixed audience. Touching on extinct suns he explained that a time would arrive when our own sun would cease to develop the heat which is a source of vitality to this earth. But he explained that 17,000,000 of years would lapse before this "intensity of sunshine, would be diminished, and that circumstances may even prolong this period."

Looking forward to such a period when our sun shall be extinguished, Professor Holmholtz observes that considering the wonderful adaptability to the conditions of life which all organisms possess, who knows to what degree of perfection our posterity will have developed in 17,000,000 of years, and whether our fossilized bones will not seem to them as monstrous as those of *Ichthyosaurus* now do; and whether they, adjusted for a more sensitive state of equilibrium, will not consider the extremes of temperature, within which we now exist, to be just as violent and destructive as those of the older geological times appear to us? Yea, even if sun and earth should solidify and become motionless, who could say what new worlds would not be ready to develop life? Meteoric stones sometimes contain hydro-carbons; the light of the heads of comets exhibits a spectrum which is most like that of the electrical light in gases containing hydrogen and carbon. But carbon is the element, which is characteristic of organic compounds, from which living bodies are built up. Who knows whether these bodies, which everywhere swarm through space, do not scatter germs of life, wherever there is a new world, which has become capable of giving place to organic bodies? And this life we might perhaps consider as allied to ours in its primitive germ, however different might be the form which it would assume in adapting itself to its new dwelling place.

Probably the lectures "On the Relation of Optics to Painting" and the address "On Thought in Medicine" are the most valuable productions of Professor Helmholtz to be found in this volume, and as space for their proper examination cannot be used in this notice, references will be again made to them on another occasion.

This work should find a place in every library of standard works of Literature.

A MOST successful experiment in theatre illumination was tried on March 30 and 31, at the Athenæum of the Rue des Martyrs, Paris, with the Werdermann incandescent light. The peculiarity of it is that it can be graduated at will for scenic effects, either by introducing resistance coils or varying the velocity of the Gramme machine.

EFFECT OF TEMPERATURE UPON THE ELECTRICAL RESISTANCE OF SELENIUM.—Mr. Shelford Bidwell, in the *Philosophical Magazine* for April, gives an account of some experiments made on the above subject. He says: "The room being 14° Centigrade, a selenium cell was immersed in turpentine at 8° C. There was a great and sudden fall in the resistance. The temperature was then gradually raised. In passing from 8° to 24° the resistance steadily increased; from 24° upwards it rapidly diminished. For this cell, therefore, the resistance is greatest at 24° C. Five other cells were afterwards submitted to the same operation, and their resistance was found to be greatest at temperatures of 23°, 14°, 30°, 25°, and 22° respectively."

ELECTRIC TRANSMISSION OF FORCE FOR WORKING CRANES.—According to E. Hospitalier, the use of hydraulic pressure for the transmission of the power required in working cranes in docks, involves a loss which, in some cases, may reach 88 per cent. This evil is entirely obviated, in addition to a great simplification of the entire plant, by means of electric transmission of power, which enables the original steam power to be fully utilised even when the crane is raising much less than its maximum load. If we reduce the loading of a crane the electro-magnetic machine which drives it will have less work to do, and will revolve more rapidly, and the stronger counter-currents thus produced will react upon the dynamo-electric machine in such a manner that there is a less current produced, and a less demand is made upon the steam-power. The only question is, how the current is to be divided into several unequal branches capable of being varied in strength at any moment.—*La Lumière Electrique.*

ON THE STATIONARY ELECTRIC CURRENT IN CONDUCTIVE SURFACES, AND ON THE GALVANIC RESISTANCE OF PSILOMELAN.—Hugo Meyer, in the first portion of this memoir, discusses the ramification of the current, and the calculation of the resistance of flat plates. The experimental results agree with calculation. In the second part the author's experimental results agree with calculation. In the second part the author examines the resistance of thin plates of psilomelan, and obtains results antagonistic to those of Braun, who found the resistance decrease under the influence of an induction current.

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Dr. H. J. Detmers, of Chicago, has forwarded to us a communication of considerable importance, which will doubtless be read with interest both on this continent and in Europe.

In 1877 the Commissioner of Agriculture reported that during the previous year, the loss due to farm animals dying from infectious and contagious diseases amounted to $16,653,428, of which amount two-thirds, or over $11,000,000, were due to loss of swine. But as this report included returns from only half of the United States, the above sum was, of course, far below the actual losses of the year.

Congress having appropriated $10,000 for defraying the expenses of a commission to investigate the causes which produced these contagious and destructive diseases, and, if possible, to discover remedies, the matter was placed in various hands to conduct the inquiry.

Among those who have received instructions from the Department of Agriculture, Dr. H. J. Detmers has shown considerable skill in attacking the problem, and the results of his work have developed several discoveries of great biological significance.

Although working with inferior microscopical appliances, he soon found that a particular kind of Bacterium was always present in cases of swine plague, and he has been able, apparently, to prove by actual experiment that these Bacteria were the active principle of contagion.

The early investigations of Dr. Detmers were given in the Report of the Agricultural Department for 1878. Since this time Dr. Detmers has, with considerable industry, continued his investigations under more favorable circumstances; for armed with new objectives made by Mr. Tolles, of Boston, with powers of definition equal to anything yet manufactured to aid human vision, a new revelation has resulted from their use.

The latest discoveries of Dr. Detmers we are able to place before our readers in another column of this issue. Possibly the conclusions drawn in this paper may be criticised, and our columns will be open to any exceptions taken on scientific grounds, but our readers must unite in giving credit to Dr. Detmers for the very thorough and exhaustive treatment which this subject has received at his hands.

The researches of Pasteur in a somewhat similar direction, which have been reported in this journal, suggest to us that Dr. Detmers should, like Pasteur, endeavor to arrest the spread of Hog Cholera by a system of vaccination. Dr. Detmers shows in his present paper that by cultivating the Bacterian infecting element, a contagious principle is secured which by inoculation produces a very mild form of the disease. Could not advantage be taken of this fact in the direction we have indicated?

We are glad to announce that Hog Cholera is rapidly becoming a thing of the past, and has decreased since 1878 so rapidly that at the present time *it is difficult to obtain badly infected specimens* for scientific experimental purposes. This fact, which is communicated to us by Dr. Detmers in a private letter, will be welcome news to those interested in this extensive industry and to the public generally. In Dr. Detmer's report, which we publish this day, it should be noticed that he states that in 1878 the malignant or fatal form (with ulcerous tumors) was found in about 75 *per cent.* of all fatal cases (in Illinois), whereas now their occurrence is probably limited to about 5 *per cent. of all* cases.

Thus the Swine-plague is now under control and is rapidly disappearing. These results are clearly due to the wise policy of publicly making known the evil and the danger, and promptly taking precautionary measures. Let the credit then be given where it is due, even if extended to that much abused Department of Agriculture at Washington, which first raised a voice of warning and secured funds from Congress to "investigate and determine the causes, and if possible *to discover remedies*" of one of the most destructive diseases that ever assailed domestic animals.

Of the Trichinæ trouble we have but a few words to offer, as it can be more profitably described without reference to other subjects. We may, however, observe that it is one of the least formidable of diseases found in hogs, and can probably be eradicated, if proper measures are taken. It is useless to assert that it does not exist, and the only common sense view of the case to be taken, is to acknowledge the evil and root it out. Action should be taken by Boards of Trade to at once gather statistics by proper examinations. If, as they assert, there are no Trichinæ in Ameri-

can hogs, the fact will be demonstrated; if, on the contrary, Trichinæ are found, the extent of the trouble will be known and steps can be taken to protect the industry by systematic examination. We believe that the presence of Trichinæ in pigs is confined to certain districts; if so, it can be localized, and the work of investigation gradually reduced within certain limits, and eventually, by proper precautions, the evil would be entirely removed.

MOUNTAIN ELEVATION, AND CHANGES OF TEMPERATURE, IN GEOLOGY.

By Samuel J. Wallace.

It seems a very little thing for heat and cold to play over the face of a continent. But light and unnoticed as the creeping of fate it goes on forever; and the foundations of the everlasting hills are in its iron grasp. Cold and heat. What should a rock-ribbed continent care for them? What do they do?

In latitude 40° to 50° a yearly change of ten degrees of heat penetrates the upper strata to considerable depths; and the expansion of various kinds of stone for 10° varies from one to three feet in twelve thousand; making, say, one foot to the mile, which across North America is half a mile.

This is an always recurring and resistless force of outward thrust. It is probably mostly compensated for in its habitual recurrence by elasticity, slippages of strata on others, and by fissures; as well as by the fact that the expansion of solid strata is sometimes less from the deep drift or soil protecting them. But, still, as the superior force is outward, without anything to compel a full return in winter, and as the expansion is less below and greater above, the continued tendency is to push the upper strata forward over others toward the margins of extended plains, with a creeping motion, tending to force up bendings, folds and faults, and to raise mountains and plateaus slowly; and even to accumulate such strain or tension as to cause earthquakes and volcanos.

Though, as Dana and others think, there has been a singular persistence in the general features of deep oceans and of continental tables, yet, great portions of the tabular areas have had their depressions and upheavals from the sea. What must have occurred in such cases?

If a tract of sea-bed is covered by an arctic current at 32°, the cold must finally penetrate to very great depths. Then, should the polar current by any means be shut off, and a warm current flow over it, the temperature would certainly be raised several degrees, and produce an expansion which would find relief in raising mountain ridges, or in arching up its own or other regions. This might go on slowly till great areas were elevated from the ocean.

Rising from the sea, also, would increase the temperature very much, to heave up mountains and plateaus, or still other lands from the sea. This result it seems would have to occur, because of the great depths to which the expansion would reach, and because there would exist no provision for relief of the tension, such as the repeated yearly expansion would work out for itself.

It seems these results must flow from what we already know, whether there is or not, any other cause of elevation. There are some further considerations that may be noticed here.

Where a deep ocean trough bearing an arctic current lies along beside a continent it would form a fixed barrier to such expansions, and probably a chain of mountains would be forced up along it, together with volcanos and earthquakes. The region of least yearly change and greatest cold is said to be in the northern edges of America and Siberia, and the bar connecting them across the pole. From the ends of this region the annual change increases southward and laterally. Singularly, the principal mountain systems of the northern hemisphere seem as if raised by forces or thrusts radiating from this bar and its ends. In America, as Dana shows, the original core of the continent was V-shaped, with its two ridges facing the end of that cold bar between them. And the later elevations preserve parallelism to these original lines, as if showing thrusts from that bar and from each other. In Europasia occur continuations of the same parallelism of elevations as facing thrusts radiating from the sides and the other and broader end of the same cold bar, to the areas of greatest annual changes southward, with still increased force and complexity.

In the southern hemisphere the bases of thrust seem as if, on the contrary, they were the three great ocean beds. And the great mountain systems of the world seem as if raised by thrusts of force radiating from these great northern and southern centers of land and ocean, opposing each other, together with some cross thrusts over broad areas of land. This feature of opposition between the northern land thrusts and the southern ocean beds, brings some of the principal lines of elevation in the northern hemisphere into diagonal courses, except where sweeping around the northern projections of the oceans, especially that of the Indian ocean, and its former connection west to the Atlantic south of Europe.

The present Alleghany system seems to have been raised by the elevation of the Mississippi Valley from the sea during and after the Carboniferous period; the Rocky Mountains by that of the plains, later; and the Alps by that of Northern Africa and Northern Europe, although previous elevations existed.

The familiar example of ice creeping up the shores of ponds and lakes, from repeated changes of temperature in winter, illustrates the principle of such elevations, the walled lakes of Iowa being special illustrations; and interesting observations have been published, showing from fixed levels that oscillations of level do occur from changes of temperature.

REMARKS ON A PATHOGENIC SCHIZOPHYTE.*

Prof. H. J. Detmers.

When about two and a half years ago it became my duty to investigate the prevailing Swine-plague, the so-called Hog-cholera, I first endeavored to ascertain the nature and the cause of that disease, and to accomplish my object, made numerous post-mortem examinations, and paid special attention to the microscopic examinations of the blood and of the morbid products and morbid tissues. Although the microscope at my disposal at the beginning of my investigation is only a small No. VIII Hartnack stand with three Hartnack and Prazmowski objectives—a 1 inch, a ¼ inch, and a 1-9th inch imm. and correctives—and consequently not a strictly first-class instrument, and in its performance by no means equal to the work of a Tolles or a Zeiss, I soon became convinced that the blood, the morbid products, and the morbid tissues of the diseased and dead animal invariably contained, while fresh, and not tainted by putrefaction, a certain kind of Schizophytes or bacteria. The same presented themselves in three different shapes, namely as small globular bacteria or Micrococci, as Zoöglœa-masses or clusters, imbedded in, or kept together by, a viscous mass, and as little rods or filaments. I soon found that all three forms belong to the same organism, and represent only different stages of development. The first or globular form predominated in the blood, the second in the morbid tissues—for instance, in the diseased portions of the lungs and in the lymphatic

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glands—and the rods occurred in greatest numbers in such morbidly changed parts and morbid products—for instance, in the ulcerous tumors of the intestines—as are accessible to atmospheric air and other external influences.

The constant occurrence of these Schizophytes soon made it appear probable that their presence is not merely accidental, but that the same, very likely, are connected with, and characteristic of, the morbid process of the disease. To get at the facts was one of my principal endeavors. How far I have succeeded I leave to others to judge.

Careful and repeated macroscopic and microscopic examinations of the tissues, but especially of the lungs, which, by the way, are always more or less affected by the morbid process of Swine-plague, soon revealed the fact that the principal morbid changes are brought about in the following way: The finer capillary blood vessels become obstructed or plugged, the more fluid portions of the blood exude into the tissues—in the lungs principally and at first into the lobules, and then into the interlobular connective tissue—some, and particularly in young animals not seldom but a great many, of the finest capillaries rupture, and innumerable small extravasations of blood, visible to the naked eye as tiny red spots, are deposited into the tissue. In the skin, subcutaneous tissue, and intestinal membranes the process is essentially the same, but to follow it further would lead too far for the present. Let me, therefore, mention another fact. While the blood taken from a vein of a diseased or dead pig invariably contains a large number of spherical bacteria or Micrococci, and very few, and usually small Zoöglœa-masses, the diseased parts of the lungs, and especially the stagnant blood, which oozes out of the capillaries, if the diseased parts of the lungs are cut into small pieces, invariably contains, besides Micrococci, numerous and large Zoöglœa-masses, which are, most of them, much larger than the blood corpuscles, and abundantly large enough to clog the finer capillaries. All this, of course, does not prove that the Schizophytes constitute the cause of the morbid process. I therefore resorted to experiments. Having found that any inoculation of a healthy pig with the fresh pulmonary exudations of a diseased or dead animal invariably produces the disease in three to fifteen days, or on an average in six days, I concluded it might be ascertained in two different ways—in a negative and in a positive way—whether or not the Schizophytes constitute the cause of the morbid process. If it were possible to free the Schizophytes from everything, and to transfer the same without any vehicle whatever from one animal to another, for instance, like a louse or an itch-mite, the question would be very soon answered. But as that cannot be done, I had to get at the facts in a more indirect way. I repeatedly charged two ounces of an innocent fluid, at first pure and fresh milk, then boiled milk, mutton broth, afterwards water, and finally albumen, with one drop of the infectious pulmonary exudation, containing an abundance of Schizophytes. In about three days the fluids thus charged, which, by the way, were kept at a suitable temperature, were found to be swarming with Schizophytes, identical in appearance to those found in the pulmonary exudation; and every inoculation made with these fluids proved to be effective, but in most cases the attack produced was of a comparatively mild type. To go further into particulars would take too much time; I therefore have to refer for particulars to my reports to the Commissioners of Agriculture. One thing, however, I must state. The fluid transferred by each inoculation was less than half a drop, but this half drop contained innumerable Schizophytes, while as far as could be ascertained by careful microscopic examinations, nothing else contained in the original exudation had multiplied. Consequently, nobody, unless he believes in the power of Hahnemannian dilutions, will contradict, and say, the effect of the inoculations is brought about, not by the Schizophytes, but by an unseen and unknown virus, or chemical something, the existence of which cannot be proved. I was, however, not satisfied with these positive results, and concluded to try also the negative way. Knowing that it is impossible to separate the Schizophytes from their vehicle, I tried to free the latter from the Schizophytes, and resorted to filtration. I filtrated the pulmonary exudations through half a dozen of the finest filtering papers obtainable, but found my effort to be in vain, for the filtrate, although freed from the Zoöglœa-masses and rod-shaped bacteria, yet contained numerous Micrococcus-forms. The filtrate was put in a vial with a tight fitting glass-stopper, and when examined three days later, it contained a great many rod-shaped bacteria, and comparatively few Micrococci. I therefore filtered it again with the same result, except that the Micrococcus-forms were not as numerous after the second filtration as after the first. So I filtered the exudation three or four times, each time through four to six filtering papers, and at intervals of about three days till I was finally not able to detect any Micrococci in the now limpid filtrate. Inoculations with this filtrate proved to be ineffective. At another time—in the following winter—I tried again to free pulmonary exudation from the Schizophytes by means of filtration, but did not succeed. The filtrate always — after each filtration — contained numerous Micrococci. Whether, in this second attempt, I did not hit the right time for my second and third filtrations, that is, a time at which most or all of the micrococci had developed to rod-shaped Schizophytes or filaments; whether the temperature was too low—the first, successful attempt was made in the summer—and therefore the development of the Schizophytes was irregular or retarded; whether my filtering papers were not fine enough; or whether all these circumstances combined made the filtration a failure, I do not know. An inoculation made with this filtrate proved to be effective, but the disease produced was of a very mild character; at any rate, the animal recovered.

If more proof is yet required that the Swine-plague-Schizophytes and nothing else constitute the infectious principle of that disease, and it seems that the above facts which have been published more fully in my reports to the Commissioner of Agriculture, are not deemed sufficient, the following facts, if not making it absolutely certain, will at any rate, especially if considered *in toto*, to a great extent, corroborate the assertion that the Schizophytes have, and must have, a causal connection with the morbid process.

1. It has been, and can be, everywhere observed, where Swine-plague is prevailing, that the infectious principle floating in the air, is attracted and taken up by sores, wounds and even scratches, but does not enter the animal organism through the whole skin and through perfectly healthy respiratory mucous membranes.

2. Antiseptics, or medicines, which are either directly poisonous to the lower forms of organic life, or destructive to those conditions, under which low forms of organic life thrive and develop, and among those antiseptics, especially carbolic acid, iodine, hyposulphite of soda, benzoate of soda, thymol, etc., have proved to constitute almost sure prophylactics. As one of the conditions necessary to the development of Swine-plague bacteria, it seems, has to be considered a certain degree of animal heat. At any rate, after, and while the animal heat of a pig is reduced by a continued treatment with carbolic acid, from the normal (102° to 104° F) to an abnormally low temperature (say 96° to 97° F), every innoculation with fresh infectious material has so far proved to remain ineffective. Further, the various antiseptics, which have proved to be good prophylactics, are very dissimilar in their chemical affinities and actions, and their prophylactic effect cannot very well be explained, if the infectious principle were a chemical agency, a virus, or a poison, but is explained, if the same consist in something endowed with life and power of propagation.

3. If the morbid process, the morbid changes effected, particularly the exudations and extravasations of blood on the lungs and in the skin, and the qualitatively unchanged condition of the blood—that is excepting such changes in its composition as are evidently the product, or necessary consequence, of the morbid changes—are taken into consideration, it becomes obvious that something which causes obstructions in the capillary system—embolism—must constitute the cause, and nothing whatever, able to accomplish that result, can be found, except the colonies or clusters of Schizophytes, the Zoöglœa-masses, imbedded in a viscous substance, while on the other hand, these Zoöglœa-masses are never absent in a case of Swine-plague.

If I am allowed to digress a little, it may be here mentioned that I am well aware of the fact that German and French investigators claim for certain, and it may be, for all, kinds of pathogenic Schizophytes chemical actions or fermenting properties, and undoubtedly many of them, especially among those belonging to the genus Bacillus—I mention *B. anthracis*—and probably some others, do possess and exercise such properties, and cause fermentation. As to the Swine-plague Schizophytes, I have not been able to observe any fermenting effect or chemical action, except such as necessarily results from depriving the animal organism of certain elements and material, appropriated by the Schizophytes, and necessary to their subsistence and propagation. All other morbid changes appear to be the consequence of the obstruction of the capillary system by the Zoöglœa-masses, and therefore, are the product of a mechanical, and not of a chemical agency.

4. The adversaries of the so-called "Germ-theory" of diseases, well knowing that a perfect separation of the Schizophytes (Micrococci, Bacteria, or Bacilli, as the case may be) from their vehicles, the animal tissues and fluids, is impossible, demand absolute proof. If conclusions may be drawn from analogy between diseases of animals and plants, Prof. T. J. Burrill,* of the Illinois Industrial University, more favored by the nature of the objects of his investigation (apple-trees, pear-trees and peach-trees) has furnished evidence, amounting to almost absolute proof, that the so-called blight of apple-trees and pear-trees, and the so-called "yellows" of peaches are caused by Schizophytes similar in size, but otherwise not identical to those which I consider as constituting the cause and infectious principle of Swine-plague, as will be seen by consulting the transactions of the meeting of the American Association for the Advancement of Science in Boston, 1880.

If the infectious principle were a chemical poison or virus, its action, one should suppose, would under all circumstances be exactly the same, and the malignancy of the morbid process, and the time required for its development—the so-called period of incubation, or, more correctly, stage of colonization—would not be subject to changes dependent upon the season of the year, upon the individuality and temperature of the animal, and upon other yet unknown external influences, as is undoubtedly the case. An organic poison or virus, one should suppose, would act somewhat like the virus of a poisonous snake. In the same localities, in the same places, or the same yards and pens, and among the same breeds of hogs, in which the disease was exceedingly malignant in 1878; it was, as a rule, much milder in 1879, and still milder in 1880. As such are unmistakable facts, repeatedly and everywhere observed, it must be concluded that nothing but what is able to undergo changes is subject to growth and development, and acquires vigor and propagates rapidly under favorable, but is weakened and multiplies slowly under unfavorable circumstances—in other words, nothing but what is corporeal and endowed with life—can constitute the cause.

* "Science," Vol. I., pp. 162, 191.

6. If the cause and infectious principle of Swine-plague were a chemical poison or virus, one should suppose a cessation of the morbid process would be impossible, and an animal would never recover, while its organism contains an abundance of the infectious principle in an effective condition, as is undoubtedly the case, because convalescents, and animals nearly recovered, frequently communicate the disease, even in a fatal form, to other, healthy pigs. Further, the fact that an animal, once recovered, possesses but little predisposition for future infection, or is seldom attacked a second time, even if ever so much exposed, and then only contracts the disease in a comparatively mild form, could never be explained; but the whole presents an entirely different aspect, and admits explanation, if low and minute forms of organic life, such as the Schizophytes of Swine-plague, which, by developing and multiplying, finally destroy or exhaust in an animal organism the conditions necessary to future development and propagation, constitute the cause and the infectious principle. (cf. an article entitled: "*The Destruction of Germs*," in "*Popular Science Monthly*," communicated in extract in *R. Hitchcock's Microscopical Journal*, Nov., 1880.)

7. If some part or organ of a pig infected with Swine-plague happens to be in a state of congestion, such a part invariably attracts the infectious principle, and becomes a prominent, if not the principal, seat of the morbid process; a fact difficult of explanation, unless the infectious principal is something solid or corporeal.

8. The adversaries of the so-called "Germ Theory," as they are pleased to call it, demand absolute proof of those who claim that certain infectious diseases owe their origin, or existence and spreading, to very minute forms of organic life. They cannot deny that these forms exist, can be found, and have been shown, but forget to show their virus, poison, fluidum, or chemical something. Does the latter exist only in their imagination? If the adversaries of the so-called "Germ Theory" demand absolute proof on our side of the question, let them set a good example and furnish it on their side, or only produce their virus, fluidum, or whatever it may be, and we will gracefully acknowledge that we are mistaken, and have labored in vain.

9. With the very best objectives ever made, and a fair ability to handle the microscope, I have never been able to find anything identical to the Swine-plague Schizophytes in the blood and tissues of other healthy animals. When I commenced my investigation, the best objective at my disposal was a very fair 1–9 four system immersion lens of Hartnack & Prazmowski, but I soon found it to be insufficient, and procured a 1–16 immersion of the same makers. This, too, after a while, did not give satisfaction, and I received a 1–12 (nominally 1–10) glycerine immersion of R. B. Tolles, which that renowned maker afterwards exchanged for a duplex 1–10 homogeneous immersion. This latter objective proved to be a very superior lens, and gave me glimpses of things of which I desired to see a little more—it showed flagella on *Bacillus subtiles*, which I had never seen with any of the other objectives—and so I thought with a higher power, and a still more perfectly corrected lens, if a more perfect correction could be made, I might be able to see more plainly the distinguishing forms and characteristics of the Swine-plague Schizophytes, and also learn a little more about their mode and manner of propagation. I therefore asked Mr. Tolles to make me a higher power objective especially adapted to my work, and he has furnished me a duplex 1–15 homogeneous immersion objective (in reality a little more than a 1–16), which is, beyond comparison, the best objective I have ever seen. It is even superior, in definition and flatness of field, to a magnificent 1–18 homogeneous immersion objective (in reality a 1–20) of Carl Zeiss, made to order a month or two ago.

As to a proper generic place and name of these Swine-plague Schizophytes, I am at a loss. The best authorities—

Cohn, Klebs, and others—who have attempted a classification are somewhat undecided themselves, and do not agree where generic lines ought to be drawn. At any rate, the Swine-plague Schizophytes do not fit into any of the genera proposed. They are not bacteria, because the single cells are spherical and not oblong; they can hardly be considered as Micrococci, because the same are bi-spherical in their advanced stage of development; and they cannot be classed among the Bacilli on account of their forming Zoöglœa-masses. I have, therefore, preferred to use, for the present, that name, which, without any serious contradiction, is given by modern investigators to the whole family: Schizophytæ or Schizophytes, or the older name, introduced by Naegeli, Schizomycetes.

The Swine-plague Schizophytes present themselves, according to their stage of development, in three different forms and shapes. Their simplest form, it seems, is that of a Micrococcus, or of a small globule of about 0.7 or 0.8 microm. ($\frac{1}{33500}$ inch) in diameter. It occurs invariably in the blood, the morbid products, and exudations, etc. of the diseased animals, and is never absent, but can always be found, though in some cases in much greater numbers than in others. The second form is bi-spherical – the spherical cell having duplicated itself by a gradual contraction in the middle, while growing endwise. These bi-spherical Schizophytes are always more or less numerous, and are motile, or move about, provided the temperature of their vehicle—lung-exudation or blood-serum, for instance—is not too low. Some of them, but probably only those, which, separated from a larger chain, as will presently be explained, are provided, at any rate at one end, with a flagellum—a post-flagellum—which, however, is so exceedingly fine that it can be seen only with the very best high-power objectives, like a Tolles 1–15, and the most favorable light obtainable, and even then only while the Schizophyte is slowly moving. I have never yet been able to see it while the Schizophyte is at rest.

These double Micrococci, or bi-spherical Schizophytes, soon undergo further development. Each single cell soon again contracts in the middle while growing endwise, and, at the same time, separates more and more, and becomes partially independent from its sister cell, with which, however, it remains connected for some time, even after it has completed its duplication. Meanwhile the sister-cell, too, has become bi-spherical, and what a short while ago was a simple bi-spherical cell, has become a double bi-spherical body, resembling a small chain of four round joints. But the duplication does not stop; each of the four single cells, within a short time, doubles again, and soon quite a little rod or filament will be formed, which, on close inspection, presents a string or chain of bi-spherical cells endways, loosely connected with each other. Under moderately high powers—say of 800 or 900 diameters—such a string presents a slender, rod-shaped moniliform bacterium. While the single cells, or each half of each bi-spherical body, soon develop into double or bi-spherical cells, the connection between the latter gradually loosens, so that finally, if the temperature is not too low, and the development a rapid one—I have frequently observed that the number of bi-spherical cells in such a chain becomes doubled in less than five minutes—the chain breaks up into smaller ones (joints), each consisting of one or two bi-spherical Schizophytes, which, in separating from their neighbors, after some swinging to and fro, spin or draw out a very slender thread, a flagellum or cilium. But before all these changes, this rapid duplication, take place, the spherical Micrococci, when about to change to bi-spherical bodies, form those clusters (Zoöglœa or Coccoglia masses), which, being imbedded in, or kept together by, an apparently viscous substance, obstruct the capillaries, and, according to my observations, constitute the principal and direct cause of the morbid process. In these Zoöglœa-masses the single Micrococci, it seems, undergo their first metamorphosis, or change to double bi-spherical cells, and this change continues, till portions of the Zoöglœa-mass separate, or till finally the glia breaks and opens, when the bi-spherical bodies, and also some yet unchanged spherical Micrococci, become free. The former, very soon, commence their duplication, but as each new cell or globule soon produces another one and becomes bi-spherical, the same cannot be the source of the spheriral bodies or Micrococci. The latter, it appears, have another origin, as will be presently explained.

In Swine-plague material, such as blood, blood-serum, lung-exudation, etc., if a day or two old, and sometimes while yet fresh, bacteria of a peculiar shape and form make their appearance. The same are rod-shaped, and a trifle longer than a bi-spherical Schizophyte, or two united spherical bodies, but are not moniliform, and have at one end, or in comparatively rare cases toward the middle, a bright and light-refracting globule of much more density than the rest of the bacterium. This globule is surrounded by a substance or an envelope of considerably less density and is therefore less light-refracting. If that globule is situated at one end of the bacterium as is usually the case, the whole bacterium presents the shape of a club, because the globule and its envelope have much more diameter than the rod. Billroth calls this form a Helobacterium, and the globule a lasting spore (Dauerspore). Such a lasting spore, according to Billroth and Cohn, at any rate, if developed by a Bacillus, is able to resist very high degrees of heat and cold, and is very prolific, as it disseminates a large number of germs, which, probably, constitute the source of the globular bacteria or Micrococci. As such Helobacteria are often found in perfectly fresh blood, and exudations, etc. (in the exudations most frequently) of hogs, which are affected with, or have died of Swine-plague, and are nearly always seen if the blood and exudations, etc., are a few days old, it appears probable that the same not only constitute the source of the spherical bacteria or Micrococci, but also that their great tenacity of life, or resistibility against adverse external influences, explains the ability of the infectious principle of Swine-plague to remain effective for a whole year, if protected, by clinging to, or being imbedded, in a moist and porous substance, such as an old straw stack, etc.

Whether or not Swine-plague-Schizophytes are able to multiply in any other form and manner than stated, I have not been able to observe. One observation, made already at the beginning, has found new and repeated confirmation, viz: wherever, or as soon as *Bacterium termo* makes its appearance in large numbers, the Swine-plague Schizophytes commence to disappear and disappear in about the same ratio, in which the former are increasing in numbers. In blood kept in a vial, Swine-plague Schizophytes cannot be found when the blood commences to exhibit a purplish color, or when the blood corpuscles commence to decay, or become destroyed. Further, the Swine-plague Schizophytes, although presenting the same general characteristics when cultivated in fluids foreign to the animal organism of a hog, show differences in so far as the same present less uniformity in size, and as this development and multiplication proceed slower, and with much less regularity. It seems the cultivated Schizophytes change and develop slower, and probably on that account are less vigorous in producing mischief—at any rate, an inoculation with cultivated Swine-plague Schizophytes, although effective in producing the disease, is always followed by a comparatively milder form of Swine-plague than an inoculation with material directly from the body of a diseased hog. This, however, does not involve that every inoculation with cultivated Schizophytes produces under all circumstances a milder form of Swine-plague, than any natural infection, for such is not the case. The difference may be stated thus: A natural infection, or an inoculation with material directly from the body of a diseased hog, as a rule, produces a malignant and dangerous attack

and as an exception a mild form of the disease—the frequency of the exception depending, it seems, to a great extent upon the prevailing character of the plague, while an inoculation with the cultivated Schizophytes is, as a rule, followed by a mild attack, and as an exception, or in rare cases only, by Swine-plague in its severest form.

Wherever Swine-plague is prevailing in its most malignant or fatal form, or, what is essentially the same, wherever formation of ulcerous tumors in the cæcum and colon is a frequent occurrence, where consequently an abundance of Swine-plague Schizophytes is discharged with the excrements of the diseased animals, there the spreading from animal to animal, and from herd to herd, is a rapid one; and *vice versa*, wherever the spreading is rapid, there ulcerous tumors in the intestines are a frequent occurrence. In 1878 the same (the ulcerous tumors) could be found in about 75 per cent. of all cases that had a fatal termination, while at present (in Illinois) their occurrence is probably limited to about 5 per cent. of all cases.

THE KANSAS CITY ELECTRIC TIME BALL.

By Prof. H. S. PRITCHETT, Astronomer at Morrison Observatory, Glasgow, Missouri.

The first time ball established in the United States was dropped from the dome of the Naval Observatory at Washington in 1855. It is still dropped at Washington mean noon, and has for a long time furnished the standard time for the city and the Departments of the Government.

The New York time ball, established in 1877, is dropped at New York noon, by an electric signal, sent from the Naval Observatory at Washington. It was erected and is maintained by the Western Union Telegraph Company, and is dropped from their building on Broadway. At 11h. 55m. the ball is hoisted half-way up the staff on the tower of the building. At 11h. 58m. it is hoisted to its highest point, when it is about 250 feet above the street and can be well seen by the shipping at the New York and Brooklyn docks, and vessels in the bay, and from suitable positions is visible to a large part of the citizens of New York, Brooklyn, Hoboken and Jersey City.

If on account of wind the ball fails to drop at 12h. om. os., it is held till 12h. 5m. and then dropped. In such cases a small red flag is hoisted at 12h. 1m. and kept flying till 12h. 10m. This ball was for some time dropped by hand, but for the last year the dropping has been automatically effected by the clock at the Observatory. The working of the apparatus has been in the main satisfactory, and the ball has been dropped quite regularly, the failures being caused almost entirely by temporary breaks in the wire or other causes which could not be foreseen.

In the evening papers of the day and in the papers of the next morning a notice is regularly inserted, stating whether the ball dropped at correct time, and if not, its error, fast or slow. Many are at a loss to know how this correction is obtained. It is arrived at in the following manner: The time of the falling of the ball records itself automatically by electricity, near the standard clock of the Western Union Company in the building, the clock itself being regulated by the daily clock-signals from Washington. The difference between the time of falling of the ball and noon, as indicated by the clock, is thus obtained by a direct comparison. This assumes of course the accuracy of the clock, and during a long continued season of cloudy weather, or in case of accident to the clock itself, the time might be somewhat in error, although the published correction might show but a few hundreths of a second. At present however, the Western Union has the benefit also of the Alleghany and Cambridge signals, for the regulation of this clock, so that even during the longest season of cloudy weather it is not probable that the clock could be much in error.

The Boston time ball, which is dropped at noon of Boston time, by means of the noon-time signal from the standard clock of the Harvard College Observatory, is placed upon the large building of the Equitable Life Assurance Company and was paid for and is now maintained by this company. The ball is of copper and weighs about 250 pounds. The machinery used in raising and controlling it is hence much more complicated and costly than in either of the cases before mentioned. The cost of ball and machinery was about $1200. The electric signal which drops it, is given by the clock itself, the ball having a drop of fifteen feet. The nearness of the Observatory, and the fact that the wire used is wholly under its control, give additional convenience and certainty in the dropping of the ball, and reduces the probability of accidents to a minimum, so that it is effected with great regularity and precision. Prof. Pickering, Director of the Observatory, reports for the year ending Nov. 1st, 1880, the ball was dropped exactly at noon on 355 days; on four other days at five minutes past noon, in accordance with the rule adopted; on four other days it was not dropped, leaving only three cases of inaccuracy of dropping.

Quite recently a time ball has been established at Hartford, Conn., and dropped by the Winchester Observatory of Yale College.

The time ball recently erected at Kansas City, and which is dropped as a part of the time service of the Morrison Observatory, is the first attempt in this direction in the West. It was paid for chiefly by an appropriation of the City Council of that city. The site selected was the large building just erected by the Messrs. Bullene, Moores & Emery, on Delaware street. The ball when raised to the top of the staff is about 140 feet above the street, and is generally visible to the business portion of the city. The ball which passes over the staff, is simply a wire skeleton covered with canvas and painted black, and is about three feet in diameter. It was loaded on the inside with lead until it was found to drop instantly and without loss of time. It has a drop of about twenty-five feet and is slowed up as it reachcs the bottom, and is received upon a set of tall springs surmounted by a stout cushion.

The apparatus by means of which the ball is dropped at precisely the right instant, was constructed under the direction of Mr. W. F. Gardner, the instrument maker of the Naval Observatory at Washington, It is of a very simple form, and is found to answer all requirements.

This has been found to work easily and without loss of time and can scarcely get out of order. The entire cost of mounting the ball and machinery was only about $120, and with this small amount it was necessary to use the utmost economy in the purchase of materials and apparatus. Kansas City is about one hundred miles from the Observatory, and except in cases of breaking of the wire, when the ball cannot be dropped at all, it is dropped within one or two-tenths of a second of correct time.

The discrepancy in the local time kept by different jewelers in the city before the erection of the ball was astonishing, and led to endless confusion in business and travel.

On the first day the ball dropped, this difference, in extreme cases, amounted to fifteen or twenty minutes, some being eight or ten minutes fast, others as slow. The establishment of the time ball has brought about a uniformity never before known, and must soon make itself felt, not only as a convenience, but a promoter of punctuality in business engagements.

From the daily clock-signals sent over the wires from the Observatory it will be easy to establish a similar time signal in any city in the West, which will take the

necessary steps to procure these signals. An arrangement has been made also by which they may be distributed to jewelers and clockmakers, and manufacturing establishments in the larger cities.

THE UNITY OF NATURE.

By the Duke of Argyll.

VII.

ON THE MORAL CHARACTER OF MAN CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(*Continued*).

Of one thing, at least, we may be tolerably certain respecting the causes which have led to this extreme dispersion of Mankind to inhospitable regions, at a vast distance from any possible center of their birth. The first Fuegian was not impelled to Cape Horn by the same motives which impelled Mr. Darwin to visit that country in the *Beagle*. The first Eskimo, who wintered on the shores of Baffin's Bay, was not induced to do so for the same reasons which led to the expeditions of Back, of Franklin, or of Rae. The first inhabitants of Australasia did not voyage there under conditions similar to those which attended the voyages of Tasman or of Cook. We cannot suppose that those distant shores were first colonized by men possessed with the genius, and far advanced in the triumphs, of modern civilization. Still less can we suppose that they went there under the influence of that last development of Man's intellectual nature, which leads him to endure almost any suffering in the cause of purely scientific investigation.

Nor is this the only solution of the difficulty which seems to be absolutely excluded by the circumstances of the case. Within the historical period, and in the dim centuries which lie immediately beyond it, we know that many lands have been occupied by conquering races coming from a distance. Sometimes they came to subdue tribes which had long preceded them in occupation, but which were ruder, as well as weaker, than themselves. Sometimes, as in the case of the northern nations bursting in upon the Roman empire, they came to overthrow a civilization which had once been, and in many ways still was, much higher than their own, but which the progress of development in a wrong direction had sunk in degradation and decay. Sometimes they came simply to colonize new lands, at least as favored, and generally much more favored, than their own—bringing with them all the resources of which they were possessed—their flocks and herds, their women and children, as well as their warriors with chariots and horses. Such was the case with some of those nations which at various times have held their sway from Central Asia into Eastern and Central Europe. They were nations on the march. But no movement of a like kind has taken place for many centuries. Lastly, we have the emigrations of our own day, when civilized men, carrying with them all the knowledge, all the requirements, and all the materials of an advanced civilization, have landed in countries which by means of these could be made fit for settlement, and could be converted into the seats of agriculture and of commerce.

Not one of these cases can reasonably be supposed to have been the case of the first arrival of Man in Australasia. The natural disadvantages of the country, as compared with the richness and abundance of the regions from which he must have come, or which were on his southward line of march, preclude the supposition that men were attracted to it by natural objects of desire. We know by experience that if the first settlers had been in a condition to bring with them the higher animals which abound in Asia, these animals would have flourished in Australia as they now do. And so, also, with reference to the cereals—if these had ever been introduced, the modern Australians would not have been wholly without them, and would not have been compelled to live so much on the lowest kinds of animal and vegetable food—on fish, lizards, grubs, snakes, and the roots of ferns.

There is, however, one answer to Mr. Darwin's question, which satisfies all the conditions of the case. There is one explanation, and only one, of the dispersion of the human race to the uttermost extremities of the habitable globe. The secret lies in that great law which Malthus was the first to observe and to establish—the law, namely, that population is always pressing on the limits of subsistence. There is a constant tendency to multiplication beyond those limits. And, among the many consequences of this tendency, the necessity of dispersion stands first and foremost. It is true, indeed, that under some conditions, such as those which have been already indicated, the most energetic races, or the most energetic individuals, have been those who moved. But under many other conditions the advantage has been in favor of those who staid. Quarrels and wars between tribe and tribe, induced by the mere increase of numbers, and by consequent pressure upon the means of living, have been always, ever since Man existed, driving the weaker individuals and the weaker families farther and farther from the original settlements of Mankind.

Then one great argument remains. In the nature of things the original settlements of Man must of necessity have been the most highly favored in the conditions he requires. If, on the commonly received theory of Development, those conditions produced him, they must have reached at the time when, and in the place where, he arose, the very highest degree of perfect adaptation. He must have been happy in the circumstances in which he found himself placed, and presumably he must have been contented to remain there. Equally on the theory of Man being a special creation, we must suppose that when weakest and most ignorant he must have been placed in what was to him a garden—that is to say, in some region where the fruits of the earth were abundant and easily accessible. Whether this region were wide or narrow, he would not naturally leave it except from necessity. On every possible supposition, therefore, as to the origin of Man, those who in the dispersion of the race were first subjected to hard and unfavorable conditions would naturally be those who had least strength to meet them, and upon whom they would have accordingly the most depressing effect. This is a process of Natural Rejection which is the inseparable correlative of the process of Natural Selection. It tends to development in a wrong direction by the combined action of two different circumstances which are inherent in the nature of the case. First, it must be always the weaker men who are driven out from comfortable homes; and, secondly, it must be always to comparativly unfavorable regions that they are compelled to fly. Under the operation of causes so combined as these, it would be strange, indeed, if the physical and mental condition of the tribes which have been exposed to them should remain unchanged. It is true, indeed, that adverse conditions, if they be not too severe, may develop energy, and result in the establishment of races of special hardihood. And in many cases this has been the actual result. But, on the other hand, if physical conditions be as insuperable as those which prevail in Tierra del Fuego or in Baffin's Bay; or if, though less severe than these, they are nevertheless too hard to be overcome by the resources at the disposal of the men who are driven to encounter them, then the battle of life becomes a losing one. Under such circumstances, degration is unavoidable. As surely as the progress of Man is the result of opportunity, that is to say, as surely as it is due to the working of his faculties under stimulating and favoring conditions, so surely must he descend in the scale of intelligence and culture, when that opportunity is taken from him, and when these faculties are placed under conditions where they have no call to work.

It is, then, easy to see some at least of the external circumstances, which, first, in the natural course of things,

would bring an adverse influence to bear upon Mankind. Here we are on firm ground, because we know the law from which comes the necessity of migrations, and the force which has propelled successive generations of men farther and farther in ever widening circles round the original centre or centres of their birth. Then, as it would be always the feebler tribes which would be driven from the ground which has become overstocked, and as the lands to which they went forth were less and less hospitable in climate and productions, the struggle for life would be alwaye harder. And so it would generally happen, in the natural course of things, that the races which were driven farthest would become the rudest and the most engrossed in the pursuits of mere animal existence.

Accordingly, we find that this key of principle fits into and explains many of those facts in the distribution and condition of Mankind, which, in the case of the Fuegians, excited the wonder and curiosity of Darwin. In the light of this explanation, these facts seem to take form and order. It is a fact that the lowest and rudest tribes in the population of the globe have been found, as we have seen, at the farthest extremities of its larger continents—or in the distant islands of its great oceans, or among the hills and forests which in every land have been the last refuge of the victims of violence and misfortune. These extreme points of land which in both hemispheres extend into severe latitudes are not the only portions of the globe which are highly unfavorable to man. There are other regions quite as bad, if not, in some respects, even worse. In the dense, uniform and gloomy forests of the Amazon and Orinoco there are tribes which seem to be among the lowest in the world. It cannot be unconnected with the savagery of the condition to which they have been reduced that we find the remarkable fact that all those regions of Tropical America are wholly wanting in the animals which are capable of domestication; and which are inseparable from the earliest traces of human culture. The Ox, the Horse, and the Sheep are all absent—even as regards the genera to which they belong. There are indeed the Tapir, the Paca, and the Curassow Turkey, and all these are animals which can be tamed. But none of them will breed in confinement, and the races cannot be established as useful servants of Mankind. In contrast with these and with other insuperable disadvantages of men driven into the forests of Tropical America, it is instructive to observe that the same races, where free from these disadvantages, were never reduced to the same condition. In Peru the Indian races had the Llama, and had also an advanced civilization.[4] In India, too, it is always the Hill Tribes who furnish the least favorable specimens of our race. But in every one of these cases we have the presence of external circumstances and physical conditions which are comparatively unfavorable. It is quite certain that these conditions must have had their own effect. It is equally certain that the races which have been subject to them for a long and indefinite time must have been once under the influence of conditions much more favorable; and the inevitable conclusion follows, that the savagery and degradation of their existing state is to a great extent the result of development in a wrong direction.

There are other arguments all pointing the same way, the force of which cannot be fully estimated, except by those who are familiar with some of the fundamental conceptions which seem to rise unbidden in the mind from the facts which geology has revealed touching the history of Creation. One of these facts is that each new organic Form, or each new variety of birth, seems to have been introduced with a wonderful energy of life. It is needless to repeat that this fact stands in close connection with every possible theory of Evolution. If these new Forms were the product of favoring conditions, the prevalence of these conditions would start them with force upon their way. The initial energy would be great. Where every condition was favorable—so favorable indeed that the new birth is assumed to have been nothing but their natural result—then the newly-born would be strong and lusty. And such, accordingly, is the fact in that record of creation which Palæontology affords. The vigor which prevails in the youth of an individual is but the type of the vigor which has always prevailed in new and rising species. All the complex influences which led to their being born, led also to their being fat and flourishing. That which caused them to arise at all must have had the effect of causing them to prevail. The condition of all the lowest races of men is in absolute contrast with everything which this law demands. Everywhere, and in everything, they exhibit all the characteristics of an energy which is spent —of a force which has declined—of a vitality which has been arrested. In numbers they are stationary, or dwindling; in mind they are feeble and uninventive; in habits they are stupid or positively suicidal.

It is another symptom of a wrong development being the real secret of their condition that the lowest of them seem to have lost even the power to rise. Though individually capable of learning what civilized men have taught them, yet as races they have been invariably scorched by the light of civilization, and have withered before it like a plant whose roots have failed. The power of assimilation seems to have departed, as it always does depart, from an organism which is worn out. This has not been the result with races which, though very barbarous, have never sunk below the pastoral or the agricultural stage. It is remarkable that the Indian races of North America are perhaps the highest which have exhibited this fatal and irredeemable incapacity to rise; and it is precisely in their case that we have the most direct evidence of degradation by development in a wrong direction. There are abundant remains of a very ancient American civilization, which was marked by the construction of great public works and by the development of an indigenous agriculture founded on the maize, which is a cereal indigenous to the continent of America. This civilization was subsequently destroyed or lost, and then succeeded a period in which Man relapsed into partial barbarism. The spots which had been first forest, then, perhaps, sacred monuments, and thirdly, cultivated ground, relapsed into forest once more.[5] So strong is this evidence of degradation having affected the population of a great part of the American continent, that the distinguished author from whom these words are quoted, and who generally represents the savage as the nearest living representative of primeval man, is obliged to ask, "What fatal cause destroyed this earlier civilization? Why were these fortifications forsaken—these cities in ruins? How were the populous nations which once inhabited the rich American valleys reduced to the poor tribes of savages whom the European found there? Did the North and South once before rise up in arms against one another? Did the terrible appellation, the 'Dark and Bloody Land,' applied to Kentucky, commemorate these ancient wars?"[6] Whatever may have been the original cause, the process of degradation has been going on within the historic period. When Europeans first came in contact with the Indian tribes, there was more agriculture among them then than there is now. They have long descended to the condition of pure hunters. The most fundamental of all the elements of a civilized and settled life—the love and practice of agriculture—has been lost. Development in the wrong direction had done its work. There is no insoluble mystery in this result. It is, in all probability, if indeed it be not certainly,

[4] "Naturalist on the Amazons," Bates, vol. i. p. 191–3.

[5] Lubbock, "Prehistoric Times," p. 234.

[6] *Ibid.*, p. 236.

attributable to one cause, that of internecine and devastating wars. And these again are the result of a natural and universal instinct which has its own legitimate fields of operation, but which like all other human instincts is liable to degenerate into a destructive passion. The love of dominion is strong in all men, and it has ever been strongest in the strongest races. But the love of fighting and of conquest very often does sink into a mere lust of blood. The natural rivalry of different communities may become such implacable hatred as to be satisfied with nothing short of the extermination of an enemy. Inspired by this passion, particular races or tribes have sometimes acquired a power and a ferocity in fighting, against which other tribes of a much higher character and of a much more advanced civilization have been unable to contend.

This is no fancy picture. It is a mistake to suppose that the decline of civilization in the American continent has been due to the invasion of it by Europeans since the discovery of Columbus. Just as the older civilization of that continent was an indigenous civilization founded on the cultivation of a cereal peculiar to the American continent, so also does the decay and loss of this civilization seem to have been a purely indigenous decay. Mr. Wilson, in his very interesting work on "Prehistoric Man," gives an account of the process by which barbarism has been actually seen extending among the Red Indian tribes. When the valley of the St. Lawrence first came under the observation of Europeans, some of those tribes were found to be leading a settled life, practicing agriculture, and constituting communities in possession of all the elements of a civilization fairly begun, or probably long inherited. The destruction of these communities was affected by the savage hostility of one or two particular tribes, such as the Iroquois and the Mohawks. In these tribes the lust of blood had been developed into an absorbing passion, so that their very name became a terror and a scourge, Wholly given up to war as a pursuit, their path was red with blood, and the more peaceful and civilized branches of the same stock were driven, a scanty remnant, into forests and marshes, where their condition was necessarily reduced to that of savages, living wholly by the chase. It is a curious and instructive fact that this sequence of events was so vividly and painfully remembered among some of the Red Indian tribes that it had become embodied in a religious myth. It was said that in old times the Indians were increasing so fast that they were threatened with want, and that the Great Spirit then taught them to make war, and thus to thin one another's numbers.[7] Although this myth stands in very close connection with the universal tradition of a Golden Age, or of a Past in some measure better than the Present, it is remarkable on account of the specific cause which it assigns for deterioration and decay, a cause in respect to which we have historical evidence of its actual effect. When the great French navigator, Cartier, first explored the St. Lawrence in 1534–5, he ascended to that point of its course whence the city of Montreal now looks down upon its vast and splendid prospect of fertile lands and of rushing waters. He found it occupied by the Indian town of Hochelaga—inhabited by a comparatively civilized people, busy not only in fishing or in hunting, but also in a successful husbandry. The town was strongly fortified, and it was surrounded by cultivated ground. Within one hundred and seven years—some time between 1535 and 1642—Hochelaga had utterly disappeared, with all its population, and all its culture. It had been destroyed by wars, and its site had returned to forest or to bush. To this day when men dig the foundations of new houses in Montreal they dig up the flint implements of the Hochelagans, which, although about 350 years old, may now be reckoned by the scientific anthropologist as relics of the "Stone Age,"[8] and of an ancient universal savagery. The same course of things prevailed over the greater part of Canada. During the first half of the seventeenth century a large part of the valley of the St. Lawrence, and vast tracts of country on both shores of the great Lakes, are known to have been devastated by exterminating wars. In 1626 a Jesuit missionary penetrated into the settlement of a tribe called the Attiwenderonks. He found them inhabiting towns and villages, and largely cultivating tobacco, maize and beans. The country inhabited by the tribe which has left its name in Lake Erie, is stated to have been greatly more extensive, and is everywhere covered with the marks of a similar stage of civilization. Within less than thirty years another missionary found the whole of these regions a silent desert. In like manner the country round Lake Huron was, at the same period of time, seen to be full of populous villages defended by walls, and surrounded by cultivated fields. But the same fate befell them.[9] They were extirpated by the Mohawks.

Here then we see in actual operation, within very recent times, a true cause—which is quite capable of producing the effects which, by some means or another, have certainly been produced—and that, too, on the largest scale—upon the American continent. It is a cause arising out of one of the universal instincts of Mankind, developed in such excess as to become a destructive mania. Many nations most highly civilized have been extremely warlike—and the ambition they have cherished of subduing other nations has been the means of extending over the world their own knowledge of the arts of government, and their own high attainments in the science of jurisprudence. But when the same passion takes possession of ruder men, and is directed by irrational antipathies between rival families and rival tribes, it may be, and has often been one of the most desolating scourges of humanity. In itself an abuse and a degradation which none of the lower animals exhibit, it tends always to the evolution of further evils, to the complete destruction of civilized communities or to the reduction of their scanty remnants to the condition and the habits of savage life.

It results from these facts and considerations, gathered over a wide field of observation and experience, that the processes of Evolution and Development as they work in Man, lead to consequences wholly different from those to which they lead in other departments of Creation. There, they tend always in one of two directions, both of which are directions predetermined and in perfect harmony with the unity of Nature. One of these directions is that of perfect success, the other of these directions is that of speedy extinction. Among the lower animals, when a new Form appears, it suits exactly its surrounding conditions; and when it ceases to do so it ceases to survive. Or if it does survive it lives by change, by giving birth to something new, and by ceasing to be identical with its former self. So far as we can actually see the past work of development among the beasts, it is a work which has always led either to rapid multiplication or to rapid extinction, There is no alternative. But in man the processes of Evolution lead in a great variety of directions—some of them tending more or less directly to the elevation of the creature, but others of them tending very speedily and very powerfully to its degradation. In some men they have led to an intellectual and moral standing, of which we can conceive it to be true that it is only a "little lower than the angels." In others they have ended in a condition of which it is too evidently true that it is a great deal lower than the condition of beasts.

We can get, however, a great deal nearer towards the understanding of this anomaly than the mere recognition of it as a fact. Hitherto we have been dealing only with

[7] "Fossil Men," Principal Dawson. p. 47. Montreal, 1880.

[8] "Fossil Men," Principal Dawson, pp. 29–42. Montreal, 1880.

[9] "Prehistoric Man," Dan. Wilson, pp. 359, 60.

one of the two great causes of change,—namely, that of unfavorable external or physical conditions. Let us now look at the other—namely, the internal nature and character of Man. We can see how it is that, when working under certain conditions, the peculiar powers of Man must lead to endless developments in a wrong direction. Foremost among these powers is the gift of Reason. I speak here of Reason not as the word is often used, to express a great variety of powers, but as applied to the logical faculty alone. In this restricted sense, the gift of Reason is nothing more than the gift of seeing the necessity or the natural consequences of things—whether these be things said or things done. It is the faculty by which, consciously or unconsciously, we go through the mental process expressed in the word "therefore." It is the faculty which confers on us a true gift of prophecy—the power of foreseeing that which "must shortly come to pass." In its practical application to conduct, and to the affairs of life, it is the gift by which we see the means which will secure for us certain ends, whether these ends be the getting of that which we desire, or the avoiding of that which we dread. But in its root, and in its essence, as well as in its application to the abstract reasoning of mathematics, it is simply the faculty by which we see one proposition as involving, or as following from another. The power of such a faculty obviously must be, as it actually is, immeasurable and inexhaustible, because there is no limit to this kind of following. That is to say, there is no end to the number of things which are the consequence of each other. Whatever happens in the world is the result of causes, moral or material, which have gone before, and this result again becomes the cause of other consequences, moral or material, which must follow in their turn. It is a necessary result of the unity of nature, and of the continuity of things, that the links of consequence are the links of an endless chain. It is the business of Reason to see these links as they come one by one gradually into view; and it is in the nature of a reasoning creature to be drawn along by them in the line, whatever it may be, which is the line of their direction. The distance which may be traversed in following that direction even for a short time, and by a single mind, is often very great—so great that a man may be, and often is, a different Being from himself, both in opinions and in conduct, at two different epochs of his life. There are, indeed, individuals, and there are times and conditions of society, in which thought is comparatively stagnant, when it travels nowhere, or when its movements are so slow and gradual as to be imperceptible. But, on the other hand, there are times when mind is on the march. And then it travels fast and far. The journey is immense indeed, which may be accomplished by a few successive generations of men following, one after another, the links of consequence. At the end of such a journey, the children may be separated from their fathers by more than the breadth of oceans. They may have passed into new regions of thought and of opinion, of habit and of worship. If the movement has been slow, and if the time occupied has been long, it will be all the more difficult to retrace the steps by which the change has been brought about. It will appear more absolute and complete than it really is—the new regions of thought being in truth connected with the old by a well-beaten and continuous track.

But these endless processes of development arising out of the operation of the reasoning faculty, are consistent with any result—good or bad. Whether the great changes they produce have been for the better or for the worse, must depend, not on the length of the journey, but on the original direction in which it was begun. It depends on whether that direction has been right or wrong—on whether the road taken has been the logical development of a lie. The one has a train of consequences as long and as endless as the other. It is the nature of the reasoning faculty that it works from data. But these data are supplied to it from many different sources. In the processes of reasoning on which the abstract sciences depend, the fundamental data are axioms or self-evident propositions. These may, in a sense, be said to be supplied by the reasoning faculty itself, because the recognition of a truth as self-evident is in itself an exercise of the reasoning faculty. But in all branches of knowledge, other than the abstract sciences, that is to say, in every department of thought which most nearly concerns our conduct and our beliefs, the data on which Reason has to work are supplied to it from sources external to itself. In matters of Belief, they come, for the most part, from Authority, in some one or other of its many forms, or from imagination working according to its own laws upon impressions received from the external world. In matters of conduct, the data supplied to Reason come from all the innumerable motives which are founded on the desires. But in all these different provinces of thought it is the tendency and the work of Reason to follow the proposition, or the belief, or the motive, to all its consequences. Unless, therefore, the proposition is really as true as it seems to be; unless the belief is really according to the fact; unless the motive is really legitimate and good, it is the necessary effect of the logical faculty to carry men farther and farther into the paths of error, until it lands them in depths of degradation and corruption of which unreasoning creatures are incapable. It is astonishing how reasonable—that is to say, how logical—are even the most revolting practices connected, for example, with religious worship or religious customs, provided we accept as true some fundamental conception of which they are the natural result. If it be true that the God we worship is a Being who delights in suffering, and takes pleasure, as it were, in the very smell of blood, then it is not irrational to appease Him with hecatombs of human victims. This is an extreme case. There are, however, such cases, as we know, actually existing in the world. But, short of this, the same principle is illustrated in innumerable cases, where cruel and apparently irrational customs are in reality nothing but the logical consequences of some fundamental belief respecting the nature, the character, and the commands of God. In like manner, in the region of morals and of conduct not directly connected with religious beliefs, Reason may be nothing but the servant of Desire, and in this service may have no other work to do than that of devising means to the most wicked ends. If the doctrine given to Reason be the doctrine that pleasure and self-indulgence, at whatever sacrifice to others, are the great aims and ends of life, then Reason will be busy in seeking out "many inventions" for the attainment of them, each invention being more advanced than another in its defiance of all obligation and in its abandonment of all sense of duty. Thus the development of selfishness under the guidance of faculties which place at its command the great powers of foresight and contrivance, is a kind of development quite as natural and quite as common as that which constitutes the growth of knowledge and of virtue. It is indeed a development which, under the condition supposed—that is to say, the condition of false or erroneous data supplied to the reasoning faculty—is not an accident or a contingency, but a necessary and inevitable result.

And here there is one very curious circumstance to be observed, which brings us still closer to the real seat of the anomaly which makes Man in so many ways the one great exception to the order of Nature. That circumstance is the helplessness of mere Reason to correct the kind of error which is most powerful in vitiating conduct. In those processes of abstract Reason which are the great instruments of work in the exact sciences, the reasoning faculty has the power of very soon detecting any element of error in the data from which it starts. That any given proposition leads to an absurd result is one of the familiar methods of disproof in mathematics.

That one of only two alternatives is proved to be absurd is conclusive demonstration that the other must be true. In this way Reason corrects her own operations, for the faculty which recognizes one proposition as evidently absurd, is the same faculty which recognizes another proposition as evidently true. It is, indeed, because of its contradicting something evidently true, or something which has been already proved to be true, that the absurd result is seen to be absurd. It is in this way that, in the exact sciences, erroneous data are being perpetually detected, and the sources of error are being perpetually eliminated. But reason seems to have no similar power of detecting errors in the data which are supplied to it from other departments of thought. In the developments, for example, of social habits, and of the moral sentiments on which these principally depend, no results, however extravagant or revolting, are at all certain of being rejected because of their absurdity. No practice however cruel, no custom however destructive, is sure on account of its cruelty or of of its destructiveness to be at once detected and rejected as self-evidently wrong. Reason works upon the data supplied to it by superstition, or by selfish passions and desires, apparently without any power of questioning the validity of those data, or, at all events, without any power of immediately recognizing even their most extreme results as evidently false. In Religion, at least, it would almost seem as if there were no axiomatic truths which are universally, constantly, and instinctively present to the mind—none at least, which are incapable of being obscured—and which, therefore, inevitably compel it to revolt against every course or every belief inconsistent with them. It is through this agency of erroneous belief that the very highest of our faculties, the sense of obligation, may and does become itself the most powerful of all agents in the development of evil. It consecrates what is worst in our own nature, or whatever of bad has come to be shown in the multitudinous elements which that nature contains. The consequence is, that the gift of Reason is the very gift by means of which error in belief, and vice in character, are carried from one stage of development to another, until at last they may, and they often do, result in conditions of life and conduct removed by an immeasurable distance from those which are in accordance with the order and with the analogies of Nature.

These are the conditions of life, very much lower, as we have seen, than those which prevail among the brutes, which it is now the fashion to assume to be the nearest type of the conditions from which the human race began its course. They are, in reality and on the contrary, conditions which could not possibly have been reached except after a very long journey. They are the goal at which men have arrived after running for many generations in a wrong direction. They are the result of Evolution—they are the product of Development. But it is the evolution of germs whose growth is noxious. It is the development of passions and desires, some of which are peculiar to himself, but all of which are in him freed from the guiding limitations which in every other department of Nature prevail among the motive forces of the world, and by means of which alone they work to order.

It is in the absence of these limitations that what is called the Free Will of Man consists. It is not a freedom which is absolute and unconditional. It is not a freedom which is without limitations of its own. It is not a freedom which confers on Man the power of acting except on some one or other of the motives which it is in his nature to entertain. But that nature is so infinitely complex, so many-sided, is open to so many influences, and is capable of so many movements, that practically their combinations are almost infinite. His freedom is a freedom to choose among these motives, and to choose what he knows to be the worse instead of the better part. This is the freedom without which there could be no action attaining to the rank of virtue, and this also is the freedom in the wrong exercise of which all vice consists. There is no theoretical necessity that along with this freedom there should be a propensity to use it wrongly. It is perfectly conceivable that such freedom should exist, and that all the desires and dispositions of men should be to use it rightly. Not only is this conceivable, but it is a wonder that it should be otherwise. That a Being with powers of mind and capacities of enjoyment rising high above those which belong to any other creatures, should, alone of all these creatures, have an innate tendency to use his powers, not only to his own detriment, but even to his own self-torture and destruction, is such an exception to all rule, such a departure from all order, and such a violation of all the reasonableness of Nature, that we cannot think too much of the mystery it involves. It is possible that some light may be thrown upon this mystery by following the facts connected with it into one of the principal fields of their display—namely, the History of Religion. But this must form the subject of another chapter.

ASTRONOMY.

DISCOVERY OF A NEW COMET.

Mr. Lewis Swift, of Rochester, N. Y., has announced to the Smithsonian Institution the discovery by himself, on Sunday morning, May 1st, 1881, of a bright comet in Right Ascension $0^h\ 0^m$, Declination 37° North. The comet rises a little before the sun and is moving slowly south.

Professor A. Hall makes the following enquiry in "The Analyst:" "Observations on the motions of the sun-spots have also established the fact that the sun is not strictly a fixed body, around which the earth revolves, but that it has a motion of its own thro' space."—*Physiography*, by T. H. Huxley, F. R. S., 2nd Ed., p. 365. How can the above fact be determined by observations of the sun-spots?

NOTES.

A BILL has been introduced into Parliament for the purpose of authorising the erection of a system of pneumatic clocks in the streets of London.

AUSTRALIAN TELEGRAPHY.—At the close of 1879 some 31,556 miles of telegraph wire were at work on the Australian Continent, and 40,634 miles with Tasmania and New Zealand added.

IT is said that the Telephone Company in Belgium has inaugurated a system by which subscribers leaving word the previous evening may be awakened at any hour in the morning by means of a powerful alarm.

COLONEL PARIS, the head of the Paris fire brigade, has concluded his report on the destruction of the Printemps Establishment by proposing that large warehouses be compelled to light by electricity.—*Nature*.

A FEAT IN NICKEL-PLATING.—The plating company of the Berlopton Lane Works, Stockton-on-Tees, have successfully plated with nickel three large cylinder covers for marine engines, on account of Messrs. Maudslay, Son, and Field, the eminent engineers. The largest cover weighs nearly 1¾ tons, and is 6 ft. 6 in. in diameter. It was plated in the large nickel bath, and polished all over successfully by one of Fenwick's patent portable polishing machines. The same company have also just nickel-plated the whole of the bright parts of Sir James Ramsden's yacht engines, built by the well-known firm, the Barrow Shipbuilding Company (Limited), also, some locomotive domes and safety-valve covers.

BOOKS RECEIVED.

ANIMAL LIFE AS AFFECTED BY THE NATURAL CONDITIONS OF EXISTENCE, by KARL SEMPER, Professor of the University of Würzburg. With two maps and one hundred and six wood cuts. D. Appleton & Co., New York, 1881.

Naturalists have been more than once taunted with too much philosophizing over the Darwinian theory, that they were content to form fanciful ideas as to how this or that difficulty could be hypothetically explained, and that fundamental causes—equally fanciful—were imagined to account for results which were actually observed.

We apprehend that if the Darwinian theory is to become a scientific dogma, the future course of naturalists must lie in the direction of applying the test of exact investigation to the hypotheses already laid down. The task is doubtless a laborious one, and Professor Semper himself says that to prove by experiment the truth of many of these hypotheses long and deep researches are indispensable, or the student will find himself wrecked upon insurmountable difficulties.

There are a number of eminent naturalists whose works tend in this direction, and Professor Semper now leads the van of those who would systematically apply themselves to this task.

Considering that Variability is one of the properties of the animal kingdom which might be most easily traced by exact investigation to its efficient causes, Professor Semper has made it the subject matter of his book, and to facilitate the task of himself and others, has presented a general view of those facts and hypotheses which bear upon the subject, and which are either of universal significance or appear to offer favorable subjects for experimental treatment.

It is not claimed that this work is a complete review of even this branch of the enquiry, but it lays out a plan fortified by a long array of facts, showing how the enquiry may be systematically conducted. It is thus a protest against casual and disconnected observation, and as such may be read with profit by every student.

The introductory chapters are of much interest, dealing with some of the salient points of the Darwinian theory. The plan of the work is also explained and the reader introduced to the subject.

The main body of the book is divided into two sections. The first treats of the influence of *inanimate* surroundings, and in this division Professor Semper directs attention to the influence of food, light, and temperature upon organisms. The results attributable to water, both still, and in motion, are explained, and finally other influences are considered.

In the concluding portion of this work, the influence of *living* surroundings is discussed in such a masterly manner, as to be of the highest service to those studying this subject.

We notice that the subject of the geographical distribution of animals is discussed by Professor Semper, who points out the chief difficulties in bringing into accord the various hypotheses, suggested to explain the undoubted fact that certain species overstep the limits apparently assigned to them by Nature.

Whenever any extensive resemblance between the faunas of two distinct countries is discovered or imagined, a hypothetical history of upheavals and subsidences is suggested, to form a bridge of mainland, as a mode of accounting for this resemblance. This appears to be a favorite theory of Mr. Wallace, and Professor Semper himself admits that such must have been the case in some instances, as he himself found an Indian elephant on Mindanao, the most southerly of the Philippines, for such an animal could scarcely have made the passage by sea. Nevertheless, Professor Semper considers these hypothetical connections of the islands and mainland as not sufficient by themselves to explain even those facts which are already known, as to the distribution of Indian and Australian forms on the islands lying between the two continents.

He further states that "until the question is finally settled whether two parallel series of animal development might not have proceeded independently in two countries remote from each other, we can never venture to regard the resemblance of two faunas as conclusive evidence of their primæval actual connection; nay, it even seems to me that the two historical series of species of the horse, recently discovered both in Europe and America, may, on the contrary, be regarded almost as a proof that each series was developed independently on the two continents, and yet led to the same result: namely, the production of the horse."

Leaving this, however, as an open question, Professor Semper advances a theory for accounting generally for irregularities in the geographical distribution of animals, by suggesting that the action of currents and winds cooperated in a large degree in producing the results which are found to exist.

As a means of distributing animal life it is evident that winds and currents conveyed certain animals from place to place, but Professor Semper points out that these influences frequently acted as a hindrance to the distribution of species. Every navigator is familiar with the fact that currents have a dividing power, shown by the tendency of objects to drift to the edge of the stream, although they may have fallen into the middle of it. This tendency of the current to clear itself—or clean itself—is stronger in proportion to its rapidity and strength. Hence, objects torn by a stream flowing between two islands from the one lying to the left of it, could be borne to that on the right side only under specially favoring circumstances; and *vice versa*, those brought from the right could never, or very rarely, be carried to the opposite side. Thus a mixture of the faunas of the two islands might be hindered, simply by the action of the current flowing between them, except in the case of free swimming animals having the power to overcome the mechanical resistance of the current. In considering the striking circumstance that the islands lying close to Africa have quite a different fauna from that of the neighboring continent, this influence is mentioned as a factor.

Between these theories, offered respectively by Mr. Wallace and Professor Semper, no positive conclusions can be drawn, for want of sufficient evidence based on general conclusions, and while neither can be rejected as erroneous, both must remain open for future discussion. Professor Semper, however, claims one advantage that his hypothesis appeals for proof only to such elements as can be brought under direct observation, while that of Mr. Wallace is intrinsically incapable of demonstration by observation.

The work concludes with sixty pages of closely-printed notes, containing much useful information, and a long array of facts bearing on the subject matter of the work.

We have probably shown by this review that Professor Semper has presented a work of the highest value to every naturalist, and we can assure the general reader that he will find in it material that will engross his attention, and cause him to regret the moment when he arrives at the last pages.

ERRATUM.

Mr. Dopp desires to make the following correction in his paper in the last issue:

"In my article on page 200 of "SCIENCE," the expression $\lambda = \frac{v}{n}$ and $\lambda' = \frac{v}{n'}$ should have been $\lambda = \frac{V}{n}$ and $\lambda' = \frac{V}{n'}$ V being the velocity of light.

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THE alleged discovery of a new motive force for driving engines, patented by Professor Gamgee, of Washington, is already condemned on theoretical grounds, both in this country and in Europe.

The principle involved is not a new one, and, so far, all previous attempts in the same direction have ended in failure. In this case, Chief Engineer Isherwood, of the United States Navy, gives an endorsement to Professor Gamgee's scheme, which has caused some eminent physicists to give an attention to it which perhaps it hardly deserved.

From what we can gather, we understand that Professor Gamgee proposes to work his engine with ammonia, taking advantage of the fact that in a liquified state it boils at—37.3° Fahr., and that at 60° Fahr. it exerts a pressure of seven atmospheres—or, say, 100 lbs. to the square inch.

Authorities differ on this subject, but so far as liquid ammonia is concerned, it is stated as follows: "That at atmospheric pressure, and a temperature of 62° Fahr., 1 lb. of the gas occupies about 23 cubic feet, while 1 lb. of liquid ammonia would occupy only 36 cubic inches."

According to Mr. Isherwood, the "zero-motor" is an apparatus in which liquid ammonia can be vaporized under considerable pressure by means of the heat in water, or in the external atmosphere, and the gas so obtained is used to propel a piston through a cylinder—the gas being employed with the greatest measure of expansion found possible.

At this point the difficulty is presented of returning the ammonia to the boiler. Professor Gamgee offers no explanation, but claims to be able to accomplish it by some method he has invented. He asserts that in its expansion the liberated gas is refrigerated and diminished in bulk, and becomes partially liquified at the end of the stroke of the piston, when it is exhausted and returned from whence it came.

Against this, Professor Simon Newcomb and some English writers assert, that in the absence of demonstration to the contrary, it will absorb as much power to convert the ammonia gas into the liquid form as the latter will give out when vaporized.

In the "zero-motor" Professor Gamgee professes to have an engine capable of exerting great power, and without the necessity of using any fuel, and indirectly the claim is made of solving successfully the problem of perpetual motion.

Apart from some fundamental errors which underlie the scheme, many theoretical difficulties could be suggested, but as a practical test of "the discovery" will probably be made, further discussion may profitably be postponed until the result of the trial is known.

It will no doubt be a genuine surprise to all students of nature to learn that a German scientist has found fossil plants and animal forms in most of the meteorites (chondrites) which he has examined for the purpose.

DR. OTTO HAHN, who has taken a prominent part in the discussion on the "*Eozoön canadense*" has, in the usual way, prepared sections of many of these bodies. These he has had photographed and thereby attained a result which is independent of the microscopist's vision. Dr. Hahn claims that they show many forms of plants and animals in a fossil state contained in their mass, of which the highest forms are crinoids, corals and allied species. He has placed this collection of sections in the hands of Dr. Weinland of Tübingen, (formerly of Philadelphia) for thorough classification.

We regret that we are unable to endorse this interesting discovery. Professor Whitfield, superintendent of the fossils and minerals in the American Museum of Natural History, has seen Dr. Hahn's drawings and was unable to verify the presence of the organic forms referred to. He attributed Dr. Hahn's error to a too sanguine temperament, and an "imagination which bodies forth the form of things unknown."

WE are indebted to our Washington correspondent for a brief mention of an interesting paper by Dr. George M. Sternberg, on "A Fatal Form of Septicæmia in the Rabbit, produced by Subcutaneous Injection of Human Saliva."

Dr. Sternberg recently published a translation of Dr. Antoine Magnin's work on Bacteria, and has had considerable experience in making investigations on septic organisms. He now asserts that the human saliva carries with it a deadly poison, which will kill a rabbit in forty-eight hours; other animals also appear to be influenced more or less by the same cause, while still others—the dog, for instance—resist the

poison. Some salivas are more fatal than others—that of Dr. Sternberg being especially virulent. It will be noticed in our report that Dr. Sternberg attributes the poisonous element to the presence of Micrococci—having found this form of Bacteria both in the saliva employed and in the poisoned blood of the victims.

These facts may be considered in conjunction with experiments by M. Pasteur in the same direction.

SCIENTIFIC SOCIETIES IN WASHINGTON.

THE BIOLOGICAL SOCIETY.—At the last two meetings the Society has listened to four papers: A Fatal Form of *Septicæmia* in the Rabbit produced by Subcutaneous Injection of Human Saliva, by Dr. George M. Sternberg; On the Mortality of Marine Animals in the Gulf of Mexico, by Mr. Ernest Ingersoll; A Statistical View of the Flora of the District of Columbia, by Professor Lester F. Ward; and Notes on Scale Insects, by Professor J. H. Comstock. All of these papers were of the highest scientific value, prepared by specialists in connection with their own immediate investigations. Dr. Sternberg has been making experiments for the past two years under the patronage of the National Board of Health, concerning the causes and development of epidemic diseases. In the course of his labors he has made careful observations with reference to inoculation, and in the paper referred to above gave the Society the benefit of his experiments on saliva injected under the skin of the rabbit. As an elaborate report will appear in the proceedings of the Board of Health, it will be necessary to state only the conclusions arrived at, which are as follows: The rabbits impregnated died invariably in less than 48 hours. Other animals which did not succumb were afflicted with sores. Dogs resist the poison, guinea pigs yield less readily than rabbits, fowls escape entirely. Some salivas are more fatal than others; that of Dr. Sternberg is especially virulent. The presence of Micrococci in the saliva, in the blood of the poisoned animals, and in that of animals infected with this poisoned blood led the author to the conviction that the evil effect was owing to these minute bacteria.

Mr. Ernest Ingersoll, who has been studying the waters of the Gulf of Mexico in the interest of the U. S. Fish Commission, reported that in certain years there occurred a great mortality among the marine animals. In the years 1844, 1854 and 1878 such disasters had been noticed, but the one most injurious in its consequences was in the year 1880. Oysters, clams, fish, and even sponges, were involved in the universal ruin. The beaches were so thickly covered with the dead bodies that the inhabitants were driven from their homes. Various attempts were made to account for the phenomenon, but with indifferent success.

Professor Ward is preparing a work to be entitled "A Catalogue of the Flora of the District of Columbia." It will include all the phænogamous plants and the vascular cryptogams. The number of species enumerated is 1233, distributed among 526 genera, as follows:

Polypetalous	genera,	173	Species,	354
Gamopetalous	"	169	"	388
Monochlamydeous	"	47	"	122
Monocotyledonous	"	120	"	321
Coniferæ	"	4	"	7
Vascular Cryptogams	"	13	"	41
		526		1233

Professor Ward then proceeded to give the census of these species with reference to the orders, to the position of the district north and south, and east and west, as well as in comparison with local floras which have been described with sufficient accuracy.

Professor Comstock's paper on the *Coccidæ*, or scale insects, was a very entertaining treatment of a very dry subject. The group under discussion is usually regarded as the most uninteresting of all the animal kingdom as well as the most anomalous. It is true that the lac of commerce and that of Arizona is the product of these insects, but the most of them are worthless or pernicious. They infest greenhouse plants and most of our useful fruit and timber trees. A specimen was exhibited which had been taken from Europe to Los Angeles, Cal, and back to Washington, upon a lemon, and at the end of its nine-thousand-mile trip was as lively as ever. The method of hatching, of the deposit of the meal, or lac, and of moulting in the male and female, were described and illustrated with drawings and cabinet specimens. The method of classifying these animals into species has been a very uncertain one. Even the later used characteristic, namely, the series of pores or openings on the penultimate ring not being always invariable. Professor Comstock has found the fringe on the last segment of the abdomen to be the most constant specific characteristic. An interesting point in the paper was a discovery made by Mrs. Comstock, that the poisers behind the wings are furnished with a hooklike process which fits into a groove on the back of the wing and helps to sustain it in flight.

THE ANTHROPOLOGICAL SOCIETY.—The entire session of the Society at its last meeting was occupied with the reading and discussion of a paper read by the Rev. Clay McAuley upon the Seminole Indians still remaining in Florida. Of this once formidable but now humbled tribe there remain in the vicinity of Lake Okeechobee 208 individuals, 37 families, 22 camps, and 5 settlements. There are no half-breeds among them, the occurrence of such a birth would probably subject the author to torture or death. They are healthy, have an abundance of food, and are probably increasing. The men are tall, well proportioned, erect, lithe, and graceful. The women are shapely, agreeable, vigorous, and many of them handsome. Dr. McAuley singled out three, whom he characterized as the stately, the beautiful, and the handsome among all the Indians whom he had visited. A very minute description was given of the dress, ornament, customs, and language of these people. A full report will appear in the publication of Major J. W. Powell's Bureau of Ethnology.

THE ULTRA-GASEOUS OR RADIANT STATE OF MATTER.*

BY PROF. H. S. CARHART.

The announcement by Mr. William Crookes, F. R. S., some six years ago, that he had produced mechanical motion by the direct impact of waves of light created a profound impression in the scientific world. But when it was found that the Radiometer, which was supposed to exhibit this new action of light, carried a system of blackened vanes, delicately balanced in a very high vacuum, it impressed most physicists as being an interesting form of heat engine receiving its supply of heat by absorption of radiant energy.

Mr. Crookes subsequently adopted the same view, which was the only tenable one, and investigated the subject in a long series of exceedingly skilful and ingenious experiments. His researches on the Radiometer formed the introduction to a more extended series of investigations into the movement of the residual gas of very high vacua, under the influence of heat and the negative discharge of electricity. These investigations carry us to the very farthest boundary of matter thus far attained, and furnish an ocular demonstration of some of those molecular movements that have heretofore been merely imagined. In fact the phenomena observed are such that Mr. Crookes has felt himself justified in announcing a fourth, or ultra-gaseous state of matter. Such an an-

*Lecture delivered before the New York Electrical Society on May 5, 1881.

nouncement startles us by its boldness, and encounters the opposition of our conservatism. I shall have the pleasure of reproducing a part of Mr. Crooke's experiments before you with tubes made after his models, and recently imported; and at the close of the exhibition I hope you will be able to draw your own conclusions.

Let us first prepare the way from our study of the subject by a clear presentation to our minds of the exact differences between the solid, the liquid, and the gaseous states. We shall then be able the better to judge whether this new claimant is sufficiently differentiated from ordinary gases to warrant its being set aside by itself.

I. A solid is composed of distinct molecules separated from one another by spaces which are large compared with the diameter of molecules themselves. These molecules, which are the indivisible units so far as all *physical* changes are concerned, are held rigidly in a fixed relation to one another by cohesion, a force which acts only across these invisible inter-molecular spaces, and the intensity of which depends upon the chemical constitution of the body. The force of cohesion is counterbalanced by the motion of the individual molecules, the energy of this motion constituting the heat of the body. As the temperature rises, the amplitude of vibration increases, and the force of cohesion is diminished by an increase of distance between the molecules. When the temperature falls the reverse process takes place.

Molecules of matter, in the solid state, retain fixity of position about their centres of oscillation; and any attempt to change the relation of these centres, by distorting the body, calls into action resistances opposing the change of form.

II. When the temperature rises so high that the molecules lose their fixity of position, and are released from the rigid thraldom of cohesion, so that they can apparently roll round one another without changing the distances of their centres, then the solid becomes liquid. As the force of cohesion differs with each kind of matter, so the temperatures of liquefaction also differ. In liquids the cohesion is much diminished but not entirely overcome by the energy of molecular oscillations, which we call heat. A liquid is characterized by taking the form of the containing vessel, by its surface becoming horizontal, and by its equal transmission of applied pressure in all directions. The state of liquidity is due to intermolecular motions of much greater amplitude than those peculiar to the solid state.

III. The temperature still rising, the molecules are finally released entirely from the force of cohesion, and move about in every conceivable direction with enormous and constantly varying velocities. The mean distance between them is sufficiently great to render cohesion inoperative; but they come into constant collisions with one another, now colliding, and now rebounding, changing their direction of motion and perhaps their velocity with every impact. On account of this rapid molecular motion and the absence of cohesion in gases, they tend to expand indefinitely; and they transmit pressure by multitudinous collisions among the molecules, and exert pressure on the walls of the containing vessel by innumerable impacts against its interior. Place a small, closed balloon, partly filled with air, under the receiver of an airpump and exhaust. The balloon distends more and more as the exhaustion proceeds, because of this molecular bombardment against its inner surface, the external coun eracting pressure being withdrawn. In much the same way an impenetrable sheet of metal might be kept suspended horizontally in the air by bullets fired vertically against its under surface. The dynamical principles concerned in the two cases are identical. When a gas is heated, the velocity of the individual molecules increases, and they exert greater pressure on the walls of the containing vessel by their impact, or by expansion secure a longer free path for themselves between successive collisions with one another. This mean free path, or mean length of path, is known to be about $\frac{1}{250000}$ of an inch at the average or normal pressure of the air, while the mean distance between the molecules is about one seven-millionth of an inch, according to the late Prof. Clerk Maxwell. Accordingly a cubic inch of air at normal density must contain the cube of 7,000,000 or 343 quintillions of molecules. Each of these molecules, moreover has, perhaps, millions of encounters every second, the number varying with the kind of gas considered. The state of gaseity is therefore pre-eminently characterized by innumerable molecular collisions, from which proceed all the properties constituting the ordinary gaseous state of matter; and this state continues so long as the motions of the molecules are in every possible direction, and the collisions almost infinite in number. Under such conditions the mean length of path of the molecules, or the distance traversed between two successive collisions, is extremely small compared with the dimensions of the containing vessel, only $\frac{1}{250000}$ of an inch.

Such are the distinctions existing between the solid, the liquid, and the gaseous states, distinctions depending entirely upon molecular aggregation. In the snow or frost crystal, for instance, the molecules of water are grouped together in a definite structural relation; the application of heat destroys the structure, the molecules becoming more nearly individualized by a certain increase of independence or freedom of motion, and the solid becomes a liquid; a still further rise of temperature increases this molecular independence to such an extent that their only physical relationship depends upon innumerable collisions with one another. If now, by any means, molecular independence can be rendered so nearly complete that the hits may be disregarded in comparison with the misses, then the free path of the molecule between the hits may become comparable with the dimensions of the containing vessel, the properties which constitute gaseity are reduced to a minimum, and matter is exalted to an ultra-gaseous state. The residual gas is then in that peculiar state when it has ceased to have the power of adjusting its own pressure; and consequently the phenomenon of diverting the molecules by suitable means into any paths at desire will be possible. Such is the ultimate result of gaseous expansion. As the rarefaction proceeds the number of molecules in a given space becomes smaller and smaller, and the mean length of path greater and greater, the molecular collisions diminishing as the free path increases.

At an exhaustion of a millionth of an atmosphere the number of molecules in any given space is reduced to one millionth of the number at the ordinary pressure; each molecule has a million times more room to move about in, and its mean length of path becomes about four inches (1,000,000 times $\frac{1}{250000}$ in.) If now the rarefaction be carried to another millionth, the space appropriated to each molecule is then increased a million million times, but the distance between the molecules becomes only one seven-hundredth of an inch, and a cubic inch contain seven then not less than 340 millions of molecules, according to Prof. Clerk Maxwell. This rarefaction is about 50,000 times further than the best mercurial air-pump can attain. A *gas* transmits pressure instantaneously in all directions by innumerable molecular collisions; an *ultra gas*, or "radiant matter," has lost the power of adjusting its pressure because of the infrequency of collisions among the molecules.

If then a bulb about four inches in diameter is exhausted to a millionth of an atmosphere, the residual gas in it will have lost the power of rapid adjustment of pressure to equality at every point. Hence any extraneous force, like heat or electricity, may "infuse order into the apparently disorderly jostling of the molecules in every direction by coercing them into a methodical rectilinear movement. * * * * * And according to the extent to which this onward movement has replaced the irregular motions, which constitute the very essence of the

gaseous condition, to that extent have the molecules assumed the condition of radiant matter."

Let us now see to what extent Mr. Crookes has reached these conditions in his Radiometer and radiant matter tubes.

In the Radiometer the blackened vanes become heated by absorbing radiant energy (both heat and light), and they project the contiguous gaseous molecules from their surfaces by communicating to them their molecular motion, much as a vibrating drum-head would project into air grains of sand strewn on it. An increase of pressure is thus produced between the heated lamp-black and the gaseous residue; but this pressure is not transmitted throughout the whole bulb because of the relative infrequency of collisions among the molecules at this degree of exhaustion. Hence the molecules are projected forward against the cooler bulb in right lines, and the blackened vanes retreat because the repulsion between them and the gaseous residue is mutual. [A Radiometer projected on the screen by an oxyhydrogen lantern; also, a diagram showing the paths of the projected molecules].

Mr. Crookes investigated the Radiometer with an ingenuity and a variety of detail that left nothing to be desired. Moreover, his visualization of the molecular motions taking place in this little instrument prepared him for a further research into the motion of the residual gas in Geissler tubes. The dark space around the negative pole, which broadens as the exhaustion proceeds, and the brilliant stratification displayed by many Geissler tubes, were hints in the line of his special studies. I cannot forbear, at this point, to call your attention to the vibration of the air in a sounding pipe, made visible by very light, precipitated silica powder. The resemblance between the beautiful segmentation of the pipe by the thin, vertical planes of silica powder, and the stratification of a Geissler tube, is most marked and suggestive. [A finely stratified Geissler tube shown; and a glass pipe, containing silica powder, and vibrated by a whistle projected on the screen]. As the silica planes limit the free swing of the air particles in the pipe, so the bright portions of the vacuum tube show when the residual gases of contiguous segments encounter each other—the vibration being set up by the passage of electricity. And it may be observed here that as the transmission of sound is the onward transference of energy by motion, so the passage of electricity is the transference of energy from one point to another by means of another form of motion.

The exhaustion which answers best for a Geissler tube is comparatively low. I have frequently obtained beautiful stratification in a tube six feet long, exhausted by a good air-pump. Mr. Crookes carried the exhaustion much further and obtained entirely new results. The first noticeable feature was the gradual broadening of the dark space surrounding the negative pole as the exhaustion advanced [dark space tube, exhibited a transverse sheet of aluminum in the middle constituting the negative pole]. This dark space is regarded as the free path of the molecules at this degree of exhaustion. It increases as the exhaustion proceeds, and contracts when the exhaustion diminishes. The molecules of air are projected normally from the negative pole, and the illumination at the boundary of the dark space is due to the collision between the gas projected from the negative pole and the more slowly moving molecules advancing toward it. Here, then the lines of molecular pressure, caused by the excitement of the negative pole, are illuminated by the induction spark.

With still higher vacua the free path becomes equal to the dimensions of the containing vessel, and the projected molecules impinge directly against its walls. The molecules stream from the negative pole with enormous velocities and dart across the tube with comparatively few collisions. Their motion is then arrested by the solid matter of the tube. A noteworthy property of this radiant matter then appears. Luminosity is produced by the impact of the projected molecules against solid matter. Phosphorescence, as this luminosity is called, is thus excited, its color depending upon the kind of matter receiving the impact. [Three phosphorescent tubes shown; also a bulb partly filled with phosphorescing material.] English glass phosphoresces a light blue, uranium glass rather dark green, and soft German glass, light apple green. Rubies always shine with a deep red hue whatever the color of the gem itself. The artificial rubies made in Paris show no variation from the real stones in the color of their phosphorescent light. This phosphorescence takes place better at an exhaustion of about a millionth of an atmosphere than at any other. A tube designed to show the dependence of the phosphorescence upon exhaustion, is made by connecting to the main tube a small supplementary tube containing caustic potash, which holds captive a certain amount of aqueous vapor [the tube exhibited]. Turning on the coil, the tube now shows green phosphorescence, but upon heating the potash tube with a small lamp, aqueous vapor is released, the phosphorescence gradually disappears, and is replaced by the stratified discharge of a Geissler tube. Withdrawing the lamp, the vapor is re-absorbed by the caustic potash, the fine stratification widens out slowly and finally retreats towards the potash bulb, while a wave of green light sweeps from the negative pole, driving the last pale stratification into the potash tube. Radiant matter moves in straight lines and absolutely refuses to turn a corner. This peculiarity is shown by a V-shaped tube. [The tube exhibited.] The flood of green light proceeds from the negative pole at the top only as far as the bottom, refusing to turn the angle toward the positive in the other branch. Reversing the current, the illuminated branch follows the negative pole. A striking contrast is thus presented between a Geissler and a Crookes tube; but this contrast is brought out more clearly still by two bulbs exactly alike, except in the degree of their exhaustion. Both are fitted with one negative terminal and three positive ones. With the low vacuum tube the stratification follows the direction of the positive pole, changing its path as that pole is changed. Changing now to the high vacuum or Crookes tube, containing only about a millionth of an atmosphere, and turning on the coil the only light to be seen is the green phosphorescence of the glass. The negative pole is a very shallow cup, and the projected radiant matter crosses its focus and then strikes on the opposite side of the bulb where the impact excites strong phosphorescence. Changing the positive pole produces no change whatever in the path of the projected molecules or the luminosity of the glass. The positive pole exercises no influence whatever upon the direction of discharge of radiant matter from the negative pole. The residual gas is here exalted to the fourth or radiant State, and its free path, under the impulsion of the negative discharge, is entirely across the bulb. Moreover, not only is luminosity induced, but the glass bulb rapidly heats where it receives the cannonade of these invisible balls.

Another peculiarity of radiant matter, depending upon its projection in right lines, is that when intercepted by solid matter it casts a shadow. This large pear-shaped bulb has the negative pole at the small end. A cross cut out of sheet aluminum is placed across the bulb so as to intercept a part of the gaseous molecules streaming from the negative pole. Connecting with the induction coil, the dark shadow of the cross is seen plainly projected on the large end of the bulb. Here the projected molecules pass by the aluminum cross and bombard the walls of the bulb, producing the usual phosphorescence Nothing could show more distinctly than the projection of matter from the negative pole in straight lines.

This bombarding causes the surface of the glass to lose its sensitiveness to intense phosphorescence. The cross is hinged so as to turn down with a slight shake.

Again turning on the coil the rays from the negative pole fall uninterruptedly on the large end of the bulb; the dark cross is now replaced by one more intensely green than the adjacent portions of the glass. The fresh parts of the glass are more susceptible to luminous excitation than the areas previously bombarded. This stenciled image of the black cross, Mr. Crookes tells us, is so persistent that it remained in one case after the glass had been heated hot, so that the end of the bulb was bent in and then blown out again. After re-exhaustion the bright green cross came out plainly in the more intense phosphorescence produced by the electrically projected residual gas. Thus the molecules hammer away upon the glass with sufficient energy to produce a permanent impression.

Since the molecules of the residual gas are driven violently from the negative pole, there should be a recoil of the pole from the molecules. That such is the case is shown by this electrical radiometer. The connections are such that the aluminum vanes constitute the negative pole. The vanes are not blackened but are covered on one side with mica, a non-conductor. The uncovered sides constitute the radiating or projecting surfaces. Turning on the induction current the vanes are repelled and the system rotates rapidly. The phosphorescent spots on the glass, produced by the impact of the molecules projected from the vanes, rotate as the vanes do, giving us a visible image of the process going on in the heat radiometer. In this second form the negative pole consists of a ring of platinum wire. Above this is a fly, composed of mica vanes inclined like the fans of a wind-mill. Turning on the current the matter projected from the wire strikes against the sloping vanes and sets them in motion. But this is not all. I now connect a battery directly with the terminals of the platinum ring, and the passage of the current heats the ring red hot. Directly the fly begins to turn more rapidly than before. Radiant matter is thus projected by heat as well as electricity, and the ordinary heat radiometer is propelled by the recoil of projected radiant matter like the electrical radiometer.

The action of magnetism on this stream of electrified particles is, indeed, curious. When a straight Geissler tube of low vacuum is employed it gives a narrow line of violet light joining the two poles. [This tube placed over the poles of an electro-magnet.] On passing the battery current through the magnet underneath, the violet line is repelled or attracted according to the direction of the currents; but it recovers itself after passing the magnet and proceeds to the other pole. Not so with a tube of high vacuum. This tube contains a mica screen, covered with material which phosphoresces under molecular impact. The radiant matter from the negative pole passes through a narrow opening and impinges upon the screen along its entire length. You observe how the phosphorescence marks the path of the projected molecules. Bringing a strong magnet down over the stream or actuating the electro-magnet, and the luminous path curves toward the side of the tube like the path of a projectile; but in this case the stream does not recover its original direction after deflection, as in the case of the Geissler tube. This same tube is fitted to determine another question of much interest connected with these wonderful phenomena. It is provided with two negative projecting surfaces, and the stream of molecules from each of these may be made to trace its path on the mica screen in a luminous line. Observe the position of this line first from one pole and then the other. What now will be the result if streams issue from both poles at once? If they constitute two currents of electricity in the same direction then we know that they will attract each other; but if they consist of a train of similarly electrified molecules, then they must repel each other Put to the test of experiment we see plainly that the stream lines diverge by mutual repulsion.

We have seen that mechanical action is produced by the recoil from the radiant matter. It is equally true that strong mechanical effects may be produced by the impact of these swiftly moving molecules. This tube is ingeniously constructed with a pair of glass rails running from end to end. Along them rolls freely the axle of a small wheel with broad vanes as paddles. The poles are so situated that the radiating molecules may strike against the vanes. Turning on the induced current, the stream of swiftly moving molecules strikes against the vanes on one side of the wheel and sets it running along the rails. Reversing the current, the wheel stops and returns on its track. [Tube projected on a screen by oxy-hydrogen light.] Another tube has been devised to show both magnetic deflection and mechanical action on the screen. The negative pole is a large, shallow cup. A mica screen intercepts the converging streams of radiant matter.

Behind the screen is placed an easily-revolving mica wheel provided with vanes and making a sort of paddle wheel. So arranged the molecular rays from the pole are cut off from the wheel, and no movement is produced. Placing a magnet over the tube the rays are deflected so as to pass above the screen and the wheel begins to revolve. Reversing the magnetic poles, the deflection is in the other direction, and the wheel now rotates like an undershot water-wheel. Even at this high vacuum of about a millionth of an atmosphere, enough matter remains in the tube to produce a sort of molecular wind under the impulsion of the negative discharge. Radiant matter produces heat as well as mechanical motion when its motion is arrested. The negative pole in this tube is a cup or concave projector. The radiant matter comes to a focus near the cup and then passes on toward the other end of the tube. But on bringing a magnet near it the stream of radiant matter is deflected so as to strike the side of the tube, producing a green, phosphorescent spot. The nearer the magnet approaches the greater the deflection of the stream and the smaller the spot. Covering the side of the tube with wax and placing it in front of the lantern, a dark image of the tube is projected because of the opaque wax. But with the coil in action the approach of the magnet deflects the radiant stream, the tube is heated by the impact of the molecules, the wax melts and becomes transparent, and the light from the lantern passes through.

The heat generated by the arrest of radiant matter is further shown by a bulb of special device. It has a negative pole of aluminum in the form of a concave projector. In its focus is fixed a piece of platinum foil. Turning on the coil, the projected molecules impinging against the platinum at last raise it to a red heat. Indeed Mr. Crookes has actually melted iridio-platinum in such a focus. We are accustomed to see powerful heating effects produced by currents of electricity traversing rather poor conductors; but here red hot platinum glows with the invisible cannonade of innumerable molecules of the air we breathe.

These effects take place indifferently with hydrogen, carbonic acid gas, and air at this high vacuum. The only difference appears to be that the phosphorescence begins at different pressures with the different gases. The results obtained depend, therefore, not on chemical properties, but on the physical, molecular condition of the gaseous residues. These phenomena are so entirely different from those obtained at ordinary degrees of exhaustion, that Mr. Crookes appears to me to be justified in considering that he has at last verified Faraday's early hypothesis of matter in a radiant state. Mr. Crookes has well said that, "In studying this fourth state of matter we seem at length to have within our grasp and obedient to our control the little indivisible particles which, with good warrant, are supposed to constitute the physical basis of the universe. We have seen that in some of its properties radiant matter is as material as this table, while in other properties it almost assumes the character of radiant energy. We have actually touched the

borderland where matter and force seem to merge into one another, the shadowy realm between the known and the unknown, which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this borderland, and even beyond; here, it seems to me, lie ultimate realities, subtle, far-reaching, wonderful."

GREAT ASTRONOMICAL TELESCOPE.

The greatest refracting telescope in the world—Lord Rosse's is a reflecting telescope—has been constructed for the Vienna Observatory by Mr. Howard Grubb, at his celebrated manufactory of astronomical instruments, at Rathmines, near Dublin. We give an illustration, from a sepia drawing of it by Mr. G. Browning, and abridge the following account of it from a description received at this office.

The idea of crowning the observatory at Vienna with a refracting telescope of surpassing power was conceived by the Austro-Hungarian Government about five years ago. Such a building was worthy of the best instrument that could be constructed. Every visitor to the Austrian capital must be struck by it, standing upon a site of between fourteen and fifteen acres at a height of 200 feet above the city, and extending 330 feet in length and 240 feet in width. Desiring to possess the finest telescope which could be procured, the Government commissioned Dr. Edward Weiss, now Director-General of the Observatory at Vienna, to visit all the principal observatories and workshops in the world. He recommended that the task should be confided to Mr. Grubb, of Dublin, who was ordered to construct a refracting telescope of at least 26 inches aperture. A commission was appointed by the Austro-Hungarian Government to superintend the work. It was composed of the following gentlemen: The Earl of Crawford and Balcarres, Dr. Higgins, the Earl of Rosse, Professor Stokes, of Cambridge, Professor Ball, Astronomer Royal for Ireland, Dr. Stoney, Secretary of the Queen's University in Ireland, many years connected with Lord Rosse's observatory, Dr. E. Reynolds, professor of chemistry, Trinity College, Dublin, and Mr. Walsh, Austro-Hungarian Consul in Dublin. On the 16th ult. the Commissioners reported their unanimous approval of the finished instrument.

The general form of the telescope is that known as Grubb's modified Gramme, and is similar to the well-known standard equatorial which he constructed for the Earl of Crawford and Balcarres, Dr. Huggins, Oxford University, Berlin, Cork, and other places. It possesses all the modern improvements and special arrangements of an ingenious character, which are rendered desirable by its great size. The length of the tube is 33 feet 6 inches, and the aperture is 27 inches. The tube is entirely of steel, 3½ feet in diameter in the centre, and tapering to each end. The entire moving parts, including the tube, polar, and declination axis, counterpoise and various adjustments weigh between six and seven tons; yet the whole apparatus is under such control that one person can move it about and manipulate it with the utmost ease. The mechanism is remarkable for its solidity and strength, as well as for its exquisite delicacy.

In order to render the motion of such ponderous instruments sufficiently easy, the makers are generally obliged to reduce the diameter of the axes, particularly that known as the declination axis, to an extent that makes one almost alarmed for their safety, to say nothing of their stability. Mr. Grubb, however, has mastered the difficulties of the position by a peculiar and most interesting system of equipoise, by which he is enabled to make his axes so large and solid as to ensure stability and give perfect confidence without sacrificing the ease of motion. The application of antifriction apparatus to the polar axis has been already successfully effected, and was a simple problem, but Mr. Grubb has the exclusive merit of applying it to the declination axis, which is a task of great and complicated difficulty, demanding the highest scientific skill.

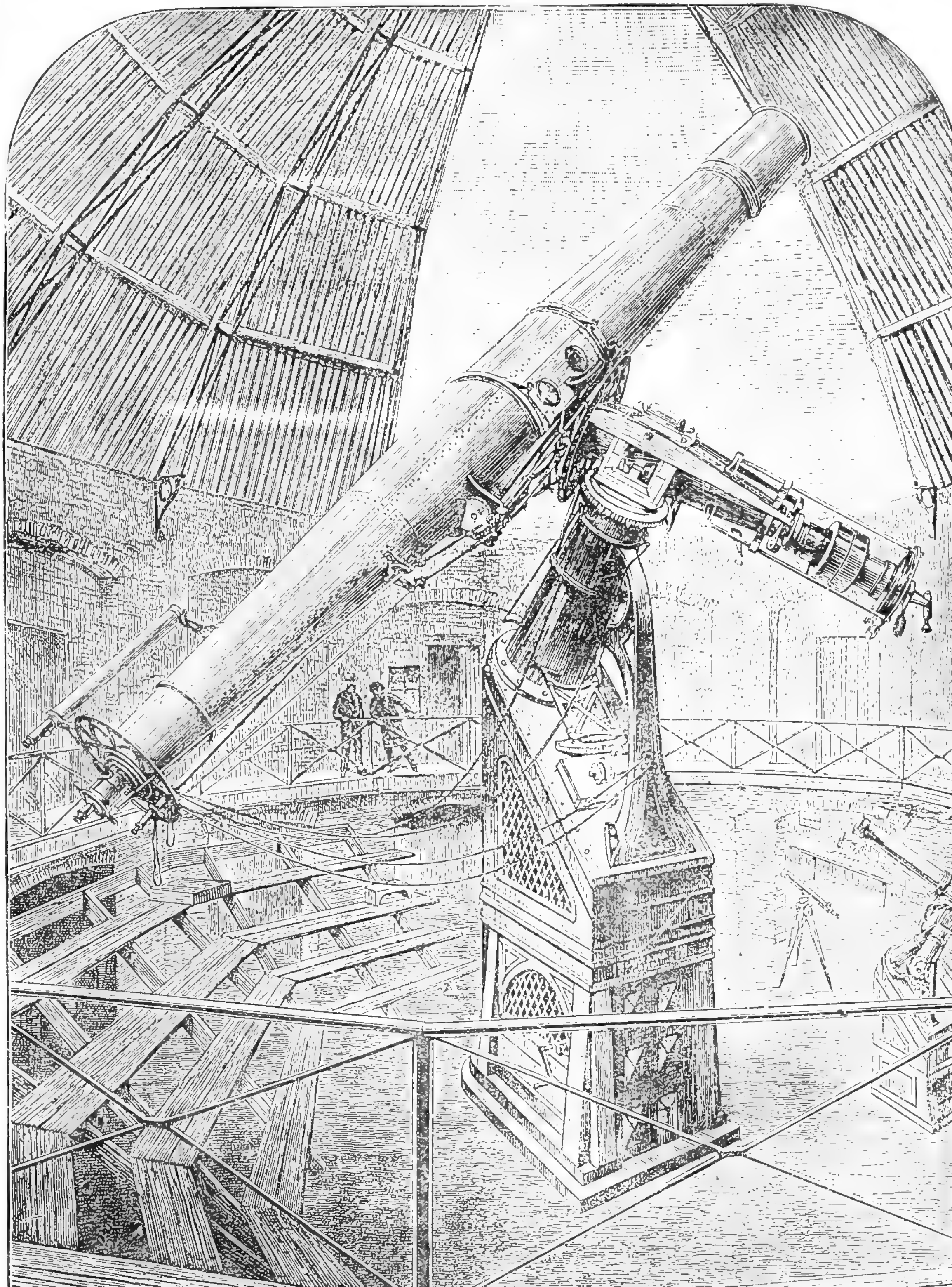
Another remarkable feature in the work is the ingenious arrangement by which the circle can be read with the utmost ease and certainty. It is usually a very troublesome operation with large telescopes to read the circle, and when the circles are about 20 feet or more from the ground the labor and delay which it involves are very formidable. In Mr. Grubb's instrument, the circles are carefully and accurately divided on a band of gold, and by a system of reflectors, at once beautifully simple and ingenious, the observer can without stirring from his chair read all the circles of the instruments through one little reader telescope attached to the side of the main telescope tube.

The setting of the telescope is massive and graceful. The frame on which it rests down to the ground level is of cast iron, and there are chambers of considerable size at the base. In the lower one, which is entered by a door at the end, is a clock for driving the instrument in order to follow the paths of the heavenly bodies. The castings of which the frame is formed are of about ten tons weight, and are of simple but not inelegant design. The clockwork is controlled by Mr. Grubb's novel frictional governor, and is also furnished with his new electric control apparatus. There are two right ascension circles, each 2 feet in diameter, one read from the eye end of the telescope and the other from the ground floor. The declination circle is 5 feet in diameter, and is read from the eye end of the telescope. All the circles are divided on an alloy of half pure gold and half pure silver, which is found to be very white and not liable to corrode or tarnish.

The material for the object glasses was procured from M. Feil, of Paris. The protracted delay in procuring this material for the work was a subject of great anxiety to Mr. Grubb, and occasioned heavy additional outlay on his part. In October, 1879, however, discs were obtained, which in working gave good promise, and in December last he was able to report the work finished—his part of it being, in fact, accomplished in less than half the time stipulated by the agreement with the Austro-Hungarian Government. His task was practically trebled by the difficulty experienced in obtaining pure discs. The success of his undertaking is regarded with great satisfaction and with national pride. He has supplied equipments to most of the modern observatories, but this telescope is his greatest achievement.

THE *Athenæum* prints an interesting extract from a letter from Prof. Draper, giving a description of the progress he has made in photographing the nebula in Orion:—"I have succeeded," he says, "in taking stars in it of the 14.1, 14.2, and 14.7 magnitudes of Pogson's scale. Prof. Pickering has made a series of measures on these magnitudes especially for me at the Harvard College Observatory. You will perceive that we have photographed stars which approach the *minimum visible* of my 11 in. telescope, and we may, therefore hope shortly to photograph stars actually too faint to be seen with the eye in the same instrument. The nebula, which was exposed 104 minutes, extends over an area of about 15′ in diameter, though, as it becomes fainter toward the exterior parts, it is difficult to determine its precise limits." This is a great advance; no star of less than the ninth and a half magnitude has hitherto been photographed.

A CURIOUS magnetic property of the meteoric iron of Santa Cattarina (Brazil), has been lately observed by Professor Lawrence Smith. Small detached fragments, not weighing more than 0.1 to 0.2 gr., were very weakly affected by a magnet; but on being flattened on a piece of steel, with a steel hammer, they become very sensitive to it. By heating red-hot, the particles were made to be still more easily attracted than by flattening. The meteoric iron in question contains 66 iron, 34 nickel.



THE NEW TELESCOPE, VIENNA OBSERVATORY.

THE POLARIZATION OF SOUND.

AN EXAMINATION INTO THE NATURE OF VIBRATIONS IN EXTENDED MEDIA.*

By S. W. Robinson,

Professor of Physics and Mechanics, Ohio State University.

The phenomena of polarization of light have heretofore presupposed transversal vibrations of particles of the luminiferous ether. Such vibrations have not only been supposed transversal when polarized, but primarily, or when in their primitive condition. It is proposed now to show that no necessity exists for considering the vibrations in light as transversal in front of a polarizer; and furthermore to show that in all probability luminous vibrations are primarily longitudinal.

It is well known that light can be radiated, reflected, refracted, diffracted, diffused, can be made to interfere and can be polarized. All these effects are known to be common to sound, except the last, and it is for the sole purpose of explaining this in light that the convenient theory of transversal vibration has been set up by physicists, for the single case of luminous vibration. It is to be noticed that transversal vibrations are not to be assumed impossible when sufficient cause exists. It is simply assumed that the cause is insufficient when a material particle is made to vibrate from the action of a disturbance at a remote single centre transmitted to the particle considered; the centre, the transmission, and the particle considered, being supposed as belonging to a homogeneous medium of indefinite extent. As regards the nature of the vibratory movements of particles of luminiferous ether may we not justly ask that, if we can go through such a range of density as from platinum to hydrogen without a change in the nature of the vibrations where, as we rise in the scale of etheral tenuity, shall longitudinal end and transversal begin? Why should the luminiferous ether, now considered as a substance, have a peculiar form of vibration? If ether undulations can be polarized, why not undulations generally? These questions are not answered by the highest authorities. The short of it all seems to be that if polarized light had never been discovered probably the device of transversal vibrations never would have been set up. Indeed, the eminent author M. J. Jamin, says in his three volume work on Physics at the outset, in his lesson on polarization, and subsequent to the treatment of interference, diffraction and other phenomena: "What has been said previously of the movement of luminous waves is absolutely independent of the directions of the vibration." This is good authority for limiting the *transversal* theory to polarization, authority with which doubtless all physicists will agree on this point. It is, therefore, only necessary to polarize sound to place all the known effects of luminous waves in common with sound waves, or to make the theory of longitudinal vibrations universal.

Assent to the above notions will be the more readily given after noticing the consideration that, in polarized light it is not necessary to suppose the vibrations transversal till after passing the polarizer, and that the latter probably imparts an effect equivalent to a lateral impulse, as due to its one-sided action upon the ray transmitted, thus giving cause for vibration which are more or less transversal; the same being true for undulations in air, water, iron, platinum, hydrogen, luminiferous ether, etc., without exception.

But independent of all questions of polarization we find powerful evidence of the unity of system, for vibrations in all possible media; evidence which, in one case at least, is employed as the basis of a rigorous mathematical demonstration of the impossibility of existence of perpetual motion. This latter named evidence is the famous principle of Helmholtz regarding the action of natural forces among mutually interacting material points, viz.: that the forces must be central forces and functions of the distance, and hence motions of remote particles can only be longitudinal with reference to the centre of force. This principle, considered aside from luminiferous ether, will be universally accepted as truth. But what is the criterion for making an exception of any homogenous substantial medium; even luminiferous ether? Indeed, if any criterion exists for such an exception, it consists simply in a desired convenient means for explaining polarization: a theory of transverse vibrations, which, though convenient and probable beyond the polarizer, has thoughtlessly and without need, been extended to the front of the polarizer, and to undulations in the primitive conditions, where, as shown above, no necessity exists for transversal vibrations. That the necessity for transversal vibrations in primitive rays of light is entirely wanting, let it be granted for the sake of an argument that the source of light, such as the sun or a gas burner, is capable of exciting vibrations in the adjacent medium, which are in all possible directions immediately at the radiant. At a considerable distance from the radiant, the effect upon a single particle will be the resultant of action of all the particles immediately surrounding the radiant, and transmitted by and through the intervening medium; such a resultant impulse can hardly be admitted to be otherwise than longitudinal. Or again, a particle near the radiant imparts an impulse to the adjacent particle. If the passive particle is in direct line with the active one, the impulse received will be in direct line also, and the direction of motion of the two particles coincides. The similar direct action of a second particle upon a third will also be in the same line, and so on indefinitely. At a distance of several hundred or thousand times the diameter of the radiant the line of vibration indicated will be almost perfectly longitudinal irrespective of where the first particle considered is situated about the radiant. That is to say, a particle at the surface of a radiant vibrating in a line of direction tangential to it or transversal, may be considered as transmitting its vibration from particle to particle in a direct line, and hence to the best advantage; and still, at a distance this direct line becomes nearly a line of longitudinal vibration for a remote particle.

Thus all lines of direction of vibration will pass through the radiant or be tangent to it, so that in sunlight tne rays, and the directions of vibration, will all lie within the visual angle of the sun, or within about half a degree of arc, and hence almost perfectly longitudinal.

These considerations all confirm the principle of Helmholtz. And that remarkable principle, together with all considerations presented above, and all which can possibly result from a careful study of the subject of transmitted impulses, go to confirm a universal law of longitudinal vibrations for primitive rays in all possible substantial media, and to antagonize the notion of transversal vibration.

Hence, if light can be polarized, why not undulations generally. The writer, after much study of the subject, became convinced of the possibility of this about eight years ago, and six years ago apparatus was made for putting the matter to an experimental test. Want of time prevented, and further study determined a modification of the apparatus which was made over two years ago. This apparatus was successful in verifying all my preconceived notions in the matter, but owing to extended study and matured views of the principles involved, the experiments simply confirmed, without developing new theories or unanticipated facts. I propose now to describe the apparatus and give the results.

The means adopted for polarizing the undulations is the same as that for polarizing light by reflection. It is well known that when sound passes from one medium into another whose velocity of sound differs, the sound is

refracted. Recent investigations of Henry, Tyndall and others have indicated that when sound encounters a change of density of medium, as when passing from clear atmosphere into a wall of fog, there is a reflection of sound. Altogether there seems no doubt but sound acts like light in these respects, that is, on meeting a change of refractive power, it is both reflected and refracted, as light is at the surface of water or of glass. The reflected light is found to be always more or less polarized, perfectly so for the so-called polarizing angle. This polarizing angle of incidence is such that, as discovered by Brewster for light, the reflected and refracted components of a single ray, as they strike away from the point of incidence are at right angles. As reflected light is polarized, reflected sound was supposed to be also.

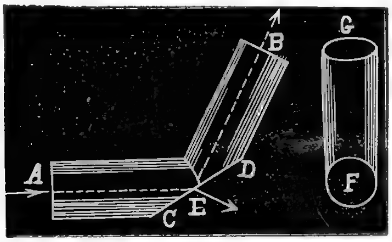


FIG. 1.

Applying the laws of Fresnel and Brewster—1st, that the index of refraction is equal to the ratio of the velocities of the waves in the media; and 2d, that complete polarization is obtained for the particular case of right angled reflected and refracted component rays, we are guided to the proper conditions. We conclude that any two substances having different velocities of propagation of waves may be selected For instance, two gases, like hydrogen and air, any two liquids, any two solids, a solid and a gas, or, generally, any two media whatever. Considerations of convenience would indicate air and illuminating gas, and these were chosen for the present purpose. The velocities of propagation in air and coal gas being as 1125 and 1420, the index of refraction, according to the first law above, is $n = 1{\cdot}26$. The second law gives for the polarizing angle of incidence, tangent $i = n = 1{\cdot}26$, or $i = 51\frac{3}{4}^{\circ}$, the rays or waves being in the gas. To realize this incidence upon a surface of separation between the gas and air, the cool gas was placed in L-shaped tubes, *AB*, Fig. 1, having a portion cut away at the angle, as shown at *CD*. The branches of the L make equal angles of $51\frac{3}{4}^{\circ}$ with the normal to *CD*. A delicate membrane was gummed to the tube covering the opening at *CD*, also shown at *F*, the object of which was to retain the gas and maintain a polarizing surface, *CD*. The arrow at *A* indicates a ray which is incident at *E*, and is then in part refracted outward at *E* in a direction perpendicular to the reflected component *EB*. Each tube was about one inch in diameter and three inches long. A number of these were made of tin, each with one end slightly larger than the other, so that they could be joined up, stove-pipe fashion, to any desired extent. Being cylindrical, the plane of one L-piece could be placed at any angle with the plane of the preceding one, according to the desired polarizing test.

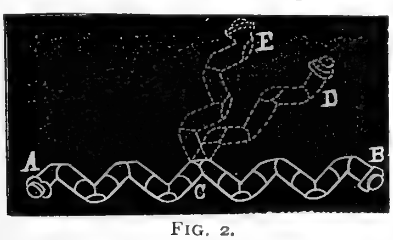


FIG. 2.

Fig. 2 shows the manner of joining the tubes, giving the effect of nine polarizing surfaces, like nine plates of glass in light arranged at the polarizing angle. The nine plates of glass can be used in two parts—one part, 4 for instance, serving as a polarizer and the remaining 5 as analyzer. The ends at *A* and *B* were capped with membranes and the whole filled with illuminating gas. Thus *AC* may serve as a polarizer and *CB*, or *CE*, or *CD* as analyzer. When arranged as in *ACB* or *ACE* all conspire to the same effect of polarization; but when arranged as in *ACD*, the plane of all the L-pieces in *CD* being at right angles to that of those in *AC*, the effect of one part antagonizes that due to the other, and to a maximum degree as regards the angle. Partial effects may be obtained with intermediate angles between 0° and 90°. Again, we observe that the L-pieces of Fig. 2 may be alternately crossed, so that no two contiguous ones will be parallel. It is believed that this arrangement will give the greatest possible antagonistic effect; also while all Ls are in one plane it is not necessary that they be arranged in a zig-zag line, like *AC* and *CE*, but may be indiscriminately connected in that plane. The few experiments made with the above-named arrangements gave very marked results. Of course it need not be confined to nine or any particular number of the L-pieces.

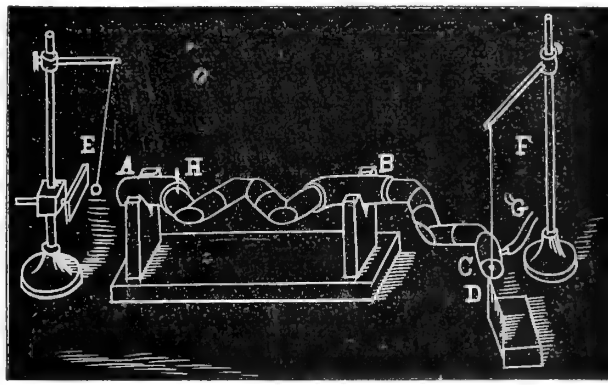


FIG. 3.

It was found, however, wanting in convenience. The apparatus finally adopted is that shown in Fig. 3. A different number of L-pieces were used at different times. The portion *AB* is the polarizer and *BC* the analyzer. The joint at B was kept tight with beeswax; the ends at *A* and *C* were capped square with the same membrane material as were the angles of the Ls, giving, when charged with illuminating gas, a continuous zig-zag column from *A* to *C*. The L-pieces of the polarizer enter half Ls at *A* and *B*, the latter having a common axis and resting in bearings at *A* and *B* in the standards, as shown. The object of this is to enable the experimenter to turn the polarizer readily from cross to parallel, etc. This convenient arrangement of the polarizer is due to my assistant, Mr. Wright. Although applied to the polarizer, it is evidently equally applicable to the analyzer instead. The half L angles were not covered with membranes, but left solid, with gradual inside curvature. Membranes might have been applied here with partial polarizing effect. The half L solid angles are supposed to have detracted in a measure from the percentages of polarization obtained; but this sacrifice is more than compensated for by the greater convenience and constancy of conditions obtained. If this arrangement gives decisive results, of course, more perfect apparatus would. The illuminating gas was admitted by a nipple and rubber hose at *C*, the same flowing the length of tubes and issuing in a small jet at *H*; my assistant kept this ignited, and used the flame length as a pressure indicator, and it served admirably.

The first trials were made by blowing an organ pipe in front of the membrane *A*, to agitate the gas column.

A small mirror was attached to the membrane *C*, reflecting a pencil of light upon a screen. The deportment of the image indicated complex and inadmissible vibratory movements of gas column, and besides quantitative indication was found preferable to qualitative; thereupon the quantitative impulse and indicator pendulums were adopted, as shown at *E* and *F* respectively, Fig. 3. An ivory ball, ¼ inch in diameter, suspended by a thread of 8 inches length, was used at *E*, and so placed that when at rest the ball would just touch the membrane at *A*. The impulse was imparted by bringing the ball back against the stop, shown by means of a spatula held in the hand, and then allowing it to swing free against the membrane, each time with a definite, predetermined arc. So much of the impulse as reaches *C* knocks the pendulum *F* through a certain arc, the same being measured on the scale *D*. This pendulum was a small, hollow glass bead, suspended by a silk fibre and trained delicately against the membrane. The bob carried a pointer for the scale *D*.

In the experiments the ball would be dropped against *A* some five or ten times, at intervals of about ten seconds, the corresponding deflections at *D* being noted and recorded; then the polarizer would be turned 90° and like observations noted. Again, 90° would be turned off, etc., etc.; occasionally the length of impulse arc would be changed, or more or less L-pieces applied, and in each case a large number of observations made.

In the experiments the initial pulse seemed to be followed by a series of vibrations in rapidly decreasing amplitudes; but it is believed that the initial pulse is equivalent to a genuine sound wave, or an undulation. Evidence of soundness of this view is found in the fact that the velocity of sound can be satisfactorily determined by similar pulses sent through tubes of 25 or 50 feet length. It was evident that the initial pulse only, was concerned in the first swing of pointer at *D*.

Observations were made with as few as one L in the polarizer, and two in the analyzer. But the results were small, averaging about 4 per cent. But during the noting of 455 individual results of observation the number of Ls in the polarizer ranged from 1 to 3 and in the analyzer 2 to 3. The average of all of these 455 observations gave a percentage of 8.87 hundredths of quenching of the polarized beam. That is to say, where the analyzer, in light, entirely quenches the polarized beam in turning through 90°, in the above 455 experiments only 8.87 hundreths of the polarized ray was quenched.

But as a higher percentage was looked for, the instrument itself was now examined for possible faults. The membranes were all found under considerable tension. whereas, of course, they should be perfectly free from it. After completely slackening, then, as was supposed, the experiments were continued with the following results:

POLARIZATION CONTINUED.

Coal Gas in L-tubes and Air outside.

Individual results.

Polarizer having 4 Ls and Analyzer 5 Ls.

	=	+	=	+	=	+	=	+	=
	6.0	6.0	8.0	5.0	6.5	5.8	6.2	6.0	6.0
	6.0	6.0	7.5	5.5	6.5	5.5	6.0	5.2	6.2
	6.2	6.0	7.0	5.3	6.3	5.6	6.5	5.2	7.2
	6.1	5.8	7.5	5.2	6.5	5.2	6.4	5.1	6.2
	6.0	5.7	7.0	5.0	6.2	5.7	6.3	5.5	6.1
	6.1	5.5	7.2	5.2	6.6	6.0	6.3	5.1	6.1
	6.2	5.8	6.5	5.0	6.8	5.2	6.2	5.2	6.1
	6.0		7.0	4.9	6.2	5.2	6.5	5.4	6.5
			7.0	4.0	6.3	5.3	6.3	5.8	6.8
							6.3	5.2	6.4
Means	6.07	5 83	7.19	5.01	6.43	5.50	6.30	5.37	6.34

In this series each value given in any column is the number of divisions on the scale *D*, Fig. 3, of the deflection of the indicator pendulum bob. After obtaining one column of results the polarizer, *AB*, was turned 90°, and the next column obtained. Thus the several columns were obtained. The = and + signify that the polarizer and analyzer are in parallel or perpendicular planes respectively.

On completing this series of observations air was passed into the L tubes, completely displacing the coal gas, so that the membranes were now suspended in mid air. Other conditions remained the same. The indicator pendulum now responded to the same impulses so slightly as to be barely observable, but not measurable, and they were apparently the same for the polarizer in all positions; that is to say, when air was upon both sides of the reflecting surfaces there was almost no appreciable reflection. This evidently should be the case, as the light membrane itself is now the chief cause of reflection. This, compared with the results obtained with gas in the tubes, shows that a considerable reflection is due to surfaces of separation of gases differing in density.

As regards the polarization, we observe that every mean under "=" is larger than any mean under "+," a fact which cannot be assumed accidental, nor explained on any other ground than polarization. To find the percentage of this in a single result, we have

Means of the above series.

	=	+
	6.07	5.83
	7.19	5.01
	6.43	5.50
	6.30	5.37
	6.34	
Means,	6.47	5.43

$$\text{Per cent.} = \frac{6.47 - 5.43}{6.47} = 16.1.$$

Whole number of observations, 80.

At this point the membranes were again examined and found to have appreciable tension, though supposed to have been entirely slackened at the beginning of the series of observations just cited. It seemed that the dampness in the gum used, expanded the membranes so that they became tight after thoroughly drying as the experiments went on. It was thereupon determined to slacken them with the utmost possible care, and continue the observations The following table of individual results was then obtained in the manner explained above, the membranes being constantly watched for entire slackness.

POLARIZATION CONTINUED.

Coal Gas in tubes and Air outside of tubes.

Polarizer having 4 Ls and Analyzer 5 Ls.

Individual results.

	=	+	=	+	=	+	=	+	=	+	=	+	=	+
	1.2	0 9	2.0	1.0	1.5	1.0	1.6	1.0	1.8	1.4	1.5	0.9	2.0	1.0
	1.5	1.0	2.1	1.2	1.5	1.0	1.4	1.1	1.5	1 2	1.5	1.0	2.1	1.2
	1.5	1.0	1.8	1.1	1.8	1.0	1.8	1.1	1.5	1.4	1.5	1.0	1.8	1.1
	1.5	1.0	2.0	1.0	1.3	1.0	1.9	1.2	1.7	1.2	1.5	1.0	2.0	1.0
	1.5	1.1	2.0	1.0	1.3	1.0	1.9	1.0	1.7	1.1	1.5	1.1	2.0	1.0
		1.0	2.0	0.9	1.2	1.0	1.8	1.0	1.7	1.2	1.5	1.0	2.0	.0.9
			2.1	0.9	1.3	1.0	1.6	1.0	1.8	1.0			2.1	0.9
					1.5	1.0		1.1	1.7	1.0				
								1.1	1.7					
								1.1	1.6					
Means	1.44	1.00	2.00	1.01	1.45	1.00	1.71	1.07	1.67	1.19	1.50	1.00	2.00	1.01

Polarizer turned 90° immediately following each column of results.

For gas displaced by air, other conditions the same; deflection, 0.00.

The smallness of these results, compared with those of the previous series, may be explained on the ground of extreme and entire slackness of the membranes; also the slackness is still further evinced by the fact that, when air displaced the gas no deflection of the indicator pendulum was observable in response to the impulses. Tense membranes would have turned the sound waves somewhat, and in the manner of the rebound of a drumstick from its drumhead.

To obtain the percentage of polarization effect, we have:

Mean of the above series.

=	+
1.44	1.00
2.00	1.01
1.45	1.00
1.71	1.07
1.67	1.19
1.50	1.00
2.00	1.01
Means, 1.68	1.04

$$\text{Per cent. } \frac{1.68 - 1.04}{1.68} = 38.1.$$

Thus with the apparatus working most perfectly, the analyzer succeeded in extinguishing or quenching 38 per cent. of the polarized wave, a percentage too great to be mistaken. Considering the analyzer and polarizer as equally efficient, the real percentage of polarization by polarizer would be 62 per cent., and of the analyzer 62 per cent., as is evident from the fact that 62 per cent. of 62 per cent, is 38 per cent. Thus, either part of the apparatus obliterates over half of the wave attempting transmission, a fraction which would be unmistakably visible in polarized light.

These results establish the following facts for sound waves or for undulations, viz.:

1st. A decided reflection occurs at a surface separating two gases of different density, confirming the views of Henry and Tyndall in this regard.

2d. In repeated reflection from such surfaces the intensity of the final component varies with the relative positions of those surfaces, the same following the laws of polarization in light, from which we conclude that longitudinal undulations can be polarized.

With sound polarized, we complete the list of effects for longitudinal undulations which are known to light, viz.: radiation, shadows, reflection, refraction, diffusion, diffraction, interference and polarization; for the laws are common for like conditions, viz.: for intensity of radiation in ambient space, $\frac{I}{d^2}$; in parallel space, $\frac{I}{d^1}$; in prismatic space, like a tube, $\frac{I}{d^0}$; for shadows, reflection, refraction and interference as well known; for diffusion, as when a steam whistle is sounded, filling the air with its resounding ring; for diffraction, as sound waves diverging rapidly after passing a narrow space between buildings, like light in passing a narrow slit and diverging; and, finally, for polarization, as above. In studying these comparisons we should recollect the vast difference between the properties of undulations in heavy, and ethereal media. Thus the wave length is very great and the velocity of propagation very small in sound as compared with light. This seems sufficient to account for the greater definition of shadows in light; but when a slit or an obstacle is made as narrow for light as for sound, in comparison to wave length, the diffraction divergence is probably about alike; that is, the divergence at a linear slit in light, or between two buildings in sound; or again the shadow of a silk fibre in light and a sound shadow of Bunker Hill monument, for instance, are about alike considering wave length. With these considerations it may be reasonable to expect incomplete or only partial polarization with such apparatus as employed above.

The conclusions to which we are conducted by the foregoing may be summed up as follows:

1st. That vibrations in extended media, produced from the action of a remote single centre of disturbance, can only be longitudinal, even in light.

2d. That vibrations will be to some extent transversal when due to two or more centres of disturbance not in the same line, as when two or more independent co-existent systems of undulations combine into one, or when a simple system is modified by such lateral disturbance as a reflection or a refraction.

3d. That undulations, to be in a condition called polarized, probably consist of vibrations which are transversal, and that no necessity exists for assuming vibrations transversal in front of a polarizer.

NOTE.—As regards longitudinal, oblique, transversal, etc., in the foregoing, the estimate is to be taken by comparing the direction of the line of vibration of a particle with that of propagation of the wave.

My acknowledgements are due to Mr. Clarence H. Wright, who, while a student in my physical laboratory last Spring, rendered valuable aid in the experimental work.

ASTRONOMY.

COMET (*a*) 1881—SWIFT.

The question of the best method of transmitting telegraphic announcements of astronomical discoveries has just been discussed by the leading European societies, and a system has been devised by which this information may be comprised in a message of sixteen words. Thinking that perhaps a better way existed of doing the same work, the Boston Scientific Society has adapted a telegraphic code to the needs of the occasion, and this system has just received a practical test. The announcements lie within the province of the Smithsonian Institution, and it was accordingly decided to transmit by cable the elements and ephemeris. These here given were computed at Dun Echt Observatory, in Scotland, by Drs. Copeland and Lohse, and have been distributed in this country to astronomers by special circulars of the Boston Scientific Society. That set which was computed at Boston, for the Society, by Mr. S. C. Chandler, Jr., has already been cabled to Europe, and distributed by mail, from the Observatory of Lord Crawford, to astronomers in England and the Continent.

The cablegram received at Boston consisted of sixteen words, and the translation is here appended. According to the same code, the announcement of discovery could be comprised in a message of seven words, which would itself contain check words against possible error in transmission.

The elements and ephemeris computed at Dun Echt, on Monday, May 9, were transmitted by cable to Boston in the following message: "Decimosexto erective contextual bewitchery anticly demonstrative courageously sputter arithmancy stomachical auriferous suety bayou synecdochically bissextile eminently." The translation of this message is as follows, viz:

ELEMENTS OF SWIFT'S COMET, 1881 (*a*).

Per. Passage, 1881, May 20.67, Greenwich Mean Time.

	°	′	
Long. Perihelion,	300	2	Eq. 1881.0.
Long. Node,	124	54	
$\omega = \pi - \Omega$,	175	8	
Inclination,	78	48	
Log. $q = 9.7674$.	$q = 5854$.		
Motion direct.			

EPHEMERIS.

Greenwich, midnight.	A.R. h.	m.	s.	DECL. °	′	Brightness.
May 10,	0	38	32	+26	46	1.69
14,		56	48	21	35	
18,	1	17	32	15	54	
22,		40	48	9	55	2.32

Computed by Dr. R. Copeland and J. G. Lohse, from observations made at Dun Echt Observatory. The light at discovery is taken as unity.

By means of control-words in the message, it is absolutely known that the elements are those computed yesterday in Scotland, and it is proposed to cable in the same way the first elements and ephemerides of future comets, obtainable at either terminus, until the code has been most thoroughly tested. J. RITCHIE, JR.

ON A METHOD OF ISOLATING THE MAMMALIAN HEART.

By H. Newell Martin (Johns Hopkins University.)

To obtain a mammalian heart isolated from the rest of the body and keep it alive for a time sufficient to allow the examination of the effect of various conditions upon its activity has long been a physiological desideratum. The frog's heart has for years been the subject of minute study but hitherto the mammalian heart has been a baffling object. It seems to have been forgotten that while the frog's heart is a spongy structure having no arteries of its own, the mammalian heart is a dense organ dependent for its life on a continuous blood flow in its capillaries; and all attempts hitherto made, so far as I know, have been efforts to apply to the mammal the methods found successful with the frog, with merely the addition of arrangements adapted to keep up the comparatively high temperature at which the mammalian heart normally beats. By working in another way I have recently succeeded in keeping the mammalian heart alive for more than an hour, and beating with perfect rhythm and normal force; the organ is thus made almost as available for study as the heart of the frog. The method adopted is as follows: The animal having been narcotised and the chest opened, the aorta is tied just beyond its arch; then the trunk which, in the cat, gives origin to the right subclavian and the two common carotids, is ligatured close to its origin, and a cannula put in the left subclavian; finally, the inferior and superior venæ cavæ and the azygos vein, and the root of one lung are tied.

Artificial respiration is of course started so soon as the thorax is opened, and kept up henceforth. The course of the circulation is thus:—left auricle, left ventricle, commencement of aorta (and along the left subclavian to the cannula which is connected with a manometer), coronary system, right auricle, right ventricle, pulmonary vessels of one lung, and then back to the left auricle; in other words, the only section of the systemic circulation left is that through the vessels of the heart itself. Since the physiological actions taking place in the lung are among the best known of all occurring in the body, they may be eliminated, and we have practically an isolated and well-working living mammalian heart for study. The nerves going to the heart may be divided if desired, but that is hardly necessary as the want of blood-flow in the nerve centres of the body incapacitates them after a very short time, and they no longer are capable of exerting any influence on the heart. It is possible, however, that changes in the lung vessels may affect the results of experiments made on the heart's work under different conditions (*e.g.* when defibrinated blood is sent into it from a vein under various pressures, or when drugs are administered to it), and an investigation of the nerves, if any, governing the lung vessels must be undertaken as a preliminary to a further study of the direct action of various conditions on the heart's work.

EDINBURGH ROYAL SOCIETY.

There was a very large attendance at the meeting of the Edinburgh Royal Society held recently, to hear Professor Helmholtz, who was announced to make a communication "On Electrolytic Convection." Professor Helmholtz stated to the Society the results of certain later experiments which he had made in working out his theory of electrolytic convection—experiments which, he said, had succeeded better than his former ones had done. He had entered upon his experiments on account of certain objections that had been made to Farraday's electrolytic law, in connection with experiments which showed that a very feeble galvanic current could be kept up by moderate electro-motive force between platinum plates dipped in a slightly acidulated solution, even if the electro-motive force of the battery was not sufficient to decompose the water. He had found, in his earlier experiments, that the only effect of the current was to absorb oxygen given off from the atmosphere and to form a new portion of water, and that the whole electrolytic effect was, not to decompose water and to produce a new quantity of elements—hydrogen and oxygen—but only that on the surface of one plate of platinum oxygen was collected and taken away from the surface of the other. He had, therefore, called these currents electrolytic convection. It was not really a decomposition, but only a transport of one of the products of electrolytic decomposition from one place to another. In his later experiments he had got rid of the atmospheric oxygen, by inclosing the whole electrolytic fluid in a sealed glass vessel. As a result, he had found that the smallest electromotive force, down to the thousandth of a Daniell's cell, produced a strong deflection which went immediately back to zero, that there was no continuous current, and that, if they broke the current, they got a deflection of the same character in the opposite direction to the first and direct one. Sir William Thomson, in speaking on the paper, stated that Professor Hemholtz's theory of electrolytic convection formed quite an era in electrolytic chemistry. Sir William himself made a communication "On the average pressure due to impulse of vortex-rings on a solid," following up inquiries suggested by Helmholtz's vortex theory. Professor Tait stated the result of certain calculations which he had made "On the crushing of glass by pressure," and which had been suggested by his recent inquiry as to error-corrections in the use of the *Challenger* thermometers. The result was that with cylindrical tubes made from ordinary Leith plate-glass, he found the glass gave way under a shear of about 1-250th, and that the strength of the tube was greater as the walls were thicker and the internal diameter decreased. But, however thick the walls might be, or however small the internal diameter, a pressure of 22 or 23 tons on the square inch would inevitably crush the strongest glass.

NOTES.

M. Daudigny, electrical engineer in Paris, has sent to the Municipal Council a petition asking for authority to establish on the top of the Colonne de Juillet a large electric lamp fed by a magneto-electric machine of fifty horse-power. This enormous light is to be diffused by a large reflector of special construction.—*Nature.*

Signor Manet, we learn from an Italian journal, whitens the albumen of blood by means of the electric light; which is projected by a system of mirrors and lenses, giving a strongly luminous effect. The time required varies according as the albumen is more or less separated from the fibrine. In general, 24 hours suffices to give complete decoloration.

Professor Loomis appears still to be experimenting in aerial telegraphy—telegraphing without wires—and it is now said that he proposes to establish communication, through the current which he claims is always found at a great altitude, between one of the highest peaks of the Alps, in Switzerland, and a similarly situated station on the Rocky Mountains, on this continent.

It has been found by M. Laurent that any ordinary good silvered glass mirror, plane, concave, or convex, and of any thickness, may be rendered a magical one by means of heat. A simple way of doing this is to heat a brass tube, and apply the end of it to the silvered face. If the mirror surface is opposite a screen, the section of the tube is reproduced in white; if the former is turned away from the screen, the image (which is seen only after removing the tube) is dark. A cold tube may be used with a hot mirror, and the experiment may be otherwise varied.

M. Duchemin, the inventor of the compass with a circular magnet, now adopted in the French navy, has recently devised, for correcting compasses, a system of magnetic compensators. In place of the straight magnetic bars generally employed he uses magnets of an annular form. If we magnetise a steel ring, it may have two poles at opposite extremities of a given diameter and two neutral lines. Such rings—round, oval, or of any other form, and with or without interruption of continuity—may be utilised for the correction of a compass, by being placed either on the bridge of the vessel or in the binnacle.—*Revue Industrielle.*

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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THE Spring Reception of the American Museum of Natural History, Central Park, New York, and the publication of the Twelfth Annual Report, remind us of the existence of this Institution, and recall its many claims for support from those interested in science, and in the intellectual progress of the people.

The additions and improvements made during the past year make only a short list on the programme, but reflect the excellent management and zeal of the officers in charge of the collections. A large portion of the collections of birds and mammals has been remounted on newly designed stands, the results obtained being most creditable to those who have carried out this improvement.

One of the new features of usefulness recently established is an Economic Department, which will contain specimens illustrating the Economic Botany of all the woods of our country, that are to be used for architectural or building purposes, or in the manufactures, each species being fully represented by specimens of the leaf, flower and fruit.

We fully appreciate the exertions made by the Trustees of this Institution to extend the usefulness of the Museum and to make it a means of teaching the laboring classes the value of scientific knowledge, and its practical bearing on many of the industrial pursuits of life. If such is, at least, one of the objects of establishing this Museum, it is difficult to understand the action of the Trustees in closing its doors on Sundays, that being the only day on which the artisan and mechanic can make a visit, without entailing a direct loss on himself and family. A petition, signed by 16,000 citizens of New York, was recently offered as a direct appeal to the Trustees to accord this privilege to the working classes, and we trust that the board of management, which has always shown a most liberal and enlightened spirit of enterprise in the conduct of this Institution, may reconsider its late decision on this subject.

The city of New York has provided a costly building for the Museum and recently appropriated $35,000 to improve the approaches. In the Report of the Trustees now before us, a direct appeal is made to the people for financial aid and support. We, therefore, believe the Trustees would confer a direct benefit on the Institution by opening its doors to the people on Sundays; the Museum would doubtless become one of the most popular Institutions in the city, and the Legislature would probably respond with no grudging hand, to provide means for the completion of the building and for its maintenance on a liberal scale.

THE announcement is made of an improved method of storing electricity, by M. Camille Faure, of Paris, the London *Times* asserting that "a box of electric energy nearly equivalent to a million feet, contained within less than a cubic foot of space, intact and potential, has been transported from France to Great Britan."

Sir William Thomson is said to have given some endorsement to the discovery, and tests and measurements are in progress at the laboratory of the Glasgow University.

The principle involved in M. Faure's discovery is understood in this country, and the possibility of its general correctness is conceded. The language employed in the announcement is rather equivocal, and the misuse of scientific terms render the exact extent of M. Faure's discovery a matter of some doubt.

We gladly welcome any progress in electical science; but as the necessity for storing electrical energy is of value only in very rare cases, the practical usefulness of M. Faure's discovery must be limited in extent.

MR. EDISON has courteously responded to a request on our part, to offer his opinion on M. Faure's discovery, and we take pleasure in placing before the readers of "SCIENCE" his reply, received since our own notes were in type.

To the Editor of "Science."

DEAR SIR: The Faure battery is an improvement on the Planté battery.

Planté was, I think, the original inventor of the battery which bears his name, invented some years ago for the purpose of storing up electricity.

Faure has simply made a Planté battery, by some means reducing its resistance, and thus reducing the percentage of loss. This is all there is in it.

Some two years ago I patented and applied a method for using the Planté battery in connection with electric lighting. Yours truly,

THOS. A. EDISON.

SENSIBILITY AND ITS DIVERSE FORMS.

By M. Oltramare.

[Translated From the French, by the Marchioness Clara Lanza.]

From the feeble cry by which the infant affirms simultaneously its birth and its sensibility, to the last long drawn sigh which bids adieu to existence, human life oscillates constantly between two opposite conditions created by the nervous system—pleasure and pain, joy and sorrow.

Being creatures developed to a great extent under the influence of the senses, we experience to an extreme degree, the action of all exterior agents, and atone for such pleasures as are granted to us by our exquisite sensibility, with moral and physical suffering. Not satisfied with momentary impressions, we foresee the influences which are to reach us, and by means of our refined intelligence we create those two great incentives to our actions—apprehension and desire.

Being mortal and also conscious of the fact, we naturally have a presentiment of the final destruction of our bodies, and most of us fear this and look upon it with dread. Nothing of this kind, however, is to be met with in animals. The last hour of life, brutal and violent though it may be and totally unexpected, does not affect them. The dog licks his master's hand affectionately whether it be extended to caress or to kill. He is no more conscious of the possibility of death, than is the ox which is led to the slaughter house.

These higher animals have nevertheless, a sensiblity and individuality upon which their reason depends. They possess what we call instinct. But, as we descend further in the animal world, we see that this function gradually diminishes in proportion as the organisms become simplified, until finally we reach a point, where to cut a living creature in two, not only produces no perception of pain, but actually becomes a means of reproduction, each half being capable of forming a distinct organism precisely like the original.

Lower still, we come to plants, which are living organisms, although Linnæus, a naturalist of the highest rank, refused to admit their sensibility. He says: "Plants live and grow; animals live, grow and feel."

This theory recalls that of Aristotle, when the Grecian philosopher affirmed that all organized beings had a soul more or less developed.

To the vegetable soul he attributed two faculties—growth and reproduction. To the animal soul he assigned four faculties—growth, reproduction, sensibility and motion. To the human soul, five faculties. The four above mentioned, to which was added intelligence, or mind.

Neither Linnæus nor Aristotle admit of any sensibility in vegetable life, and yet this is as great an error as to deny the existence of this same faculty in animals. An error, which is almost universal even in the thinking world, and which certainly should no longer be allowed to exist. From the most minute plant, to the most perfect animal, we find sensibility under various forms, but always corresponding to Claude Bernard's definition: "Sensibility is the *ensemble* of all kinds of modifications, determined in living things by different stimuli, or rather, the aptitude to reply to the provocation of these stimuli by means of modifications."

Bichat distinguishes three forms of sensibility:

1. Conscious sensibility, which presides over relations to exterior movements.

2. Unconscious sensibility, representing internal movements.

3. Insensible, or imperceptible sensibility, so called because it is manifested in other ways than by movement.

Putting aside these fine distinctions, let us admit two forms of sensibility—conscious and unconscious—and we shall be able to demonstrate the possibility of a passage from one state to the other, which proves that they are but modifications of a single attribute.

When we learn to read, it is with considerable difficulty, and we doubt if any one ever mastered the art unconsciously. But later, can we not peruse page after page mechanically, without having an idea of their contents? A transformation has therefore taken place in two kinds of sensibility. It is precisely the same with walking and many other acts in which the brain—that is to say, the conscious agent—plays but a secondary part.

If I prick the foot of a frog with a needle the animal draws it away, and, forewarned by the pain, endeavors to escape. Sensibility here evidently assumes a conscious form. If, however, I decapitate the frog, that is, if I destroy the organ which is the *ego*, so to speak, and once more perform my experiment, the mutilated body draws the leg away, but makes no attempt to escape. The act is purely reflex, unconscious, and in this case, by a simple experimental artifice, I am able at once to substitute the second form of sensibility for the first.

We breathe without knowing it, without the intervention of our will; but if our attention is directed upon this mechanical act, we become immediately conscious of it.

In eating, when once our food is swallowed we know nothing more about it, and yet our sensibility is constantly played upon by these substances, which, physically and chemically modified, are introduced into the circulation of the blood, and thence carried to the anatomical elements, whose sensibility they incite to action. All vital properties, and, consequently, sensibility, reside in those little numberless organic unities which go to form living beings.

There exists a fundamental matter, protoplasm, an amorphous substance endowed with peculiar properties and which Huxley has justly termed the physical basis of life. This protoplasm, which sometimes alone constitutes an inferior living creature, not only moves but attaches to itself minute particles which it meets with in the water, digests them and assimilates them with itself. Ether, the great reagent of sensibility, causes it to lose its transparency, and its movements to disappear. Then, when it is evaporated, the fluid reappears with all the attributes of this inferior life. This is undoubtedly sensibility, but in an unconscious form.

If we begin to mount the organic ladder, we see gradually appear certain cells which specify sensibility, and which, created solely to perform this function, elevate and perfect it. These are the so-called nerve cells. They are scattered throughout living organisms; in the higher animals they are very numerous, and serve to centralize impressions and produce individuality. When they are united to others called cephalic cells, they admit not only of sensation, but also the interpretation of sensation which then becomes conscious.

Thus, beginning with this infinite attribute of living matter which Haller and Glisson, being too timid to call sensibility, termed irritability, we gradually come to the highest forms, whence originate the greater portion of intellectual and physiological phenomena.

In man, all the sensible nerve cells are united in one mass called the cerebro-spinal axis, or the encephalo-medullary mass. It is composed of the spinal cord, the medulla oblongata, and the brain, each of its departments representing one form of sensibility. The spinal cord, properly speaking, corresponds to unconscious sensibility. This is illustrated by that involuntary and spontaneous movement which we call reflex action. The medulla oblongata controls sensations which, like respiration for instance, are frequently unconscious, but which, however, by an effort of the will, can be interpreted as precisely the opposite. The brain possesses the highest form of sensibility, and it is here that the greater part of our physical and intellectual acts are performed. By means of the mi-

croscope we are able to-day, to separate in each nervous centre, the sensitive cells from others of a like kind performing different functions which can be recognized by their shape, dimension and situation.

It is useless here to go into minute details concerning this point. I will call attention, however, to the fact that each sensitive nerve cell is connected with exterior agents by a long fibre called the cylinder axis, which resembles a telegraph wire carefully concealed by a layer of fat, and which, surrounded by numerous protecting membranes, extends throughout portions of the body, and produces sensibility. All these nerve fibres, whose receptive apparatus is in the encephalo-medullary mass, are grouped together and form those little white filments which we designate as nerves. If the end of a nerve is touched, or the root, a modification can instantly be determined, which carried to the nervous centres, becomes a sensation. This sensation is, of course, not always the same, but is in accordance with the determining agent, optic, acoustic, gustative, etc.

If for instance, we cut the nerve which conducts light from the eye to the brain, this sensation will immediately be felt; but if, on the contrary, one of the skin nerves be cut, intense pain will be experienced. It is not, therefore, as M. Delboeuf very justly remarks, the nature of the excitation which determines that of the impression, but the manner in which the brain centre is brought into activity; so that if the optic and acoustic nerves be cut, united and inverted, a noise would be interpreted by a sensation of light, and *vice versa*. The sight of a picture would determine sounds in relation to the brilliancy of the paint employed, while an orchestra would produce colors varying, according to the sounds. Sensations experienced in consequence of exterior impressions do not therefore depend upon the character of the latter, but upon the nature of our nervous cells. We do not feel that which occurs upon our body, but only that which takes place in our brain. If, therefore, all our nerve cells were identical, the exterior world would doubtless produce sensations, but they would be precisely alike, merely differing in intensity. There are certain animals which exist in this condition.

M. Helmholtz and other physiologists have calculated the amount of time required for the transmission of the excitation to the sensitive nerves, and have decided upon thirty metres a second—that is to say, a rapidity equal to an express train advancing at full steam power. Imagine a man whose brain is in Paris while the extremity of one of his limbs is in Geneva, and we will see that it must require precisely four hours and forty-four minutes for a sensation to pass from the latter city to the former.

Given the small distance which separates our extremities from the nerve centres, and the time of transmission is short. It is remarkable, however, that those organs which play the greatest *rôle* in the preservation and conservation of the individual, sight and hearing should be placed in close proximity to the brain. This produces a rapid transmission, and enables a speedy evasion of destructive objects—a disposition evidently acquired by natural selection. It seems, moreover, that the intensity of the impression is in accordance with the distance intervening between the excitation and the nerve centre. We may thus explain the extreme violence of neuralgia of the face and head, as compared with that affecting other portions of the body.

All the various forms of sensibility have an analogous basis. The connection and fundamental identity can be demonstrated by the action of narcotics. We shall see that this is the most general and characteristic property of life, and this axiom can be fully established—that everything which lives, whether animal or vegetable, feels and can be rendered insensible.

It is a well known fact that certain plants rebound when they are touched. The sensitive one closes its leaves while a great number of carnivorous plants shut up like traps as soon as a fly alights upon them, imprisoning and crushing the poor insect which is to serve them as nourishment. The action of day and night has been equally verified in regard to plants. Some flowers only open when the sun shines, while others bloom solely in the dark. It has also been seen that the leaves sometimes turn towards the sun, but these phenomena have been called exceptional, many persons even placing them in the category of problematic occurrences, not wishing evidently to open their eyes to facts which they consider humiliating to the animal species.

Now, however, doubt is no longer possible. Ignorance upon this point can no longer be permitted, and every one must know that animals and plants alike possess sensibility. A great philosopher and physiologist, Claude Bernard, first demonstrated this important truth, not by means of tortuous reasoning, but by the brilliant light of experiment.

Provided with an anæsthetic agent, ether or chloroform, he was able to prove that the highest forms of conscious sensibility and the lowest forms of unconscious sensibility can be successfully affected. When the action of the narcotic begins to take place the *ego* sleeps and with it, conscious sensibility. That is sufficient for the surgeon who can then begin to cut and burn without the shadow of an *arrière pensée*.

Upon continuing the introduction of the fumes of ether into the organism, we see all the forms of unconscious sensibility gradually become annulled subsequent to conscious sensibility. After having acted upon the nerve cell, the anæsthetic destroys the sensibility of all the tissues, that is to say their vital characteristic, causing them to react upon exterior agents—in one word, it kills the individual.

If we pass on from the animal to the plant, we find that ether and chloroform act in identically the same manner. Subject the leaves of a sensitive plant to the fumes of either of these agents, and you will be able to handle them without eliciting the slightest movement on their part. They no longer feel the contact of the hand, for knowing as we do that anæsthetics respect the functions of movement, we can only attribute this inertia to the impotency of the excitation.

Let us now take a rapidly germinating seed, such as that of the water-cress and place it upon a sponge soaked in water. In twenty-four hours it will have blossomed into a tiny stem and root. Repeat the experiment under the same conditions of oxygen, water, light and heat, but place the sponge beneath a glass which has been dipped in ether. The seed will remain intact. It is not dead however, it merely sleeps, for if we remove the glass it will recover from its stupor and by the following day will have sprouted. This unseen life possessed by the seed, life which asks nothing more than to make itself apparent, is, however, subject to external and internal conditions. The first are the necessity of water, oxygen, heat and all physio-chemical conditions; but there is still something else, internal, inherent to the seed itself and constituting the essence of its life. It is sensibility. Destroy this function and notwithstanding the most favorable surroundings, the development will be effectually stopped.

Do not think that this is owing to any peculiarity of the plant and its embryonic condition, for a hen's egg, that latent condition of life of an organism belonging to a comparatively high order, cannot be hatched with any desirable result in an etherized atmosphere.

Germination, the first vital act of the individual, be it plant or animal, is therefore subject to sensibility and in this function we see it appear for the first time. Afterwards it is not difficult to follow it in its course through all the vital acts of the organism. The plant breathes and grows by assimilation, absorbing either the substances contained in the earth, or the carbonic acid in the air. For a long time this gaseous assimilation was

confounded with respiration, and the mistaken conception was spread abroad that plants breathe in direct opposition to animals by absorbing carbonic acid and exhaling oxygen. By means of anæsthetics we can separate these two phenomena. An aquatic plant placed in etherized water ceases to absorb carbonic acid and emit oxygen. It however, remains green, and breathes as animals do, a phenomenon which existed before, but was hidden by the assimilation of the carbon; still, further back, we can encounter one of those phenomena long considered chemical and which nearly escape vital acts inasmuch as in the laboratory some of them can be reproduced without the aid of life. I speak of fermentations. These are produced by a microscopic fungus, which decomposes fermentable matter, nourishing itself with a portion, while the remainder forms a new product which stays in the liquid. These fermentations, in spite of their extreme tenuity and their inferiority in the organic scale, are susceptible of being stupefied by ether and losing their active power. We may place them with impunity in close contact with the liquid, but the latter remains undisturbed.

Thus, from the very bottom of the ladder, from the simplest protoplasm, and the most insignificant fermentation to the most elevated creature to be found on the earth, we find always the same characteristic and fundamental property of life, modified, it is true, to a degree which forces us to follow the thread of its diverse forms step by step, but always identical in substance, and invariably demonstrable by those infallible reactive agents, anæsthetics. Without this property there can be no life, or rather no active life, no exterior manifestations. With it, any plant or animal, no matter how simple in construction, develops, grows, prospers and reproduces itself. It is easy to see, therefore, that sensibility is the principal attribute of all organic beings, and in some way the cause of everything that takes place within us. If, as Condillac says, we should take an immovable and insensible image and endow it gradually with all our senses, it would soon rise from nonentity and begin to augment the sphere of its knowledge. By giving it the sense of hearing, we open that vast field of observation and reasoning which procures sound, but it could form no idea of the existence of matter, or of sunshine, or of taste. It could only conceive one thing, until put in complete possession of the other senses.

Intelligence, that precious gift which alone renders us superior to other creatures, is, therefore, nothing more than the result of our accumulated impressions, controlled one by the other, and we may even affirm that the man who has felt is alone capable of thought. The development of our minds should be adequate to the development of our sensibility, and in fact, it can be observed everywhere, that those persons whose senses are the most refined, possess the highest form of intelligence. I may even go so far as to parody the famous proverb and say to my neighbor; "Tell me what you feel, and I will tell you what you think."

Not so very long ago, as we have seen, Linnæus refused to admit of sensibility in regard to plants, saying that it was an attribute of the animal world only. An attentive investigation, however, causes us to reject such distinctions to-day. Let us even go further back, leaving behind us the lowest forms of organic matter, and see if any phenomenon approaching sensibility is to be met with. In a word, let us ask the following question: Is matter sensible?

Referring once more to Claude Bernard's definition of the term, "sensibility is the *ensemble* of all kinds of modifications determined in living beings by different stimuli," we find no possibility of its application to the properties of matter, for it distinctly states that the condition is an attribute of living beings only. But a mere definition should not arrest our investigation, for it is nothing more than the result of knowledge hitherto acquired, and as such admits of change. The substance of it all amounts to this; given a living being placed in immediate contact with matter, and the matter will act upon the being, producing sensation. But how do we know that the living being does not in its turn act upon the matter and modify its condition? I will even affirm that life does act upon certain substances, for fermentation is a positive proof that this is the case. If I place a sweetened solution of wine in contact with the air, a short time will suffice to develop therein millions of tiny living creatures proceeding from atmospheric germs. This fermentation increases with great rapidity, producing a chemical effect, so that after a certain time the sugar will be transformed into carbonic acid and alcohol. The presence, therefore, of life in the liquid served to modify the properties, and in this we see one of those strange occurrences where the so-called vital forces are so closely allied to chemical processes, that we hardly know whether the phenomenon is the result of the biologists' skill or the chemist's. Each of these *savants* have claimed it as their own, and with reason too, for chemistry and biology are twin sisters who can never quarrel.

When the sugar is once transformed into alcohol another organism appears, which, in its turn, determines the transformation of this substance into another, acetic acid, by means of an analogous fermentation. It is a remarkable fact, however, that while the chemist has, as yet, been unable to produce alcoholic fermentation by means of the action of matter upon matter, he can, on the contrary, easily determine the second without the aid of life at all. It is, therefore, the presence of these bodies which acts, and not the construction of the fermentation. It is not that life decomposes the liquid, but that the liquid decomposes itself when assimilated with certain agents. It is therefore sensible of their action.

Once *en route*, it is not difficult to multiply examples and to demonstrate that light, heat, electricity and all other forces which operate upon our sensibility, are uniform modifiers of matter. What is a photographer's negative but a glass plate sensitive to the action of light? Is not a piece of wire about which we pass an electric current sensible of electricity, inasmuch as it acquires thereby a new property, that of attracting a like piece of wire? It becomes, in fact, magnetic.

Heat, as we can observe every day, modifies bodies to such an extent, that beneath its influence they liquify and evaporate. All these facts demonstrate clearly that matter is sensible of exterior agents. According to the second part of Claude Bernard's definition, it possesses the "aptitude to reply to the provocation of these stimuli by means of modifications."

Consequently, universal attraction, that law which affirms that all bodies attract each other in direct ratio to their mass, and in inverse ratio to the square of their distance, is merely a simple and general way of expressing the sensibility of matter.

CONTRIBUTION TOWARD A NEW COSMIC HYPOTHESIS.

By Samuel J. Wallace.

Our familiar knowledge and ideas in astronomy relate generally to matter in large bodies, and in great numbers of small bodies, which now and then fall into the larger as meteorites. This seems to show a condition of slow centralization, as if to finally collect all matter, however far distributed, into a few large bodies. And a consistent conception requires in its plan, somewhere, a means of decentralization, or distribution of matter through space again, to form a closed system of action.

Gravic force as one of the interchangeable forms of kin-

etic energy also requires a starting place; some way by which the energy continually changed into mass motion and heat of bodies gravitating into each other shall be changed back again into gravic force.

I think the luminously hot gaseous nebula shows the earlier stages of these two required dispersions taking place together.

The fall of matter from space into central masses takes up just so much gravic energy. This is changed into mass motion and heat, so that on mechanical principles, each particle bears just enough force still to carry it back into space again.

This leads us up to the idea of the life of a particle in the universe, as being through an endless series of cycles of change from space into masses and from masses into space, each being, as it were, almost an eternity in duration, making a grand orbit, something like this:

Meteoric matter falls from space into planets and suns; planets fall into suns; suns grow continuously larger, accumulating the momentum of particles acquired in falling from space, as motion and heat; the heat of overgrown suns at last becomes so intense, from accretions and collisions, that it flies into a still higher form of repulsive energy, as gravic force, causing disruptive explosions; the particles, by this change into gravic force outward, receive a projectile force carrying them out into space again; and particles from space fall again into planets and suns.

This forms a closed cycle of action. The projectile and outward gravic forces may carry part of the particles forward into space till they become parts of other systems to run like courses; and part may be driven back at last to rebuild another system instead of that destroyed. Some might be driven back by the extra-force given others, and form nuclei of one or more suns. And the disrupted parts going in different directions might form a number of nuclei, having such dispersive motion as to carry them continuously apart, as in some reported cases.

The history of a nucleus having such an initial projection might be something like this:

I. It might pursue an interminable course; should this lead laterally near any region having matter in great quantity, it would be deflected to pursue a great curve, attracting to itself distributed matter within its reach, from gravitation.

II. The growth of a body pursuing a curved course would produce in it a rotary motion, which would be the resultant of the course and force of its own prior parts and those of its continued accretions, and of the combined attractions on its several parts of all matter passed, which of course would predominate on the inner side of its curved path.

III. Matter drawn into a moving body would come in curved lines and spiral courses, tending to form great rings and to produce planets, whose courses and velocities would be the resultants of the total averages of those of the various particles uniting to form each of them.

IV. Secondary bodies of large size would approximate primaries in the nature of their actions, falling in by slow degrees: those of long life as secondaries tending to eventual motion in one general direction, from having similar producing forces and from their actions on each other neutralizing conflicting equivalents; giving to each such position as its resultant velocity leads to, by union or separate course, together with revolution, and possibly secondaries to them.

V. Differences of inclination of orbits of secondaries may occur from the course of primaries having been at some time near regions having predominant quantities of matter to be drawn in on special sides, which unites to form bodies having their special courses; and, from analogous causes some smaller bodies might chance to have courses contrary to others.

VI. The four terms of orbital and rotary velocity, heat, and loss from radiance and from a resisting medium, would form a sum equal nearly to the energy of fall from infinity to the mean position of each body, qualified by whatever initial velocity and direction each particle may have had from its prior projections.

VII. The distance of each body from its primary would depend on its velocity in proportion to that which fall from infinity would produce; and all causes changing orbital into rotary motion, into heat and radiance, and into friction on a resisting medium would shorten the mean distance; but add to the velocity.

These I think are about the things which would occur, stamping the history of the system upon its internal peculiarities.

Now, does this sort of hypothesis come nearer accounting for the solar system than the original or present day forms of the "nebular hypothesis?"

I think that requires far too extravagant a conception of a common initial force taking possession of the matter thinly distributed through so immense a region as a globular space extending far out beyond the orbit of Neptune; and that it does not account for the variations of detail, nor for a final closed system of action, as does this.

I have no faith in this stuff about burnt out worlds, thinking that planets and suns grow warmer instead of colder as they slowly grow larger from fall of meteorites, as now going on.

Gravic force, which causes gravitation of bodies toward each other, probably has some resistance and loss in passing through large bodies; which would heat them and give ample source for all extra heat in the sun and large planets, and the interior of the earth; and would give ample time to geology for all its work. In fact, it may thus greatly increase the stores of heat beyond that given off by radiance, and that gained by falling from space; and so assist in forming the store for final dispersal.

The analogies of the other forces in passing through massive media seem to force us to expect such loss causing heat; and there is reason to suppose there is a slight resisting medium to matter in space: the dynamic equivalent of this loss of gravic force, in the life of the particle.

Without a resisting medium to matter in space it is hard to understand how matter can congregate into masses instead of always flying off again with equivalent velocity.

And it is equally hard without a resistance to gravic force in bodies to understand how gravic force can act to cause gravitation, or how the particles in a fixed body, radiating its force, can ever again get energy for dispersal into space.

But the arrangements may be so wonderfully balanced that the one propulsive gravic force may bring particles together from far space to form solid bodies and suns, and then when their course is run, store up energy in them to carry them back into space again, to go onward and build up new systems of glory.

The mountains of the moon, which have been called dead volcanoes, are simply the drop marks of great meteoric masses falling into the light meteoric dust forming in the outer part of the moon. They are similar in their peculiar forms to the rain drop marks familiar to geologists in sand-stones; and have the same peculiar raised rims and concavities, with the bodies which produced them still standing in their centres.

Some seem to have exploded like shells, sending masses in various directions tearing great furrows, some radiating across the whole face of the moon.

They may represent an enormous period, as the absence of wind and moisture would permit marks once made to remain until obliterated in a like way; water, if there, being a frozen dust, and if ever melted sinking into the deep porous mass.

AMERICAN CHEMICAL SOCIETY.

The regular meeting of the American Chemical Society was held Friday evening, May 6, Vice-President Leeds in the chair. Messrs. J. G. Mattison, Theo. Tonnelé, and Dr. Otto Grote were duly elected members, and Messrs. A. H. Van Sinderin and C. P. Sawyer as associate members. Mr. A. P. Hallock was proposed for election. The first paper on the programme was "On a Slight Modification of the Wilkinson Gas Eudiometer," by Mr. James H. Stebbins, Jr., S. B. Having found considerable difficulty in the manipulation of the instrument as described by Dr. Wilkinson, Mr. Stebbins succeeded in overcoming his objections by bringing the stopcock nearer to the Eudiometer itself. It is very difficult, in fact impossible, to properly explain the improvement without illustrations. At the conclusion of Mr. Stebbins' paper, Dr. C. A. Doremus very thoroughly explained the method of procedure used by Dr. Wilkinson in his working of the Eudiometer. This made the matter clearer, still the improvement by Mr. Stebbins was thought desirable.

The next paper* was by Dr. T. O'C. Sloane, "Note on the Purification of Baric Sulphate." The authon finds in order to obtain a precipitate of barium sulphate that will not run through the filter, a few rules must be observed. These he gave as follows: 1st. The solutiou must be barely acid. This end he secures by using cochineal, finding by its use that the neutralization can be more expeditiously and exactly performed than with litmus. 2d. The precipitant is added when the solution is almost up to a boil and kept at that temperature for some minutes. By following these two suggestions a heavy precipitate with a perfectly clear supernatent liquid will be obtained. In case any iron salts have been carried down with the barium sulphate, the precipitate is to be treated first with hydrochloric acid and secondly with sulphuric acid, but this process is open to some objections. It is therefore best to fuse the precipitate with sodium carbonate and a very little sodium nitrate and redetermine the sulphur. As an improvement, Dr. Sloane finds the following method quick and reliable: The sulphur is precipitated in the conventional manner with the previous mentioned precautions carefully observed. The solution is then decanted to the last possible drop through a filter paper; 5 or 10 c.c. of conc. hydrochloric acid are then added and the beaker held in the hand over a hot plate until the acid is brought to a full boil. It is allowed to continue so for a few minutes, then cooled and diluted. The liquid neutralized with cochineal solution, re-acidified and poured into the filter. By this manner a white and clean precipitate was obtained. Dr. Sloane immediately followed with a description of a new "Qualitative Test for Carbon Disulphide and Carbon Dioxide in Coal Gas." A piece of caustic potash, a few m. m. long, is added to ten or twenty c. c. of alcohol, into which a piece of potassium carbonate has been added. The alcoholic solution of potash is placed in a suitable absorption tube and a cubic foot or more of gas passed through it. It is then removed from the absorption apparatus and poured into a test tube. If the gas contains any carbon dioxide, an oily looking layer, nearly colorless, of a solution of potassium carbonate will underlay the alcohol, which latter will have acquired a reddish color. The alcoholic solution, which, if any carbon bisulphide be present, will contain potassium xanthate, is boiled and tested for hydrogen sulphide. Another method, is to add an excess of a copper salt, filter out the precipitated copper compounds and pour ammonia through the filter paper, when a highly characteristic yellow precipitate of copper xanthate will remain behind.

The fourth paper was by A. R. Leeds, Ph. D. Its title was "Upon the Direct Conversion of the Aromatic Amides into their corresponding Azo-compounds."

This paper was a sketch of the recent work which Dr. Leeds has been prosecuting in his laboratory at the Steven's Institute. It consisted, as described in the title, of the details incidental to the conversion of the different aromatic amides into the corresponding azo-compounds with the peculiarities of each commented on. Many of the hydroxylated compounds were also operated on by Dr. Leeds.

Mr. A. A. Julien followed with a very interesting paper "On the Chemical Contents of the Fluid Cavities of Minerals." Mr. Julien is the well known lithologist of the School of Mines in this city, and has a higher reputation in this specialty than almost any other scientist in this country.

He first gave a general outline of the history of the subject. It is only comparatively recent that any attention has been paid to these cavities, which are very minute in size and generally of a rounded shape, though sometimes following the outlines of a crystal, that is to say, the cavity is of the same shape as a crystal of the substance in which the cavity occurs. New York is, for many reasons, the best place to study this subject; for instance, a greater number of specimens find their way to this city. Among the substances found in these cavities are: water, carbon dioxide, nitrogen, sulphur dioxide, ammonia, fluorine, chlorine, oxygen, hydrogen disulphide, and rarely bituminous and light hydro-carbons.

Herkimer, N. Y., is a locality where the latter are frequently found.

Carbon dioxide is, however, the most interesting of these substances to the chemist, and it is also the one most frequently met with. It is found in some fifteen localities throughout the United States. One locality is known in New York State. These cavities are generally found in granites, granito-porphyries, hornblendic and other gneisses and in smoky quartz. The most characteristic feature of the carbon dioxide in the cavities is its remarkable expansive quality—so great that in touching it the warmth of the hand will completely vaporize the liquid compound. Mr. Julien has devoted special attention to the determination of the temperature at which the liquid expands and for that purpose has devised a form of apparatus to be used in such estimations. The piece of mineral containing the cavity is mounted on a microscopic slide and placed in the new apparatus, which consists of a long metallic box, with a small tube on the surface, to which a rubber tube is attached. The whole apparatus is placed in the microscope. On blowing into the rubber tube sufficient heat is obtained to cause the expansion of the bubble of carbon dioxide. Readings are made of the temperature at which the bubble disappears and also of the temperature at which the bubble reappears. Mr. Julien's results agree within two tenths of a degree Fahrenheit.

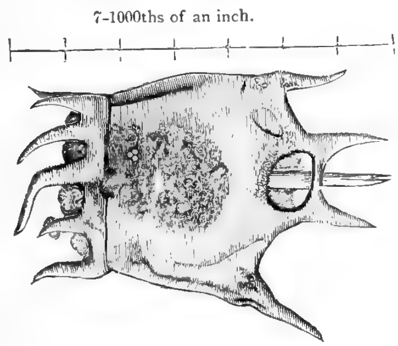
Thus by the ordinary method (Fuessi's) two results, 80.1 and 79.5 were obtained, while with the improvement six results were obtained as follows: 79.6, 79.4, 79.6, 79.5, 79.5 and 79.6. Mr. Julien also gave a very interesting description of "Reticular Fluid Cavity" in Topaz from Brazil, whose bubble was the largest ever discovered, being 2.28 m. m. in length. He also referred to the spontaneous motion observed in the bubbles and to the general bearing of the entire subject of the gensis and formation of rocks. M. B.

* I would acknowledge my indebtedness to Dr. Sloane for his kindness in lending me his original MSS.—M. B.

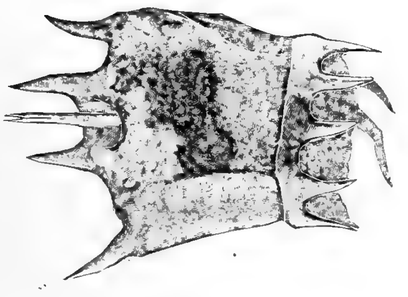
M. DUCHEMIN, the inventor of the compass with circular magnets, now adopted in the French navy, has lately devised, for correction of compasses, a system of magnetic compensators, in which magnetic bars of annular or circular form are used in place of the straight ones. These have the advantage of insuring much greater magnetic stability than straight bars, especially when lightning occurs in the neighborhood of the ship.

A NEW ROTIFER.

In a filtering of Hemlock lake water (Rochester's water supply) made in August of last year, I noticed a rotifer that at once struck me as different from any that I had before observed or seen described. On classification it proved to be a Brachionus, and a diligent search through the somewhat scattered literature on the subject has since failed to satisfy me that this form has ever been described.



DORSAL.



VENTRAL.

BRACHIONOUS CONIUM.

The Micrographic Dictionary uses the classification of Ehrenberg, while Carpenter in his work, "The Microscope and its Revelations," adopts that of Dujardin. While all classifications of the Rotatoria thus far made are in some ways unsatisfactory, that of Ehrenberg seems the least faulty, and according to it I find that this organism, by reason of having its rotary disk divided into two parts (*Zygotrocha*) and having a carapace, would show that it belongs to the family "*Brachionœa.*" There are five genera in this family. The Brachionus has one eye-spot and forked foot, and to this genus the rotifer unquestionbly belongs: "*Brachionus Conium.*"

Lorica irregularly truncate, slightly reticulated over entire surface except the collar carrying frontal spines; this latter portion has a hard vitreous appearance.

Ten frontal spines, the middle one on the dorsal surface longer than the balance and describing almost a right angle turn near its center to one side. This spine half as long as the carapace of the rotifer. Eye-spot prominent. No openings on dorsal surface of carapace.

Four posterior spines, one at either extreme side and one on either side of anal opening. Tail or foot, slender and bifid. Extreme length of rotifer including anterior and posterior spines, seven one-thousandths (7-1000ths) of an inch.

Unfortunately a dead specimen had to be used for the drawing, hence no definite description can be given of mouth parts for internal structure. The external appearance is, however, so strikingly characteristic as to serve all purposes of identification until the internal structure can be fully described. H. F. ATWOOD.

MICROSCOPICAL NOTES.

The subject of standard screw gauges was recently brought before the R. M. S., and the question of the accuracy of 50 duplicates, made for distribution, was discussed. Mr. Bevington considered "they were as near the standard as could be expected." Mr. Beck pronounced them on trial to be defective. It seemed to be conceded that the original "taps and dies" had been lost, but as Mr. Crouch thought that the present set of duplicates was sufficiently perfect for all practical purposes, we suppose opticians must rest and be thankful for what they can obtain. Considering the deterioration, which must occur from the wear and tear of the cutter, it is to be regretted that perfect accuracy cannot be given to the standard gauges issued by this society.

On the presentation of a paper by Mr. Shrubsole on the "*Diatoms of the London Clay,*" the President, Professor Martin Duncan, made the following interesting statement on the subject. He said that "those who studied this class of subjects would be greatly interested in the paper which had been brought before them; and no doubt had it been read before the Geological Society, there would have been considerable discussion upon it. The London clay had at the bottom of it large beds of pebbles; these were all water-worn, and clearly indicated an old shore. Just above this, on a sinking shore like it, would be precisely where they should exexpect to find diatoms. But the London clay just above this became a little more marine, and this fact would acccunt for their not finding these fresh-water forms there also. Then it should be remembered that the occurrence of diatoms was subject to great variations, and that they were always found in greatest abundance in the neighborhood of silicious rocks. As regarded their age, he thought there could be no doubt that they lived at the time of the Lower Eocene. There were, however, some peculiarities about the London clay, there being no other strata which were deposited under the same conditions, because it was not a reef deposit, but it positively told the story of an open estuary leading down to a very large river. This was one reason why they would not find the diatoms in similar deposits in Italy or Wales. It was not an uncommon thing to find that in other fossils the carbonate of lime was replaced by sulphide of iron. Phosphate of lime was often also replaced by sulphide of iron, and the interstices of other fossils were often found filled with the same substance, which was an exceedingly common mineral in the London clay. Silica was not the difficultly-soluble substance which it was formerly thought to be, so that its place could be as easily filled up by any other mineral which was less soluble than itself—from which consideration he thought the matter might be explained. But when they came to the question of antiquity, it was not so easy to give an opinion as to whether Count Castracane's diatoms in the Carboniferous series were with good reason thought to be diatoms. In the Tertiary of course they found them; but if Count Castracane's propositions hold good, we ought to be sure to find them in the intermediate series."

Mr. Shrubsole said Mr. Kitton's idea was that they were fresh water diatoms which had been washed down into the coal-beds.

The President expressed himself unable to accept such a suggestion.

LAST week the Whittaker Court Marshal was continued, and Dr. Piper of Chicago, was examined as an expert on Microscopy. In cross-examination questions were submitted to the witness on the construction of the Microscope, which Dr. Piper admitted were beyond his knowledge. One question related to the composition of the glass used for the construction of lenses for the Microscope.

Possibly few Microscopical experts could answer

questions on this point, and we would be obliged if any of our correspondents would furnish a few facts. We understand that some makers make use of a very soft glass, the surface of which becomes defaced in a short time: a dealer calls this "*spongy glass.*" We would like to know where the respective makers purchase glass for objectives, and its composition.

ASTRONOMY.

COMET (*b*) 1881.

Prof. Barnard, of Nashville, Tenn., announces the discovery of a comet on the morning of May 12, 1881, in R. A. $22^h\ 59^m\ 18^s$, dec. + $14^\circ\ 24'\ 30''$. An observation on the following day gave R. A. $22^h\ 58^m\ 52^s$, dec. + $14^\circ\ 36'\ 0''$, thus indicating a motion 24^s in R. A., and $11''$ in Dec. The comet is reported as very faint.

ON THE USE OF THE ELECTRIC TELEGRAPH DURING TOTAL SOLAR ECLIPSES.*

If we suppose a single observer to be prepared for the observation of all the total solar eclipses of a century, we shall find that the entire amount of time during which he may contemplate the totally eclipsed sun will not differ much from an hour. We may be sure then of the expediency of any scheme whereby the rare moments of these eclipses may be utilized to their utmost extent. If such scheme is devised, two important results are likely to follow.

(1.) Economy of the sum-total of energy in any particular line of solar research.

(2.) A consequent enlargement of the means of research in other directions.

The general conception of the scheme proposed in this paper may be very briefly stated: Suppose a station to the east and a station to the west on the line of any total eclipse, as widely separated as practicable, and equipped for similar observations of discovery during the progress of the eclipse; the method proposes the electro-telegraphic transmission of important observations made at the western station to observers at the eastern station, with due speed for their verification or rejection when the lunar shadow reaches the latter station.

For illustration, consider the next total eclipse,—that of 1882, May 16. In detail, the particular advantages in connection with this eclipse seem to be about these:

(1.) The path of totality is almost exclusively on land. Central eclipse begins in West Africa; the line of totality passes to the north-east, crossing Upper Egypt and the Nile at El-Akhmym; thence over the Red Sea, crossing the Tigris a few miles to the south of Bagdad; then passing a little to the south of Teheran, it traverses Central Asia, and leaves the Asiatic Continent somewhat to the north of Shanghai.

(2.) Though not generally through the inhabited regions of the globe, the path of totality lies through several inhabited regions which are widely separate, viz: Egypt near the Nile, Central Persia and Eastern China.

(3.) These regions are inter-connected by telegraphic cables and land-lines.

Now, we will suppose that an important observation of discovery is made at El-Akhmym,—an observation of an intra-mercurial planet for example. Between 40 and 45 minutes of absolute time elapse before totality comes on at Teheran. During this interval the observer at El-Akhmym will have an abundance of time for transcribing the apparent magnitude and the precise position of the new body, and transmitting the same to his fellow-observer at Teheran several minutes before the lunar shadow reaches him. The latter observer will then have leisure to proceed with the setting of his circles, the verification of their readings, and the pointing of his instrument to the precise part of the heavens indicated. He may then be able to see the suspected object before the eclipse becomes total. He may also decide upon a neighboring star for comparison with the planet, and thus obtain a very accurate determination of its position. The observer at Teheran should also be prepared for an independent search for the suspected planet, in the event of receiving a negative message from the observer at El-Akhmym.

The observation at El-Akhmym should also be transmitted to Shanghai, (reached by the shadow more than two hours after totality at Teheran), for independent verification at that point. We might thus observe the result of nearly three hours' motion of the planet,—which we might reasonably expect to give important data in regard to its orbit about the sun. Of course, the result of observation at Teheran would also be transmitted to the observer at Shanghai.

It was my intention primarily to have considered the total eclipse of 1882 merely as an illustration of the method proposed. Further investigation, however, seems to show that it is at least one of the two most favorable eclipses during the present decade, if not during the present century.

WASHINGTON, *May* 18, 1881. W. C. W.

*Abstract of a paper by D. P. Todd, M. A., presented before the American Academy of Arts and Sciences, Jan. 12, 1881.

COMET (*b*), 1881, BARNARD.

To the Editor of "SCIENCE:"

On the morning of the 12th, while sweeping the eastern sky in search of comets, at about three o'clock, an object entered the field of my telescope which I strongly suspected was a comet, as I did not know of any nebula near its place. I at once secured its position relative to *α Pegasi*, it being in the field with that star. Its position at seven minutes past three o'clock was:

$$\text{R. A. } 22^h\ 59^m\ 18^s$$
$$\text{Decl. } + 14^\circ\ 24'\ 29''$$

The object was watched at intervals until about four o'clock, when daylight prevented further observation. During this time no motion was detected. Wishing to confirm the discovery by a second observation, before announcing it, I waited until the following morning, when upon turning my telescope to the point where the object was seen, I found it had disappeared.

No doubt now remained in my mind of its cometary character. I began a search to re-discover it. After sweeping for some time in the immediate neighborhood, I found it again as day-light was whitening the sky. It was very close north following *α Pegasi*. The object was then only visible when the bright star was obscured by a part of the ring suspended in my eye-piece. It followed the star by six seconds and was therefore in R. A. $22^h\ 58^m\ 52^s$. I estimated the difference of declination between comet and star, and found it to be in north declination $14^\circ\ 36'$. No doubt now remaining that it was a comet I telegraphed its position to Professor Swift, Director of the Warner Observatory at Rochester. On the morning of the 14th I again began a search as soon as the object had risen above the horizon, but it could not be found. At first I attributed my not finding it to its low altitude and the bright moonlight. The search was continued until daylight, and I was deeply mortified at not finding any trace of the object. In the morning telegrams from Rochester and Boston announced failure to find it at those places.

A short search this morning, when the sky had cleared, at about day-light, resulted no better than yesterday morning. The object on the 12th was slightly smaller than Swift's last comet, which I had been observing on the 11th, and was probably a little brighter. On the 13th it

appeared very faint. This I attributed to its proximity to the bright star.

I shall continue the search for it. The moon will leave in a few days and I then hope to be able to see the comet again. E. E. BARNARD.

May 15, 1881.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

I should have attempted a reply to the restrictions of Mr. Dopp before this time if I had not had my hands too full of other work, but lest any might think I have nothing to say if an answer of some kind did not shortly appear, I will ask the favor of a little space, and first I entirely disclaim the pretension of undertaking to reconstruct Physical Science which Mr. Dopp seems to impute to me, and whatever was put forward as new was only hypothetical, and perhaps I was not guarded enough in specifying it as such. Yet there is more that may be said for some of the statements made than appears in those papers, which were very brief and did not pretend to give references. But now if I shall deal with the subject of internal and external energy which is attacked in the last part of Mr. Dopp's paper, it will save saying very much about the first part.

Mr. Dopp quotes from Maxwell's works on Heat, and says they disprove and invalidate all my calculations. But it will probably be allowed to hear Maxwell in 1875 against Maxwell of 1872:

"In 1860 I investigated the ratio of the two parts of the energy on the hypothesis that the molecules are elastic bodies of invariable form. I found, to my great surprise, that whatever be the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of two parts of the energy must be always the same, *the two parts being in fact equal.*" He also says a few lines beyond when speaking of the researches of Boltzmann, he "makes the whole energy of motion twice the energy of translation." See Nature, volume 11, p. 375. This language justifies my work and my calculations are not invalidated. What is to be understood by $E' - E = \epsilon$, is their difference and not a ratio, and the expression in the paper is wrong, but there is nothing in the paper that depends for its correctness upon any mathematical expression in it, whether it is right or wrong, and cannot be raised against it. That is to say, there is nothing in the first paper that is a deduction from any mathematical work given.

As to my definition of ether as not matter, again Maxwell is quoted against me, and I will therefore again quote Maxwell in my favor. "According to Thomson, though the primitive fluid is the only true matter, yet *that which we call matter is not the primitive fluid itself but a mode of motion of that primitive fluid.*" See Art. *Atom* Enc. Brit., 9th Ed. The italics are mine, but if it does not plainly make a distinction between ether and what we call matter, then I don't understand it. But I claim more, that to call ether the primitive matter is to call two different things by the same name, and my first paper was a protest against that. Newton's law of Universal Gravitation states that "every particle of matter in the Universe attracts every other particle of matter," and until it is discovered that ether possesses this property of attraction, I hold that the name *matter* should not be applied to it. If, however, any one thinks it to be a proper use of words, I shall not quarrel with him, only when he talks to me of matter I shall need to ask whether he means gravitative matter or non-gravitating matter. As for the objection that I use the term density applied to ether and am therefore to be held to what is implied in the word; any one who undertakes to express a new conception must either employ words that have some fixed meaning or else coin some new word which in its turn must be defined with old words. So while the term density conveys my meaning in a tolerable way, I do not wish to have it imply that density in ether and density in matter are identical. In the same article on Atoms, Maxwell says concerning the vortex-ring theory: "We have to explain the inertia of what is only a mode of motion," and this is in strict accordance with all I have written about it.

We do know that the motions of atoms set up corresponding motions in the ether, and it is not difficult to perceive how it may happen, though the particular mechanical conditions may not all be known. *Assuming that the conditions are mechanical,* then the analogy of the vibrating tuning fork is not so far fetched as it might be. I do not see the necessity for my being held to atoms combining in only one plane. It is as easy to see that three or four or more could all unite at the same place so as to form a radial structure or a triangular one when one of the two represented in the diagram should swing round 120°, which, so far as I can see, would not imperil its stability at all, and it then would be in position for another similar atom to unite with each, and so on almost any kind of a geometrical solid made. But I did not intend to assert at all that in this hypothesis there was anything more than an idea. I am not ignorant of the molecular form of ordinary matter, but my assumption was that the molecular form was due to its vibratory energy, and, consequently, I was mostly treating of atoms, and the statement was made that at or near absolute zero the chemical affinity was *nil*, and hence dissociation. This is plainly the case if chemism is due to heat vibrations, but it is corroborated by mathematical calculations. In a paper read before the American Academy, in February last, by Mr. D. E. N. Hodges, of Harvard College, but which has not yet been published, the same conclusion is deduced from thermo-dynamic considerations, namely, that at absolute zero "there can be no cohesion of molecules, and probably the same for atoms; it is the temperature of dissociation." Mr. Dopp quotes from Professor Tait what he knew about the phenomena of vortex-rings, but since Mr. Dopp's paper was written he has probably heard of some more phenomena of vortex-rings. See "SCIENCE," April 16th.

As to the paper on Atoms as forms of Energy the *idea* is not mine, but Thomson's, and whether or not the method therein shown of computing atomic weights is mathematical jugglery, as Mr. Dopp calls it, all I have to say is, I did not stake anything upon it. I thought if matter is a form of energy, the fact should appear in atomic weights, and so I made the calculations and published them, and if anyone thinks they signify nothing, why I will not quarrel with him. After so long a paper finding fault with anything I had written, it was something of a pleasure to read that he thinks my theory can be made "a fair working hypothesis to explain adhesion, cohesion, and even crystallization,—surface tension of liquids and capillary attraction, and possibly those of osmosis, dialysis and occlusion."

This is not an unworthy stock of phenomena to explain, and if what I advanced can not be made to do all I proposed to have it do, I might be content if it explained in a fair way any one of the above phenomena.

A. E. DOLBEAR.

COLLEGE HILL, Mass., *May* 10th, 1881.

To the Editor of SCIENCE:—

As two of your correspondents, Mr. A. E. Dolbear and Mr. George W. Rachel, have adversely criticized certain points in my article in the April 9 number of "SCIENCE, and as I still consider my position as stable, I must request a limited space to reply to these gentlemen.

The main difficulty seems to be that I have gone

counter to certain authors whom they are disposed to consider as authorities. But, in my view of the case, Science has no authority, except the authority of facts, and theoretical views are always fair food for criticism. Mr. Dolbear quotes from Clausius to the effect that "all heat existing in a body is appreciable by the touch or the thermometer; the heat which disappears * * * * exists no longer as heat, but has been converted into work." Heat is undoubtedly appreciable, but not necessarily measurable, by the touch or the thermometer. As the heat capacity of any substance increases its temperature effect for equal volumes of inflowing heat diminishes, so that the thermometer fails to indicate the exact quantity of heat which a substance receives in passing through a fixed range of temperature. It is customary in late authors to speak of this apparently lost heat as converted into work, or, in other cases, to speak of it as changed from actual into potential energy. This is, undoubtedly, a very convenient way of getting around the difficulty; but, with all due deference to the distinguished writers who advance this hypothesis, I venture to question if it is a strictly scientific way. To come plump up against a difficulty in your path, to explain this difficulty by a nicely sounding word which explains nothing, and then to go swimmingly on, enables one to get over a great deal of ground in a short time; but it is very apt to leave stumbling blocks for those who come after.

I should certainly like to see a precise definition of the word "work" in this connection. Heat produces a certain effect. That effect is called work. But the important question remains, what has become of the heat? It was a motion. Has it ceased to be a motion? If so, then motion can cease to exist. Yet I hardly think any scientist will admit such a possibility. But if it has not ceased to be motion, where is it? Is the word "work" advanced as a name for some new mode of motion? Whether it is or not, however, it fails to explain what has become of the heat. We meet with a like difficulty in the theory of the conversion of actual into potential energy. Actual energy we can readily comprehend; it is the energy of the motion of masses. But what is potential energy? It is a possibility of mass motion. A body rests upon the earth. It cannot possibly descend further. It has no potential energy. A body is suspended in the air. It may possibly descend further. It has potential energy. Potential energy then, is possibility of motion. Actual motion has been converted into possible motion. If this amounts to more than the explaining of a difficulty by a meaningless phrase, I should certainly be glad to have some one scientifically explain the explanation. I must quote from my former article: "Motion is motion and cannot possibly be or become anything else." Actual motive energy cannot cease to exist, and be replaced by an abstract possibility of motion, called potential energy.

In regard to Mr. Rachel's remarks on my views respecting variation in heat capacity, he must permit me to correct his quotation. He quotes me as saying: "Temperature and heat are very different things." I find my expression to be: "Temperature and *absolute* heat are very different things." There is a considerable difference of meaning between these two expressions, which it would have been well for him to give me credit for. The main difficulty in the minds of both my critics seems to be a somewhat confused idea as to what constitutes heat. Mr. Dolbear claims that the free vibration of molecules is not heat. In this he certainly disagrees with most authors. Mr. Rachel states that "latent heat is not heat." He intimates that it is work, but will he be kind enough to explain scientifically just what work means in this connection? He says further, "Water does not contain more heat than ice at 32°; it contains * * * more motion, but not motion of the heat kind." Of what kind then? "Nor is it true that as density diminishes the heat capacity increases." The heat has disappeared as heat, "but it nevertheless exists in the gas as a greater range of mobility."

We here get his definition of "work." It is "motion, but not motion of the heat kind;" it is "a greater range of mobility." Motion, then has not ceased to exist, and we have been splitting hairs about nothing. It is molecular motion, but not the special mode of motion which he calls heat. Yet it would be well to bear in mind that scientists are somewhat indefinite in their ideas as to just what mode of motion does constitute heat. In one case they speak of radiant waves as heat, in another as local molecular vibrations as heat; in a third, of the free motions of gas particles as heat, and in a fourth, of motive influences which cease to affect the thermometer as heat, for what else is meant by absolute heat? The *authorities* certainly consider that heat continues to exist as heat in the case of increased heat capacity, when they assert that specific heat varies with variation in the temperature of substances. Thus it seems that all motive influences of which we become aware in matter, outside of gravity, electricity, magnetism, light, chemism, and mass motion, are grouped together as heat, their varying conditions being simply pointed out by qualifying adjectives. The phrase, "Latent Heat," has by no means gone out of use. Sir William Thomson, in the last edition of the Encyclopedia Britannica, considers it necessary to still retain it. In fact there are various modes of motion, some centrifugal, others centripetal in their effects, so closely related to ordinary heat, that it has proved more convenient to consider them as special heat conditions than to devise separate names for them.

Mr. Rachel is still more decisive in regard to another portion of my article. He says, "Mr. Morris's conception of the action of gravity is still more erroneous. This gentleman says, 'the earth must fall towards the body with the same energy that the body displays in falling towards the earth'! Now the two fundamental laws of gravitation, as discovered by Newton, are attraction acts in direct proportion to mass and in indirect proportion to square of distance. The statement of Mr. Morris is therefore absolutely false."

Perhaps so, yet I hardly think that Newton himself would have so absolutely denied my proposition. Let us suppose the falling body to be increased until it equals the earth in weight. What would follow then—would not gravity cause them to approach each other with equal energy? Their attractive pulls upon each other would be equal, and therefore the effects of these pulls must be equal.

If, however, the falling body be greatly decreased in weight, this may seem to some to change the elements of the problem. Yet it can readily be shown that difference in weight makes no difference whatever in the result. We must not look upon the earth as fixed and the falling body alone as movable. They are both freely floating masses, each capable of yielding to any exterior impulse. The size or weight has nothing to do with the question. If an atom and the earth be side by side, and be attracted by a distant mass with the same vigor, they must move with equal energy towards it. Yet an energy which would give the atom excessive speed would produce an inappreciable effect upon the earth.

Suppose, for the sake of illustration, that the falling body weighs one pound and the earth one million pounds. Then the falling body will attract each pound of the earth's mass with a vigor dependent on its distance, and be attracted by it with equal vigor. To reach the whole attraction of the falling body we must add together this million of separate attractions. But, in like manner, to get the whole attraction of the earth we must add together its million of separate attractions. The body exerts a separate attraction upon each pound of the

earth's mass. Each pound of the earth's mass reacts with an equal vigor of attraction upon the body. We must add all these separate attractions together to get the whole sum of attraction in either case, and these whole sums are necessarily equal. The body, therefore, attracts the earth with as much vigor as the earth attracts the body, and necessarily, therefore, they must approach each other with equal energy. Of course not with equal speed. Under the above supposition their respective weights were as a million to one, and a million pounds falling one inch would be equivalant to one pound falling a million inches. Their acceleration in speed must likewise, in both earth and body, obey the law of gravitative acceleration.

It is well, therefore, to bear strictly in mind, that in gravitation, as in every other form of force, action and reaction are always equal and opposite.*

CHARLES MORRIS.

2223 Spring Garden street, Philadelphia.

*In my previous article, above referred to, there is a typographical error, which slightly confuses the meaning. On page 167, line 49, the phrase "this force is increasing," should read, "this force is unceasing."

PRIMEVAL ROTATION AND COSMICAL RINGS.
II.

To the Editor of "SCIENCE":—

Prof. A. Winchell, recounting the history of formation of the solar system from a sphere of incandescent gas, says: "The cooling and contraction of this vapor inaugurated a rotation." [1]

Matter is governed by law, hence the ball of gas must have obeyed laws governing gases. Men have detected several laws of nature, while doubtless there are others eluding research. The globe of gas was dominated by known or unknown laws; if by unknown, no scheme of planetary evolution can be outlined; by known, hypotheses are tested by their application. There exists a doctrine, the Nebular Hypothesis, and we take it for granted that its advocates conceive the gaseous sphere to have been wrought by known laws.

But no law of nature yet discovered is able to cause a sphere of gas to rotate.

Contracting by cooling did not begin rotation, for, by dynamic laws, the mass was not hot, but cold. If hot, contracting would not cause rotary motion, but would give rise to two motions, centripetal and peripheral, both radial instead of circular. The heaviest atoms, gravitating towards the centre, would displace the lightest towards the circumference.

Repulsion did not exist; this force can only obtain in matter *not* dissociated. Repulsion causes dissociation and vanishes, gravity reasserting dominion. Hence, repulsion is more ancient than that gravity which caused the mass to develop a solar system; else the first state of matter was in dissociation.

These things are unknowable; therefore, with adherents of the hypothesis, we dismiss repulsion, leaving the mass subject to no known energy but gravity. If repulsion did act it could not cause rotation. Gravity could never cause the ball to turn; it would bring every atom to a rest. The whole mass would arrange itself in concentric strata, whose distance from the centre would depend on specific gravity. Calm would ensue unless pressure was sufficient to force atoms within range of chemism. Chemical reaction would have no power to start axial revolution. It would evolve heat, repulsion and temporary expansion, which, waning, would leave the mass smaller through combination, no sign of rotary motion having appeared. The mass extended half way to α Centauri, it being equal in mass to the sun. Helmholtz has shown that if the matter in the solar system expanded to Neptune, "it would require several cubic miles to weigh a single grain."[2] But the same matter filled a sphere whose radii were half the distance of the stars in length. Estimation of its tenuity indicates that a space as large as the moon only contained a grain. Yet it was "intensely heated."[3] It is not known how many atoms make a grain; counting them by the million, they were yards apart, in frigid voids—hot! Obeying gravity, they descended with slowest conceivable motion; at no point in their fall displaying tendency to move in arcs of circles at right angles to their radial movement, which they must do to begin rotation in the cosmic sphere. In the present state of knowledge, judging from laws at work in the Universe, it can be safely asserted that the ball had no rotary motion. Ignoring these considerations, we will assume with Winchell that it was in revolution.

"The cooling and contraction of this vapor inaugurated a rotation which was inevitably accelerated to such an extent that a peripheral ring was detached which became a planet. The same process continued and other rings were detached which became other planets in due succession. Similarly, the planetary masses detached rings which became their satellites."[4]

Conceiving the mass to have cut loose from 61 Cygni and other cosmic masses; admitting cooling, contraction and acceleration, then the sphere would be unable to cast off by any law of nature hitherto discovered, the least particle, to say nothing of a massive ring. The ball had dwindled to the orbit of Neptune, acquiring such velocity as to no longer remain intact, so it cast off equatorial matter enough to form that planet.

The rate of motion on the equator was only 3.36 miles a second; and a vacuum as made by Crookes is as a solid compared with the density of the ring; yet Neptune's mass is nearly 102 sextillions of tons[5]. The material, being exterior, was of the lowest specific gravity of any in the mass; thence its volume was enormous; so great as *not* to be peripheral. The word periphery alludes to the surface, and Winchell says the ring was peripheral. It was not,—it was formed of gas torn up from a depth of hundreds of millions of miles, in order to secure substance sufficient to form Neptune. If not,—the mass was piled above the level of the equator, an impossibility, as gravity would bring it down. As soon as force raised a line of atoms above the equatorial level, around the ball, the next line of atoms below would ascend, then the next, and so on. The poles would depress causing the mass to assume lenticular form. This would retard rotation, allowing central attraction to regain control, bringing the mass to a sphere as in the beginning. This oscillation must take place so long as the mass remained a gas. Should it become fluid, then the alternations would be between a sphere and spheroid, and the mutation would obtain until solidification sets in. No atom at any period had power to overcome gravity, the stability of the mass being assured by inhering laws. The mass of the solar system, the mass of any planet, the direction and velocity of the planet's original motion, determine what orbit it shall traverse.

The orbit of Neptune is determined; it makes regular revolutions, hence the centre of the assumed ring that formed it, when abandoned coincided with the present track of the planet's centre. Therefore the ring was not detached when the mass was lenticular, for its edge then extended far beyond where Neptune now revolves; if it had been the planet would now describe our orbit much *farther* from the sun.

The mass reached the present path of Neptune when spherical, and that world was thrown off where it now makes circuit, the mass being a sphere when it parted with its first ring.

[1] Geology of the Stars, p. 260.
[2] Youman's Correl. and Con. Forces, p. 231.
[3] Geology of the Stars, p. 279.
[4] Geology of the Stars, p. 279.
[5] Chambers' Astronomy, p. 898.

Then a segment half way to Uranus was torn away entirely around the ball, and the rupture took place along the chord of the arc. A section of this ring would be flat inside and curved outside; else a ring lifted out of the equator quite around the sphere, whose sections were circular, leaving concave walls of gas in north and south latitude. Neptune would condense somewhere on a line in the centre of gravity of the ring. In either case the orbit of the first world would be *nearer* the sun than now. It could not have been thrown off the surface, as there was not material enough; nor from the edge of the lens-shaped mass, nor from beneath the surface of the sphere, for from either place the orbit would not be where it is. It could not have been cast off at all. EDGAR L. LARKIN.

NEW WINDSOR OBS., *May 11th*, 1881.

BOOKS RECEIVED.

THE HUMAN BODY.—An Account of its Structure and Activities and the Conditions of its Healthy Working. By H. NEWELL MARTIN, D.Sc., M.A., M.B. Henry Holt and Company, 1881.

This book is the fourth of the "*American Science Series*" of manuals prepared under the direction of Messrs. Holt and company, and will be found of equal value, as a popular guide to the subject treated, to the three works which preceded it. It is a reliable work, being compiled from the best authorities, and is not intended for specialists, but for general readers and students.

In an age when the Physician is called upon to explain to his patient, the *raisen d'etre* of the treatment suggested, and even to describe the peculiar condition of the organs affected, some knowledge of Human Anatomy and Physiology appears essential to those who desire to act as consulting physician in their own cases.

To meet such a demand for a popular work on the human body, Dr. Martin has prepared the present volume, which is free from technicalities, or scientific terms requiring interpretation. The reader has the advantage of one hundred and sixty-five excellent illustrations, and as Dr. Martin's style of writing is both clear and comprehensive, the task of the reader is an easy one.

The earlier works of this series have been reviewed in "SCIENCE" and comprise the following manuals: *Astronomy*, by Professors Simon Newcomb and Edward S. Holden: *Botany*, by Professor C. E. Bessey: *Zoology*, by Professor A. S. Packard, Jr.

OSTEOLOGY OF SPEOTYTO CUNICULARIA Var. Hypogæa, and of Eremophila alpestris, by R. W. SCHUFELDT, U. S. A.—Extracted from the Bulletin of the U. S. Geological and Geographical Survey—Washington, Feb. 11th, 1881.—Four full page illustrations.

ABSTRACT OF TRANSACTIONS of the Anthropological Society of Washington, D. C., with the annual report of the President.—For the year ending Jan. 20, 1880, and for the second year ending January 18th. 1881. Prepared by J. W. POWELL.—Washington, 1881.

THE TWELFTH ANNUAL REPORT of the American Museum of Natural History—Central Park, New York City—Dated February 15th, 1881.

REPORT of the Cruise of the U. S. Revenue Steamer *Corwin* in the Arctic Ocean, by Capt. C. L. HOOPER, U. S. R. M,—November 1, 1880—Washington, 1881.

REPORT of the Director of the Detroit Observatory of the University of Michigan—October 1, 1879, to January 1, 1881, Ann Arbor, Michigan, 1881.

ABSTRACT of some Paleontological Studies of the Life History of Spirifer lævis H. by, Professor H. S. WILLIAMS of Cornell University, Ithaca, N. Y.—Reprinted for American Journal Science.

OBSERVATIONS on Jupiter by L. TROUVELOT—Presented March 9th, 1881.—Reprinted from the proceedings of the American Academy of Arts and Sciences.

PROCEEDINGS of the U. S. National Museum, 1881. Check List of Duplicates of Fishes from the Pacific coast of North America (221 Species) distributed by the Smithsonian Institution in behalf of the United States National Museum.—Prepared by DAVID S. JORDAN and PIERRE L. JOUY.—April 13, 1881.

DESCRIPTION of a new species of Squalius (Squalius aliciæ from Utah Lake, by PIERRE LOUIS JOUY.

DESCRIPTION of a new Gobioid Fish (Othonops eos) from San Diego, Cal. by ROSA SMITH.

ON a Duck new to the North American Fauna, by ROBERT RIDGWAY.

ON Amazilia yucatanensis (Cabot) and A. cerviniventris, Gould, by ROBERT RIDGWAY.

DESCRIPTIONS of new species of Fishes (Uranidea marginata, Potamocottus Bendirei) and of Myctophum crenulare, J. and G.—by TARLETON H. BEAN.

NOTES on the Fishes of the Pacific Coast of the United States by DAVID S. JORDAN and CHARLES H. GILBERT.

In this paper descriptions are given of 109 species of fishes known to occur along our Pacific Coast between the Mexican boundary and that of British Columbia, with notes on the distribution, habits, size, value, etc., of each species, in advance of the publication of a general descriptive work.

AMERICAN KINDERGARTEN MAGAZINE.—Edited by Emily M. Coe, Bible House, New York.

We have pleasure in recognizing the sterling merit of this excellent little Monthly, a leading feature of which appears to be an attempt to popularize science in a form suitable for children. The present number contains articles introducing the young readers to the best methods of classification of the Animal Kingdom. The journal is in its third volume, and is sold for one dollar a year.

NEW APPLICATION OF THE SUB-PRODUCTS OF COAL-TAR.—Mr. Sanders, of St. Petersburg, has succeeded in producing from the heavy oils of coal-tar, a new substance which, in many cases, takes the place of india-rubber with advantage. It is prepared in the following manner. A given weight of a mixture in equal parts of wood-oil and coal-tar oil, or of coal-tar and hemp-oil, is heated for several hours, at a temperature of about 318° Fahr., so as to disengage the injurious substances and increase the viscosity of the mass, until it may be drawn out in threads. A second quantity, equal to the former, of linseed-oil, preferably thickened by boiling, is now added, and also from one-twentieth to one-tenth per cent. of ozocerite with a little spermaceti. In the meanwhile, the mass is kept at a uniformly high temperature for some hours, when from one-fifth to one-half part of sulphur per cent. is added, after which the product is moulded or otherwise worked in the same manner as india-rubber. The proportions of the three oils named above may be varied so as to obtain a harder or more elastic substance, as may be required. The product is elastic and tenacious, standing the weather better than india-rubber, and is not deteriorated by great pressure or a high temperature. It is said to be specially suitable for the insulation of telegraph wires, and may be employed alone or mixed with india-rubber or similar resinous substances.

ELECTROLYSIS.—Mr. E. F. Smith finds that a black hydrated oxide, Ur_3O_4, is precipitated when a galvanic current is passed through a solution of uranium acetate, formate, or nitrate.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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In his recent address to the Royal Microscopical Society of London, the President, Dr. Lionel S. Beale, F. R. S., introduced some interesting facts relating to the present limits of microscopic vision, and indicated the advance that may be anticipated for the future in this direction.

Within five years it has been often asserted by those who make the Physics of the Microscope their special study, that the limits of microscopic vision had been almost reached by modern objectives, and that further advance was barred by insuperable difficulties. Since this time the record of progress contains numerous instances of advances made beyond these barriers which authorities considered until now insurmountable. Dr. Beale claims that "he only who is quite ignorant of the many and great improvements made in our methods of research, and in the instruments required for investigation, would think of fixing any limit to the advance of microscopical inquiry."

With improved instruments, the Microscopists have discovered improved methods of preparing objects for examination, and subtle agents united with the most delicate manipulation are now employed to develop structure, requiring the highest power of microscopic definition and amplification. We remember with Dr. Beale the time (within ten years) when in many branches of inquiry it was truly said that the optical instruments were in advance of the methods of making examinations, when our magnifying powers were higher than we could use, without losing, rather than gaining, as regards the definition of delicate structure. All this has now changed; the power of definition of objectives has been more than doubled, but the Biologist, in his investigations, anxiously demands higher powers and more perfectly corrected objectives.

Until recently the Histologist was satisfied with powers of five to six hundred diameters. Dr. Beale, in his recent address, states: "Our present limit of observation in investigations on the structure and action of the tissues of man and the higher animals, in my opinion, includes the use of magnifying powers of 2000 diameters. Objects considerably less than the hundred-thousandth of an inch can be studied with advantage, but how much less than these dimensions cannot, I think, be determined with accuracy at this time; for so much depends upon the character of the object, and a number of small points of detail as regards mode of examination.

But in other departments of Microscopical research our present means of investigation enable those familiar with the requisite methods of inquiry to demonstrate characteristics of structure far more intricate and minute than the above remark would infer. Various modifications of immersion lenses and in immersion media have greatly contributed to advance our knowledge of structure and action in the lower forms of life, and there is every reason to think that, as time goes on, methods of observation will be still improved and new methods discovered."

Another aid to perfect Microscopy is Photography, for by its use "things dimly seen by the eye may be very distinctly and correctly delineated, and with a perfection of accurate detail which a few years ago we should not have supposed possible." In this direction Dr. Beale states that "in all probability the application of photography to investigations upon minute structural details will be carried far beyond anything yet reached, although it is really wonderful how much has been achieved up to this time."

It will thus be seen that a variety of circumstances is steadily leading the way to what may be termed A NEW MICROSCOPY.

Both the Microscope and objectives, as also methods of manipulation, are being *revolutionized*, producing entirely new results. Even a new style of literature of the subject is developing. As far back as June, 1875, the editor of this journal, in a paper prepared for *Popular Science Monthly*, then foreshadowed this change. The article was headed, "*The Microscope and its Misinterpretations.*" A happy satisfaction then reigned among Microscopists, both with their instruments and their work, and the article was criticised as an assault upon the integrity of Microscopical research. It is some satisfaction to the present writer to find that those who then came forward as champions of the perfect microscopical work of that day, are now the most active leaders of the *new reform.* We refer to Mr. John Phin, the present editor of *The American Journal of Microscopy*, who can claim the honor of having established the first successful microscopical journal in the United States, and Professor J. Edwards Smith, of Cleveland, the author of

the recent book "How to see with the Microscope," a work which is a valuable addition to Microscopical Literature; both wrote articles against "*The Misinterpretation of the Microscope.*" In that article we gave very strong illustrations of the "*misrepresentations*" referred to, but the paper was written some years in advance of the present developments, which have made the case much stronger. The disputed resolution of the "Podura" scale was then quoted as an instance of an objective giving two distinct resolutions of an object, one of which was clearly an erroneous one, but who would have then anticipated that the spherules on "*Angulatum*" which we have for so many years religiously regarded as the true ultimate resolution of that diatom, would prove to be an illusion? While to make the case more complicated, Professor E. Abbe states that "while it is not my opinion that the *Angulatum* valve is composed of spherules, yet even if such should exist, they would not have a different effect."

Thus "*The Misinterpretation of the Microscope*" under certain conditions, is no myth, but an admitted fact; we welcome then the improvements which shall at least partially remedy the evil. The high angle objectives of the present, although far from perfect, give great hope for the future, and we trace in Professor Smith's work, to which reference has been made, the advent of a higher intelligence among Microscopical workers. This new spirit of progress is well described by Dr. Beale when he says, the Microscopist, like the Astronomer, is ever longing to get a little beyond the point at which he has already arrived. Each new fact gained by research seems but to indicate the existence of more and more important things beyond. Limit is reached and then surmounted, but soon a new limit seems to rise from the mists in the distance towards which the worker is impelled by new hopes and desires. It is this never-halting progress which distinguishes scientific from every other kind of inquiry, and particularly microscopical investigation, for it can never be completed. It deals with the illimitable. The boundaries of to-day are found to have vanished to-morrow, and the eyes and understanding begin to penetrate into regions which but a short time before had been considered far beyond the range of possible investigation.

CONDUCTIBILITY OF GLASS FOR THE GALVANIC CURRENT. —According to A. Sewarz, if two platinum wires are interposed in the same circuit, the one passing through the free air while the other lies between two glass plates, or is melted into a thick capillary tube, at a certain temperature of the tube the former glows brilliantly, while the second remains dark. If the glass becomes heated the former grows dark, whence the author concludes that the glass has become more conductive.

THE PRODUCTION OF SOUND BY RADIANT ENERGY.*

BY ALEXANDER GRAHAM BELL.

In a paper read before the American Association for the Advancement of Science, last August, I described certain experiments made by Mr. Sumner Tainter and myself which had resulted in the construction of a "*Photophone*," or apparatus for the production of sound by light; † and it will be my object to-day to describe the progress we have made in the investigation of photophonic phenomena since the date of this communication.

In my Boston paper the discovery was announced that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight. The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of the various substances which became sonorous in the condition of thin diaphragms, but this failure was explained upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was therefore supposed that, if we could lead to the ear air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. The first experiments made to verify this hypothesis pointed towards success. A beam of sunlight was focussed into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam, a clear, musical tone was heard, the pitch of which depended upon the frequency of the interruption of the light and loudness upon the material composing the tube.

At this stage our experiments were interrupted, as circumstances called me to Europe.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds produced by masses, but would also permit us to test the more general proposition that *sonorousness, under the influence of intermittent light, is a property common to all matter.*

The substance to be tested was to be placed in the interior of a transparent vessel, made of some material which (like glass) is transparent to light, but practically opaque to sound.

Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing tube.

Some preliminary experiments were made in Paris to test this idea, and the results were so promising that they were communicated to the French Academy on the 11th of October, 1880, in the note read for me by Mr. Antoine Breguet.‡ Shortly afterwards I wrote to Mr. Tainter, suggesting that he should carry on the investigation in America, as circumstances prevented me from doing so myself in Europe. As these experiments seem to have formed the common starting point for a series of independent researches of the most important character, carried on simultaneously, in America by Mr. Tainter,

*A Paper read before the National Academy of Arts and Sciences, April 21, 1881.

† Proceedings of American Association for the Advancement of Science, Aug. 27, 1880; see, also, American Journal of Science, vol. xx, p. 305; Journal of the American Electrical Society, vol. iii, p. 3; Journal of the Society of Telegraph Engineers and Electricians, vol. ix, p. 404; Annales de Chimie et de Physique, vol. xxi.

‡ *Comptes Rendus*, vol. xcl, p. 595.

and in Europe by M. Mercadier,† Prof. Tyndall,‡ W. E. Röntgen,§ and W. H. Preece,‖ I may be permitted to quote from my letter to Mr. Tainter the passage describing the experiments referred to:

METROPOLITAN HOTEL, RUE CAMBON, PARIS,
Nov. 2, 1880.

DEAR MR. TAINTER:

* * * I have devised a method of producing sounds by the action of an intermittent beam of light from substances that cannot be obtained in the shape of thin diaphragms or in the tubular form; indeed, the method is specially adapted to testing the generality of the phenomenon we have discovered, as it can be adapted to solids, liquids, and gases.

Place the substance to be experimented with in a glass test-tube, connect a rubber tube with the mouth of the test-tube, placing the other end of the pipe to the ear. Then focus the intermittent beam upon the substance in the tube. I have tried a large number of substances in this way with great success, although it is extremely difficult to get a glimpse of the sun here, and when it does shine the intensity of the light is not to be compared with that to be obtained in Washington. I got splendid effects from crystals of bichromate of potash, crystals of sulphate of copper, and from tobacco smoke. A whole cigar placed in the test-tube produced a very loud sound. I could not hear anything from plain water, but when the water was discolored with ink a feeble sound was heard. I would suggest that you might repeat these experiments and extend the results," &c., &c.

Upon my return to Washington in the early part of January,†† Mr. Tainter communicated to me the experiments he had made in my laboratory during my absence in Europe.

He had commenced by examining the sonorous properties of a vast number of substances enclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard rigid bodies like crystals, or diaphragms such as we had hitherto used.

In order to study the effects under better circumstances he enclosed his materials in a conical cavity in a piece of brass closed by a flat plate of glass. A brass tube leading into the cavity served for connection with the hearing-tube. When this conical cavity was stuffed with worsted or other fibrous materials the sounds produced were much louder than when a test-tube was employed. This form of receiver is shown in Fig. 1.

Mr. Tainter next collected silks and worsteds of different colors, and speedily found that the darkest shades produced the best effects. Black worsted especially gave an extremely loud sound.

As white cotton-wool had proved itself equal, if not superior, to any other white fibrous material before tried, he was anxious to obtain colored specimens for comparison. Not having any at hand, however, he tried the effect of darkening some cotton-wool with lamp-black. Such a marked re-enforcement of the sound resulted that he was induced to try lamp-black alone.

About a teaspoonful of lamp-black was placed in a test-tube and exposed to an intermittent beam of sunlight. The sound produced was much louder than any heard before.

Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lamp-black surface towards the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lamp-black surface turned from the sun the sound was much feebler.

Mr. Tainter repeated these experiments for me immediately upon my return to Washington, so that I might verify his results.

Upon smoking the interior of the conical cavity shown in Fig. 1, and then exposing it to the intermittent beam, with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube.

The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver, as illustrated in the drawing, Fig. 1.*

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor re-inforcements were constantly occurring, which became more and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The effects produced by lamp-black seemed to me to be very extraordinary, especially as I had a distinct recollection of experiments made in the Summer of 1880 with smoked diaphragms, in which no such re-enforcement was noticed.

Upon examining the records of our past photophonic experiments we found in vol. vii, p. 57, the following note:

"Experiment V.—Mica diaphragm covered with lamp-black on side exposed to light.

"Result: distinct sound about same as without lamp-black.—*A. G. B. July 18th,* 1880.

"Verified the above, but think it somewhat louder than when used without lamp-black.—*S. T., July 18th,* 1880.

Upon repeating this old experiment we arrived at the same result as that noted. Little if any augmentation of sound resulted from smoking the mica. In this experiment the effect was observed by placing the mica diaphragm against the ear and also by listening through a hearing-tube, one end of which was closed by the diaphragm. The sound was found to be more audible through the free air when the ear was placed as near to the lamp-black surface as it could be brought without shading it.

At the time of my communication to the American Association I had been unable to satisfy myself that the substances which had become sonorous under the direct influence of intermittent sunlight were capable of reproducing the sounds of articulate speech under the action of an undulatory beam from our photophonic transmitter. The difficulty in ascertaining this will be understood by considering that the sounds emitted by thin diaphragms and tubes were so feeble that it was impracticable to produce audible effects from substances in these conditions at any considerable distance away from the transmitter; but it was equally impossible to judge of the effects produced by our articulate transmitter at a short distance away because the speaker's voice was directly audible through the air. The extremely loud sounds produced from lamp-black have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed.

The drawing (Fig. 2*) illustrates the mode in which the experiment was conducted. The diaphragm of the transmitter (A) was only 5 centimetres in diameter, the diameter of the receiver (B) was also 5 centimetres, and the distance between the two was 40 metres, or 800 times the diameter of the transmitting diaphragm. We were unable to experiment at greater distances without

† "Notes on Radiophony," *Comptes Rendus*, Dec. 6 and 13, 1880; Feb. 21 and 28, 1881. See, also, *Journal de Physique*, vol. x, p. 53.

‡ "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." *Proc. Royal Society*, Jan. 13, 1881, vol. xxxi, p. 307.

§ "On the tones which arise from the intermittent illumination of a gas." See *Annalen der Phys. und Chemie, Jan.*, 1881, No. 1, p. 155.

‖ "On the Conversion of Radiant Energy into Sonorous Vibrations." *Proc. Royal Society*, March 10, 1881, vol. xxxi, p. 506.

†† On the 7th of January.

*See page 247 for illustrations.

a heliostat on account of the difficulty of keeping the light steadily directed on the receiver. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lamp-black receiver.

In Fig. 3* is shown a mode of interrupting a beam of sunlight for producing distant effects without the use of lenses. Two similarly-perforated disks are employed, one of which is set in rapid rotation while the other remains stationary. This form of interrupter is also admirably adapted for work with artificial light. The receiver illustrated in the drawing consists of a parabolic reflector, in the focus of which is placed a glass vessel (A) containing lamp-black or other sensitive substance, and connected with a hearing-tube. The beam of light is interrupted by its passage through the two slotted disks shown at B, and in operating the instrument musical signals like the dots and dashes of the Morse alphabet are produced from the sensitive receiver (A) by slight motions of the mirror (C) about its axis (D).

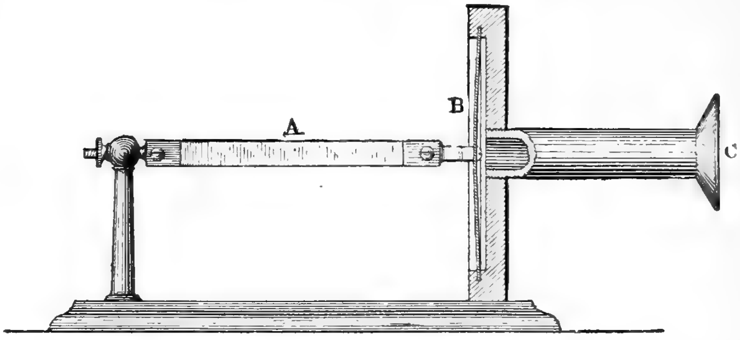


Fig. 5.

In place of the parabolic reflector shown in the figure, a conical reflector like that recommended by Prof. Sylvanus Thompson† can be used, in which case a cylindrical glass vessel would be preferable to the flask (A) shown in the figure.

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the color are two conditions that markedly influence the intensity of the sonorous effects. *The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colors.*

The materials from which the best effects have been produced are cotton-wool, worsted, fibrous materials generally, cork, sponge, platinum and other metals in a spongy condition, and lamp-black.

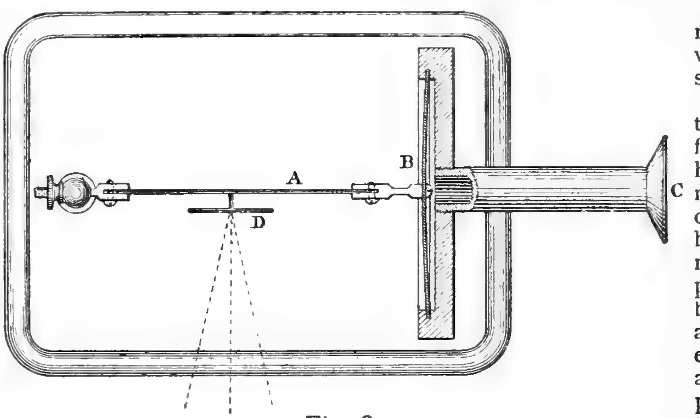


Fig. 6.

The loud sounds produced from such substances may perhaps be explained in the following manner: Let us consider, for example, the case of lamp-black—a substance which becomes heated by exposure to rays of all refrangibility. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass the particles of lamp-black are heated, and consequently expand, causing a contraction of the air-spaces or pores among them.

Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge.

The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lamp-black. When the light is cut off the converse process takes place. The lamp-black particles cool and contract, thus enlarging the air spaces among them, and the enclosed air also becomes cool. Under these circumstances a partial vacuum should be formed among the particles, and the outside air would then be absorbed, as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon lamp-black, and a wave of rarefaction is originated when the light is cut off. *We can thus understand how it is that a substance like lamp-black produces intense sonorous vibrations in the surrounding air, while at the same time it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.*

This curious fact was independently observed in England by Mr. Preece, and it led him to question whether, in our experiments with thin diaphragms, the sound heard was due to the vibration of the disk or (as Prof. Hughes had suggested) to the expansion and contraction of the air in contact with the disk confined in the cavity behind the diaphragm. In his paper read before the Royal Society on the 10th of March, Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to the vibrations of the confined air, and that the *disks do not vibrate at all.*

I shall briefly state my reasons for disagreeing with him in this conclusion:

1. When an intermittent beam of sunlight is focussed upon a sheet of hard rubber or other material, a musical tone can be heard, not only by placing the ear immediately behind the part receiving the beam, but by placing it against any portion of the sheet, even though this may be a foot or more from the place acted upon by the light.

2. When the beam is thrown upon the diaphragm of a "Blake Transmitter," a loud musical tone is produced by a telephone connected in the same galvanic circuit with the carbon button (A) Fig. 4.* Good effects are also produced when the carbon button (A) forms, with the battery (B), a portion of the primary circuit of an induction coil, the telephone (C) being placed in the secondary circuit.

In these cases the wooden box and mouth-piece of the transmitter should be removed, so that no air-cavities may be left on either side of the diaphragm.

It is evident, therefore, that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, independently of any expansion and contraction of the air confined in the cavity behind the diaphragm.

Lord Rayleigh has shown mathematically that a to-and-fro vibration, of sufficient amplitude to produce an audible

*See page 247 for illustrations.

† Phil. Mag., April, 1881, vol. xi, p. 286.

* See page 248 for illustrations.

sound, would result from a periodical communication and abstraction of heat, and he says: "We may conclude, I think, that there is, at present no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating." (Nature, xxiii, p. 274). Mr. Preece, however, seeks to prove that the sonorous effects cannot be explained upon this supposition; but his experimental proof is inadequate to support his conclusion. Mr. Preece expected that if Lord Rayleigh's explanation was correct, the expansion and contraction of a thin strip under the influence of an intermittent beam could be caused to open and close a galvanic circuit so as to produce a musical tone from a telephone in the circuit. But this was an inadequate way to test the point at issue, for Lord Rayleigh has shown (Proc. of Roy. Soc., 1877) that an audible sound can be produced by a vibration whose amplitude is *less than a ten-millionth of a centimetre*, and certainly such a vibration as that would not have sufficed to operate a "make-and-break contact" like that used by Mr. Preece. The negative results obtained by him cannot, therefore, be considered conclusive.

The following experiments (devised by Mr. Tainter) have given results decidedly more favorable to the theory of Lord Rayleigh than to that of Mr. Preece:

1. A strip (A), similar to that used in Mr. Preece's experiment was attached firmly to the centre of an iron diaphragm (B) as shown in Fig. 5, and was then pulled taut at right angles to the plane of the diaphragm. When the intermittent beam was focussed upon the strip (A), a clear musical tone could be heard by applying the ear to the hearing tube (C).

This seemed to indicate a rapid expansion and contraction of the substance under trial.

But a vibration of the diaphragm (B) would also have resulted if the thin strip (A) had acquired a to-and-fro motion, due either to the direct impact of the beam or to the expansion of the air in contact with the strip.

2. To test whether this had been the case an additional strip (D) was attached by its central point only to the strip under trial, and was then submitted to the action of the beam, as shown in Fig. 6.

It was presumed that if the vibration of the diaphragm (B) had been due to a *pushing force* acting on the strip (A), that the addition of the strip (D) would not interfere with the effect. But if, on the other hand, it had been due to the longitudinal expansion and contraction of the strip (A), the sound would cease, or at least be reduced. The beam of light falling upon the strip (D) was now interrupted as before by the rapid rotation of a perforated disk, which was allowed to come gradually to rest.

No sound was heard excepting at a certain speed of rotation, when a feeble musical tone became audible.

This result is confirmatory of the first.

The audibility of the effect at a particular rate of interruption suggests the explanation that the strip (D) had a normal rate of vibration of its own.

When the frequency of the interruption of the light corresponded to this, the strip was probably thrown into vibration after the manner of a tuning fork, in which case a to-and-fro vibration would be propagated down its stem or central support to the strip (A).

This indirectly proves the value of the experiment.

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here, and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment.*

* Carbon and thin microscope glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy, (Comptes Rendus, vol. xlc, p. 595.) All these substances have since yielded sounds under more careful conditions of experiment.

EXPERIMENTS WITH LIQUIDS.

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absorptive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light, but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment. In the experiments made in my laboratory a very long test-tube was filled with the liquid under examination, and a flexible rubber-tube was slipped over the mouth far enough down to prevent the possibility of any light reaching the vapor above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focussed upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

RESULTS.

Clear water	No sound audible.
Water discolored by ink	Feeble sound.
Mercury	No sound heard.
Sulphuric ether*	Feeble, but distinct sound.
Ammonia	" " " "
Ammonio-sulphate of copper.	" " " "
Writing ink	" " " "
Indigo in sulphuric acid	" " " "
Chloride of copper*	" " " "

The liquids distinguished by an asterisk gave the best sounds.

Acoustic vibrations are always much enfeebled in passing from liquids to gases, and it is probable that a form of experiment may be devised which will yield better results by communicating the vibrations of the liquid to the ear through the medium of a solid rod.

EXPERIMENTS WITH GASEOUS MATTER.

On the 29th of November, 1880, I had the pleasure of showing to Prof. Tyndall in the laboratory of the Royal Institution the experiments described in the letter to Mr. Tainter from which I have quoted above, and Prof. Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapor of sulphuric ether, (a good absorbent of heat,) and another with the vapor of bisulphide of carbon, (a poor absorbent,)and he predicted that if any sound was heard it would be louder in the former case than in the latter.

The experiment was immediately made, and the result verified the prediction.

Since the publication of the memoirs of Röntgen* and Tyndall† we have repeated these experiments, and have extended the inquiry to a number of other gaseous bodies, obtaining in every case similar results to those noted in the memoirs referred to.

The vapors of the following substances were found to be highly sonorous in the intermittent beam: Water vapor, coal gas, sulphuric ether, alcohol, ammonia, amylene, ethyl bromide, diethylamene, mercury, iodine, and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen.

I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition (solid, liquid, and gaseous), and the probability is, therefore, very greatly increased that sonorousness, under such circumstances, will be found to be a universal property of matter.

*Ann. der Phys. und Chem., 1881, No. 1, p. 155.

†Proc. Roy. Soc., vol. xxxi, p. 307.

UPON SUBSTITUTES FOR SELENIUM IN ELECTRICAL RECEIVERS.

At the time of my communication to the American Association the loudest effects obtained were produced by the use of selenium, arranged in a cell of suitable construction, and placed in a galvanic circuit with a telephone. Upon allowing an intermittent beam of sunlight to fall upon the selenium a musical tone of great intensity was produced from the telephone connected with it.

But the selenium was very inconstant in its action. It was rarely, if ever, found to be the case, that two pieces of selenium (even of the same stick) yielded the same results under identical circumstances of annealing, etc. While in Europe last autumn, Dr. Chichester Bell, of University College, London, suggested to me that this inconstancy of result might be due to chemical impurities in the selenium used. Dr. Bell has since visited my laboratory in Washington, and has made a chemical examination of the various samples of selenium I had collected from different parts of the world. As I understand it to be his intention to publish the results of this analysis very soon, I shall make no further mention of his investigation than to state that he has found sulphur, iron, lead, and arsenic in the so-called "selenium," with traces of organic matter; that a quantitative examination has revealed the fact that sulphur constitutes nearly one per cent. of the whole mass; and that when these impurities are eliminated the selenium appears to be more constant in its action and more sensitive to light.

Prof. W. G. Adams* has shown that tellurium, like selenium, has its electrical resistance affected by light, and we have attempted to utilize this substance in place of

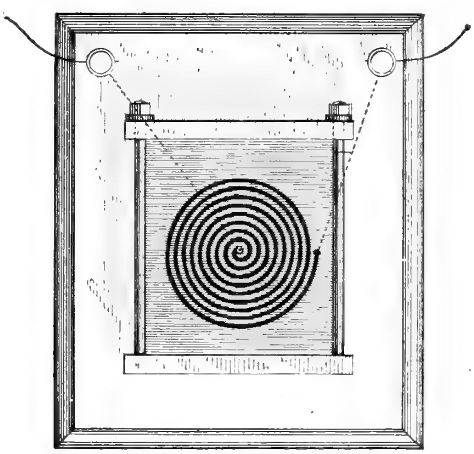


Fig. 7.

selenium. The arrangement of cell (shown in Fig. 7) was constructed for this purpose in the early part of 1880; but we failed at that time to obtain any indications of sensitiveness with a reflecting galvanometer. We have since found, however, that when this tellurium spiral is connected in circuit with a galvanic battery and telephone, and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone. The audible effectis much increased by placing the tellurium cell with the battery in the primary circuit of an induction coil, and placing the telephone in the secondary circuit.

The enormously high resistance of selenium and the extremely low resistance of tellurium suggested the thought that an alloy of these two substances might possess intermediate electrical properties. We have accordingly mixed together selenium and tellurium in different proportions, and while we do not feel warranted at the present time in making definite statements concerning the results, I may say that such alloys have proved to be sensitive to the action of light.

*Proc. Roy. Soc., vol. xxiv, p. 163.

It occurred to Mr. Tainter before my return to Washington last January that the very great molecular disturbance produced in lamp-black by the action of intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lamp-black could be employed in place of selenium in an electrical receiver. This has turned out to be the case, and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

The form of lamp-black cell we have found most effec-

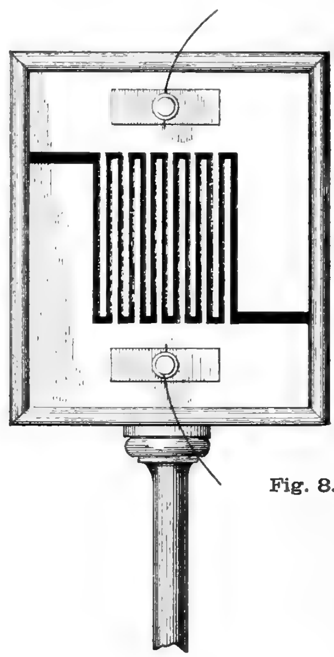


Fig. 8.

tive is shown in Fig. 8. Silver is deposited upon a plate of glass, and a zigzag line is then scratched through the film, as shown, dividing the silver surface into two portions insulated from one another, having the form of two combs with interlocking teeth.

Each comb is attached to a screw-cup, so that the cell can be placed in an electrical circuit when required. The surface is then smoked until a good film of lamp-black is obtained, filling the interstices between the teeth of the silver combs. When the lamp-black cell is connected with a telephone and galvanic battery, and exposed to the influence of an intermittent beam of sunlight, a loud musical tone is produced by the telephone. This result seems to be due rather to the physical condition than to the nature of the conducting material employed, as metals in a spongy condition produce similar effects. For instance, when an electrical current is passed through spongy platinum while it is exposed to intermittent sunlight, a distinct musical tone is produced by a telephone in the same circuit. In all such cases the effect is increased by the use of an induction coil; and the sensitive cells can be employed for the reproduction of articulate speech as well as for the production of musical sounds.

We have also found that loud sounds are produced from lamp-black by passing through it an intermittent electrical current; and that it can be used as a telephonic receiver for the reproduction of articulate speech by electrical means.

A convenient mode of arranging a lamp-black cell for

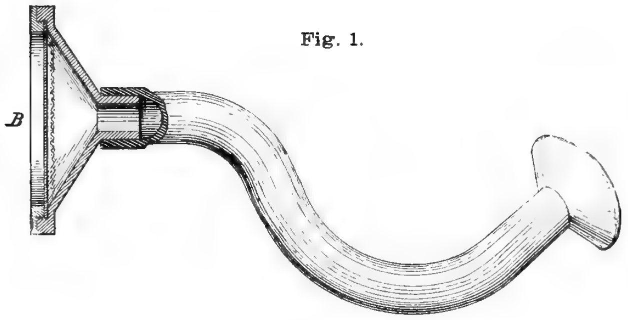


Fig. 1.

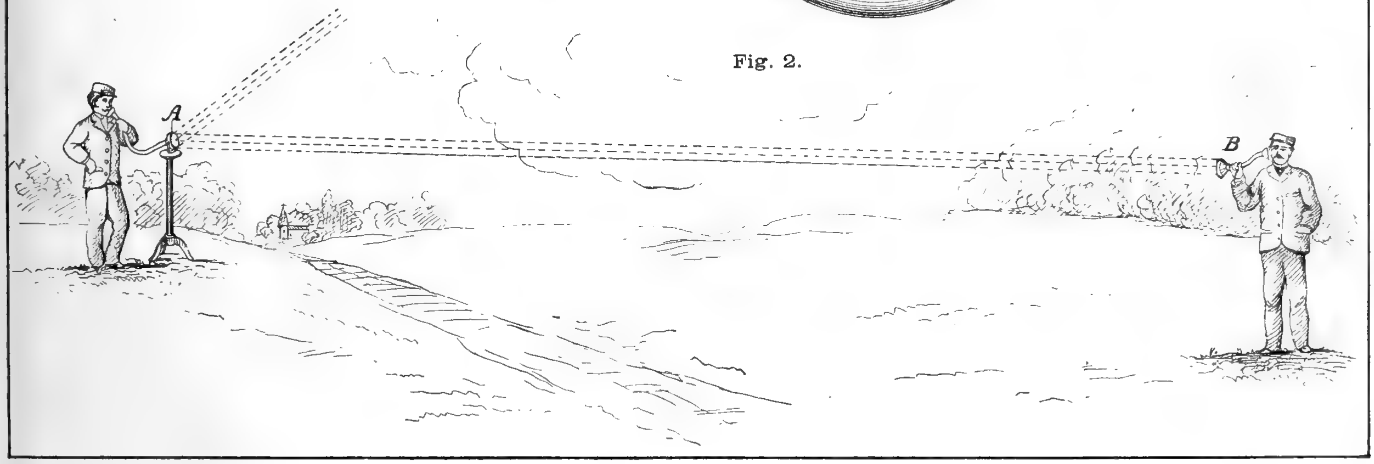


Fig. 2.

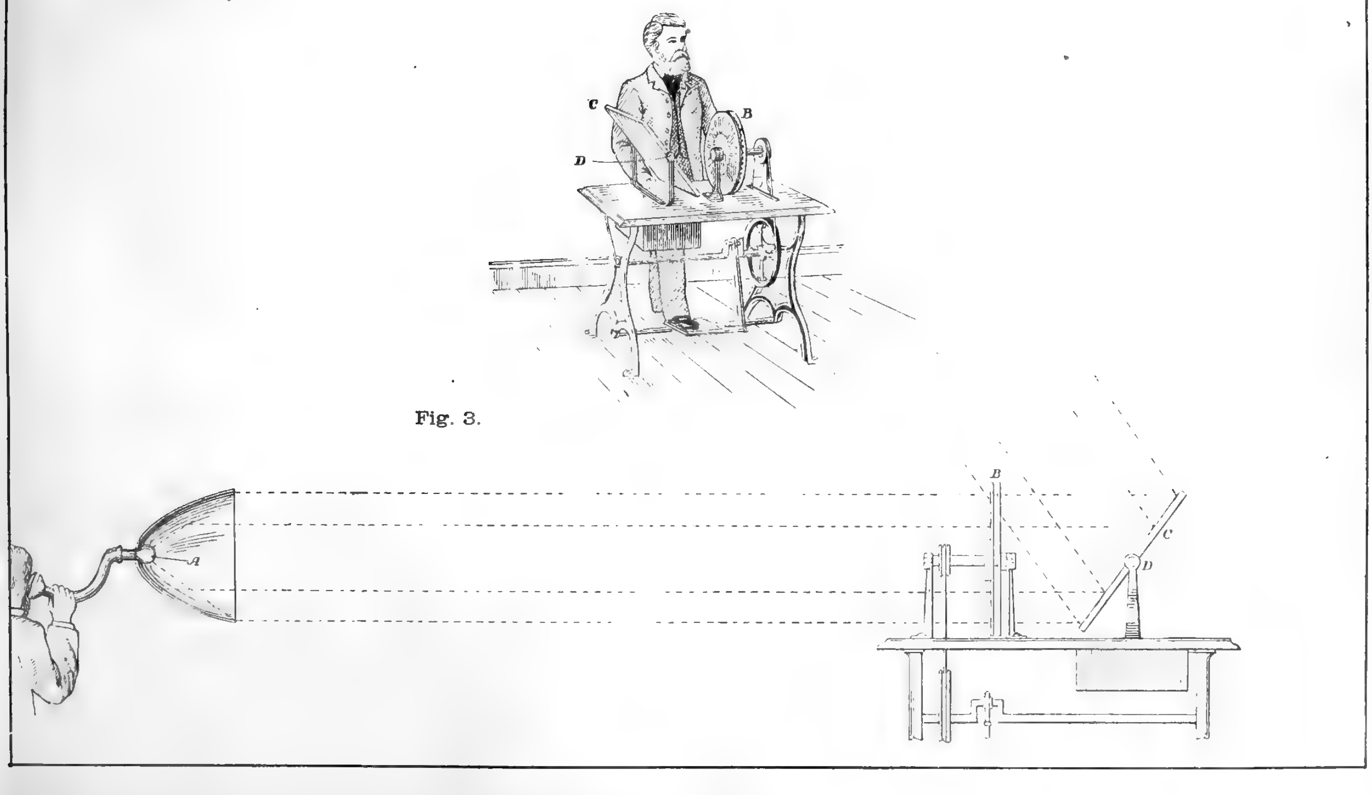


Fig. 3.

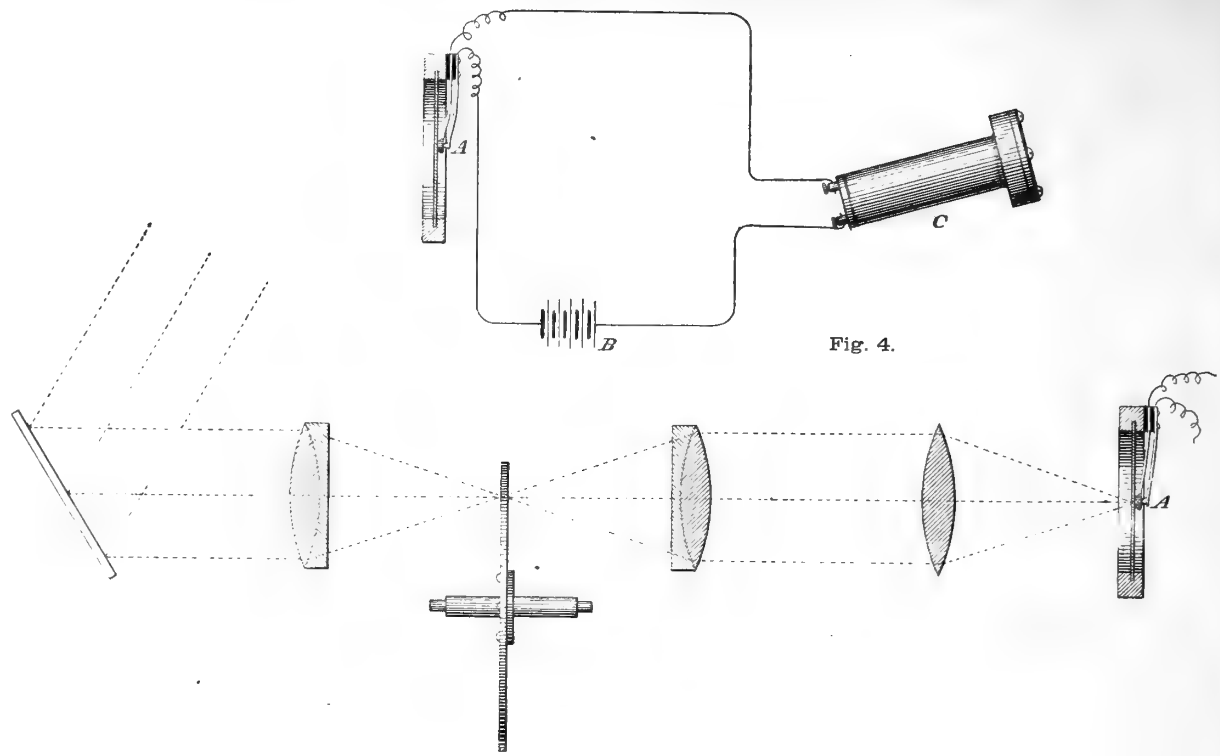


Fig. 4.

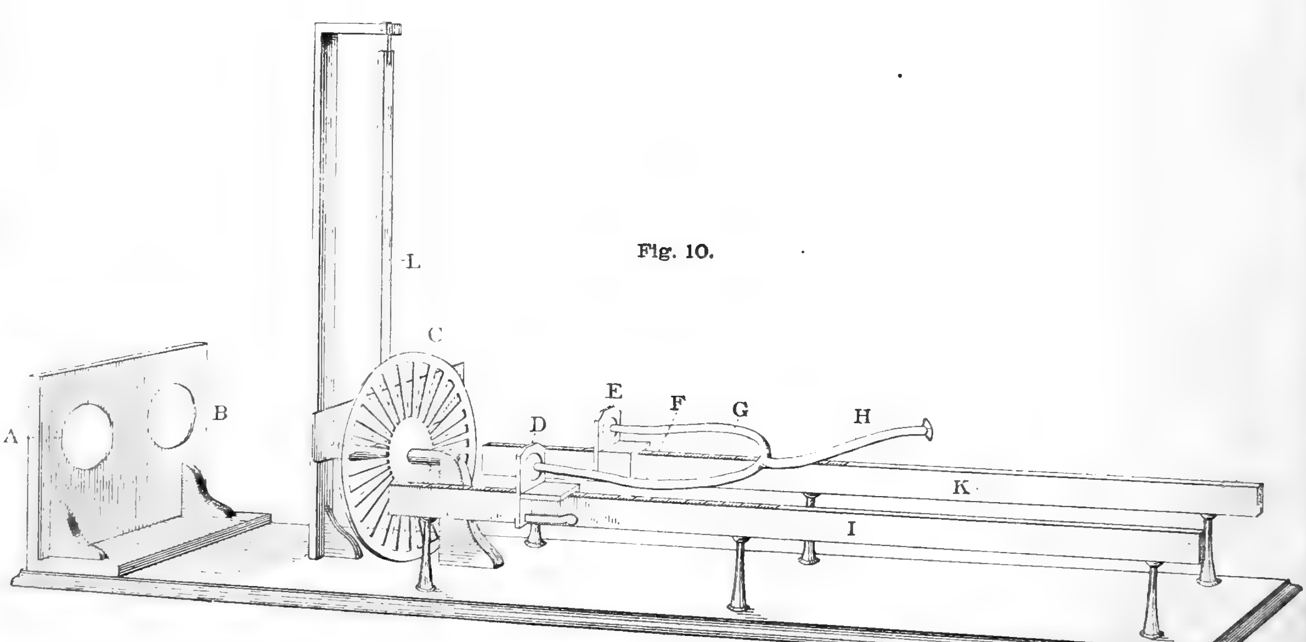


Fig. 10.

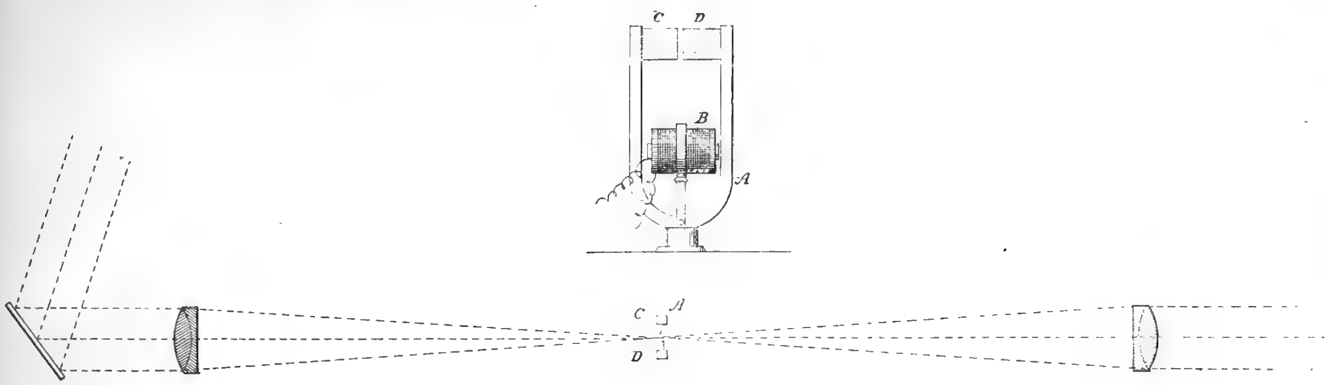


Fig. 11.

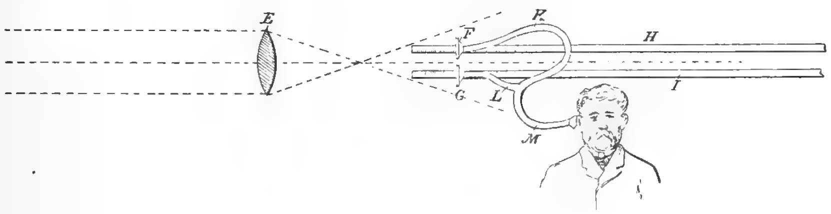
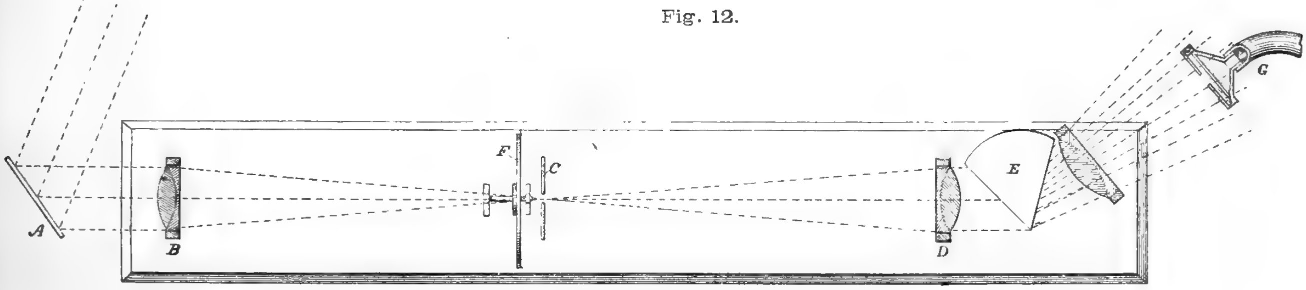


Fig. 12.



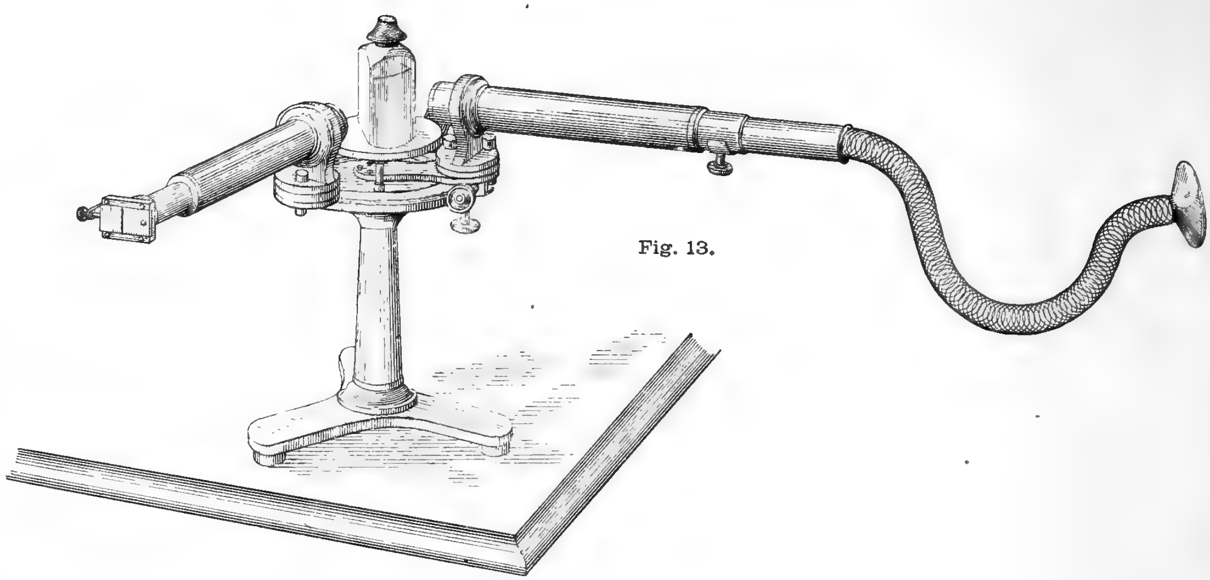


Fig. 13.

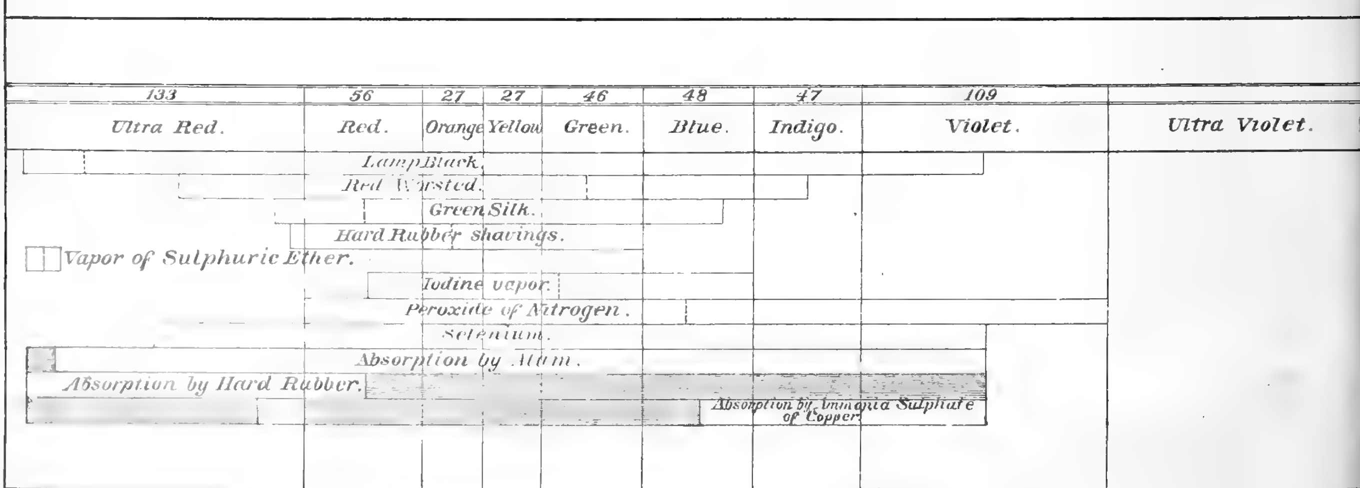

133
56
27
27
46
48
47
109
Ultra Red.
Red.
Orange
Yellow
Green.
Blue.
Indigo.
Violet.
Ultra Violet.
Lamp Black.
Red Worsted.
Green Silk.
Hard Rubber shavings.
Vapor of Sulphuric Ether.
Iodine vapor.
Peroxide of Nitrogen.
Selenium.
Absorption by Alum.
Absorption by Hard Rubber.
Absorption by Ammonia Sulphate of Copper.

Fig. 14.

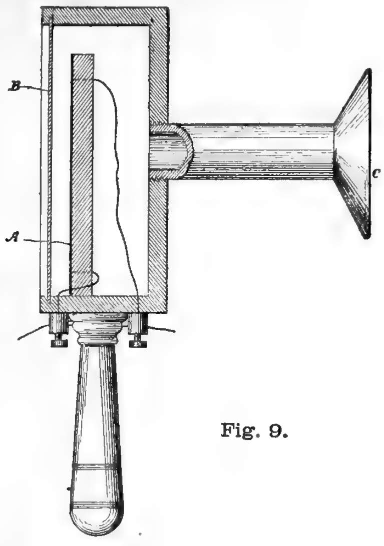


Fig. 9.

experimental purposes is shown in Fig. 9. When an intermittent current is passed through the lamp-black, (A) or when an intermittent beam of sunlight falls upon it through the glass plate (B) a loud musical tone can be heard by applying the ear to the hearing-tube (C). When the light and the electrical current act simultaneously, two musical tones are perceived, which produce beats when nearly of the same pitch. By proper arrangements a complete interference of sound can undoubtedly be produced.

UPON THE MEASUREMENT OF THE SONOROUS EFFECTS PRODUCED BY DIFFERENT SUBSTANCES.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment, and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects, but as our researches are not yet complete, I shall confine myself to a simple description of some of the forms of apparatus we have devised.

When a beam of light is brought to a focus by means of a lens, the beam diverging from the focal point becomes weaker as the distance increases in a calculable degree. Hence, if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity, we can calculate their relative sonorous powers.

Preliminary experiments were made by Mr. Tainter during my absence in Europe to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between different substances in this respect.

DISTANCE FROM FOCAL POINT OF LENS AT WHICH SOUNDS BECOME INAUDIBLE WITH DIFFERENT SUBSTANCES.

Zinc diaphragm (polished)		1.51 m
Hard rubber diaphragm		1.90 "
Tin-foil	"	2.00 "
Telephone	" (Japanned iron)	2.15 "
Zinc	" (unpolished)	2.15 "
White silk,	(In receiver shown in Fig. 1.)	3.10 "
White worsted,	" " " "	4.01 "
Yellow worsted,	" " " "	4.06 "
Yellow silk,	" " " "	4.13 "
White cotton wool,	" " " "	4.38 "
Green silk,	" " " "	4.52 "
Blue worsted,	" " " "	4.69 "
Purple silk,	" " " "	4.82 "
Brown silk,	" " " "	5.02 "
Black silk,	" " " "	5.21 "
Red Silk,	" " " "	5.24 "
Black worsted,	" " " "	6.50 "
Lamp-black. In receiver the limit of audibility could not be determined on account of want of space. Sound perfectly audible at a distance of		10.00 "

Mr. Tainter was convinced from these experiments that this field of research promised valuable results, and he at once devised an apparatus for studying the effects, which he described to me upon my return from Europe. The apparatus has since been constructed and I take great pleasure in showing it to you to-day.

(1.) A beam of light is received by two similar lenses (A B, Fig. 10*), which bring the light to a focus on either side of the interrupting disk (C). The two substances, whose sonorous powers are to be compared, are placed in the receiving vessels (D E)—so arranged as to expose equal surfaces to the action of the beam—which communicate by flexible tubes (F G) of equal length, with the common hearing-tube (H). The receivers (D E) are placed upon slides, which can be moved along the graduated supports (I K). The beams of light passing through the interrupting disk (C), are alternately cut off by the swinging of a pendulum (L). Thus a musical tone is produced alternately from the substance in D and from that in E. One of the receivers is kept at a constant point upon its scale, and the other receiver is moved towards or from the focus of its beam until the ear decides that the sounds produced from D and E are of equal intensity. The relative positions of the receivers are then noted.

(2.) Another method of investigation is based upon the production of an interference of sound, and the apparatus employed is shown in Fig. 11.* The interrupter consists of a tuning-fork (A), which is kept in continuous vibration by means of an electro-magnet (B).

A powerful beam of light is brought to a focus between the prongs of the tuning-fork (A), and the passage of the beam is more or less obstructed by the vibration the opaque screens (C D) carried by the prongs of the fork.

As the tuning-fork (A) produces a sound by its own vibration, it is placed at a sufficient distance away to be inaudible through the air, and a system of lenses is employed for the purpose of bringing the undulating beam of light to the receiving lens (E) with as little loss as possible. The two receivers (F G) are attached to slides (H I) which move upon opposite sides of the axis of the beam, and the receivers are connected by flexible tubes of unequal length (K L) communicating with the common hearing-tube (M).

The length of the tube (K) is such that the sonorous vibrations from the receivers (F G) reach the common hearing-tube (M) in opposite phases. Under these circumstances silence is produced when the vibrations in the receivers (F G) are of equal intensity. When the intensities are unequal, a residual effect is perceived. In operating the instrument the position of the receiver (G) remains constant, and the receiver (F) is moved to or from the focus of the beam until complete silence is produced. The relative positions of the two receivers are then noted.

(3). Another mode is as follows: The loudness of a musical tone produced by the action of light is compared with the loudness of a tone of similar pitch produced by electrical means. A rheostat introduced into the circuit enables us to measure the amount of resistance required to render the electrical sound equal in intensity to the other.

* See pages 248 and 249 for illustrations.

(4.) If the tuning-fork (A) in Fig. 11 is thrown into vibration by an undulatory instead of an intermittent current passed through the electro-magnet (B), it is probable that a musical tone, electrically produced in the receiver (F) by the action of the same current, would be found capable of extinguishing the effect produced in the receiver (G) by the action of the undulatory beam of light, in which case it should be possible to establish an acoustic balance between the effects produced by light and electricity by introducing sufficient resistance into the electric circuit.

UPON THE NATURE OF THE RAYS THAT PRODUCE SONOROUS EFFECTS IN DIFFERENT SUBSTANCES.

In my paper read before the American Association last August and in the present paper I have used the word "light" in its usual rather than its scientific sense, and I have not hitherto attempted to discriminate the effects produced by the different constituents of ordinary light, the thermal, luminous, and actinic rays. I find, however, that the adoption of the word "photophone" by Mr. Tainter and myself has led to the assumption that we believed the audible effects discovered by us to be due entirely to the action of luminous rays. The meaning we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage, quoted by my Boston paper:

"Although effects are produced as above shown by forms of radiant energy, which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone' *because an ordinary beam of light contains the rays which are operative.*"

To avoid in future any misunderstandings upon this point we have decided to adopt the term "*radiophone*," proposed by M. Mercadier, as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone, photophone*, and *actinophone*, to apparatus for the production of sound by thermal, luminous, or actinic rays respectively.

M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum. (*Comptes Rendus*, Dec. 6th, 1880.)

We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier.

A beam of sunlight was reflected from a heliostat (A, Fig. 12*) through an achromatic lens, (B) so as to form an image of the sun upon the slit (C).

The beam then passed through another achromatic lens (D) and through a bisulphide of carbon prism (E), forming a spectrum of great intensity, which, when focussed upon a screen, was found to be sufficiently pure to show the principal absorption lines of the solar spectrum.

The disk-interrupter (F) was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the receiver (G), which was so arranged that the lamp-black surface exposed was limited by a slit, as shown.

Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver (G) gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to decrease, and then stopped so suddenly that a very slight motion of the receiver (G) made all the difference between almost maximum sound and complete silence.

*See page 249 for illustrations.

(2.) The lamp-blacked wire gauze was then removed and the interior of the receiver (G) was filled with red-worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

(3.) Upon substituting green silk for red worsted the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

(4.) Some hard-rubber shavings were now placed in the receiver (G). The limits of audibility appeared to be on the one hand the junction of the green and blue, and on the other the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red, and to his ear the maximum was about the junction of the red and orange.

(5.) A test-tube containing the vapor of sulphuric ether was then substituted for the receiver (G). Commencing at the violet end, the test-tube was gradually moved down the spectrum and out into the ultra-red without audible effect, but when a certain point far out in the ultra-red was reached a distinct musical tone suddenly made its appearance, which disappeared as suddenly on moving the test-tube a very little further on.

(6.) Upon exploring the spectrum with a test-tube containing the vapor of iodine the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

(7.) A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were obtained in all parts of the visible spectrum, but no sounds were observed in the ultra-red.

The maximum effect seemed to me to be in the blue. The sounds were well marked in all parts of the violet, and I even fancied that the audible effect extended a little way into the ultra-violet, but of this I cannot be certain. Upon examining the absorption spectrum of peroxide of nitrogen it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption lines made their appearance.

(8.) The spectrum was now explored by a selenium cell, and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red. The audible effect extended a little way into the ultra-red on the one hand and up as high as the middle of the violet on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that *the nature of the rays that produce sonorous effects in different substances depends upon the nature of the substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.*

THE SPECTROPHONE.

Our experiments upon the range of audibility of different substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis, which was described and exhibited to the Philosophical Society of Washington last Saturday.* The eye-piece of a spectroscope is removed, and sensitive substances are placed in the focal point of the instrument behind an opaque diaphragm containing a slit. These substances are put in communication with the ear by means of a hearing tube, and thus the instrument is converted into a veritable "spectrophone" like that shown in Fig. 13.†

* Proc. of Phil. Soc. of Washington, April 16, 1881.

†See page 250 for illustrations.

Suppose we smoke the interior of our spectrophonic receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum (visible and invisible), except the ultra-violet. Now, pass a rapidly-interrupted beam of light through some substance whose absorption spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption bands. Of course, the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum; but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum, lamp-black alone may be used in the spectrophonic receiver. Indeed, the sounds produced by this substance in the ultra-red are so well marked as to constitute our instrument a most reliable and convenient substitute for the thermo-pile. A few experiments that have been made may be interesting.

(1.) The interrupted beam was filtered through a saturated solution of alum.

Result: The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

(2.) A thin sheet of hard rubber was interposed in the path of the beam.

Result: Well-marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time. (See table of results in Fig. 14.*)

(3.) A solution of ammonio-sulphate of copper was tried.

Result: When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear, however, the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination, but I do not wish you to understand that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man, and I feel that *our* first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognize the fact that the spectrophone must ever remain a mere adjunct to the spectroscope, but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption spectra in the ultra-red.

CONTRIBUTIONS TO COMPARATIVE PSYCHOLOGY.

By S. V. Clevenger, M. D.

I. INSTINCT AND REASON.

In St. George Mivart's recent work, "The Cat," Chap. XI treats of the Psychology of that animal. Amidst the usual ambiguity to be found wherever such subjects are treated, Mivart occasionally formulates his views. On page 369 his words admit of no other interpretation than an acknowledgement that instinct is nearly, though not quite pure automatism. The possession of reason by the cat is at first evasively dealt with, and finally on page 373, flatly denied. Mivart finds fault with Herbert Spencer's views as to instinct: "According to Mr. Spencer it is a higher development of reason which it has replaced, owing to the establishment of a more perfect adjustment of inner relations to outer relations than exists where mere reason is concerned." That opinion of Spencer's is one of the many which deserves to be rescued from the oblivion his involved style threatens to inflict upon the mass of his writings. From pure morphological and histological observations I have been led to the conclusions at which Spencer arrives by a wholly different route. The nervous system is a net-work of conducting substance interrelating the units of the animal body.

In an article by Spitzka ("Insane Delusions" page 34, *Journal Nervous and Mental Disease*, January, 1881), occur the following words: "In fact I should, if asked to point to the chief factor on which the higher powers of the human brain depend, lay less stress on the cortical development as such, than in the immense preponderance of the white substance due to the massive associating tracts."

Automaticity created by unvarying persistence of impressions resulting in certain definite movements, whether occurring through heredity, or during the lifetime of the individual (as proficiency in piano playing, etc.), has, for its material substratum, absolute definiteness of association of those parts which the nervous system connects; thus, regarded as a colony, the component individuals of the organism are brought into thorough automatic relationship with one another, and to that part of the environment to which the organism responds instinctively.

On the other hand reason is represented by the disconnected, shifting, short and long nerve fibres, as the arcuate of the cerebrum, not as yet assigned to any definite location. Reason thus is the struggle toward automatism. Instinct is the outcome of the struggle. Broadly viewing the higher nervous organization of animals there is a perpetual tendency to the establishment of nerve routes which would eventuate in handing over perfect control of every function to the highest nerve system. Spitzka expresses this (Architecture of the Brain, page 649 J. N. & M. D. Oct. 1879): "With the development of these highest projection fibres, the cerebral hemispheres gradually encroached on the independency of the lower ganglia, until in its maximal development as found in man, it resembles a great empire which holds a number of tributary states in sway under a common powerful rule. The automatical unity now attained, finds its parallel psychical culmination in that more perfect consciousness of the *ego*, which is peculiar to man." There is nothing debatable about this tendency on the part of the nervous system; the greater relative masses of the longitudinal and transverse associating tracts in the spinal cords, spinal and cerebro-spinal nerves and brains of all animals, in proportion to their reasoning and instinctive abilities, point to a prevailing law which seeks the reduction of all animal movements to the simplest mechanical methods. A corollary from instinct being perfected reason, would be that the salvation of reason to the race depended upon the vicissitudes and shifting circumstances with which we are surrounded, amounting to rescue from the fate mentioned by Wallace in degradation through parasitism. DeQuincey calls the human brain a palimpsest. In old age new tissues of any kind are formed with difficulty, new routes in the brain strive in vain for establishment; in senility the nervous tracts established in youth, and upon which all subsequent associations are founded, are the last to suffer disintegration, hence youthful recollections become at this time more vivid.

*See page 250 for illustrations.

FURTHER NOTES ON THE BRAIN OF THE SAUROPSIDA.

By E. C. Spitzka, M. D.

1. A most notable feature of the cerebral hemispheres of such reptiles as the Alligator, Iguana and sea-turtle is the absence of a proper choroid plexus in the lateral ventricle. This is the more remarkable as in the amphibia, the choriod plexus is very well developed. The sea-turtle has a few vascular coils protruding into the lateral ventricle at its posterior portion; nothing of the kind can be identified in the Iguana or in birds.

2. On removing the inner cerebral wall of an Alligator's hemisphere it can be seen that the Corpus Striatum is continued into the pedicle of the olfactory bulb, as a distinct prominence. In fact the substance of the pedicle is in the main a continuation of the Corpus Striatum and of the basilar part of the hemisphere, the dorso-lateral cortex becoming attenuated to a mere film on entering that structure. The lumen of the pedicle is a continuation of that recess of the lateral ventricle which undermines the mesal side of the root of the Corpus Striatum.

3. The Corpus Striatum is relatively more massive in the Sauropsida, than in any other animal group. It reaches its maximum in birds, where also the lateral ventricle is most reduced. It seems as if a secondary fusion must occur, as explaining the apparent obliteration noted in the latter group.

4. A careful study of the structure designated as the anterior commissure of the reptile's brain has failed to convince me that this structure is to be considered as the homologue of the same commissure in the mammalian brain. So far I am inclined to consider it as representing the Corpus Callosum, at least in part. Its fibres are medullated.

5. The inner face of the hemispheric wall is finely striated; this is due to the fasciculation of the nerve fibres lying subjacent to the ventricle; they correspond to the Corona radiata.

6. It is not difficult to see that the greater part of the cerebral surface, that is, the entire basilar and more than half of its lateral aspect is the representative of what in the mammalia is the least voluminous and functionally the least important portion, namely of the Island of Reil and the præperforate region. In some reptiles (Chelydra, Boa) these two districts or their homologues are demarcated from each other by a shallow sulcus. The area homologous with the Island of Reil, corresponds pretty accurately to the base of the Corpus Striatum; the other, represented in mammals by the *Substantia perforata anterior* is a bodily continuation of the thalamic halves, a marked constriction separates them from the thalami proper, on the dorsal surface. Perhaps they constitute a species of prothalamus.

There remains then as the representative of the convoluted portion of the cerebral hemispheres of the placental mammalia, merely the delicate thin walled portion of the reptilian cerebrum. It is here where the pyramidal nerve cells are found in the best development. In the tenuity of the subjacent nerve layer, it closely resembles the hemispheric wall of the mammalian embryo.

7. There are two varieties of cerebelli found in the Sauropsida; to these might be added a third or fundamental type from which the other or divergent types may be derived.

The fundamental type is found in serpents and apodal lacertians, as well as in Chelonia of a low type (Boa, Bascanion, Pseudopus, Chelydra). Here the cerebellum is a mere lip covering the entrance to the mesencephalic ventricle, as in the Amphibia, and in embryos.

The second type is found in the higher Chelonia (Cistudo, Naunemys, Calemys, Thalassochelys) and the Crocodilia (Alligator). Here the lip has become inflated, and extends like a hollow hood directly backwards over the fourth ventricle. It corresponds in its best development to nothing so much as to a baseball cap. This resemblance is heightened by the presence in the Alligator and Thallassochelys of a distant rim. I have found, in an individual of Cistudo, the Cerebellar cap dented from above, and turned inside out, as it were; the individual had suffered prolonged starvation.

The third variety is found in lacertians (Iguana) and birds (Struthio, Aro, Trichoglossus, Gallus, Columba, Phœnicopterus, etc). Here the cerebellar lip creeps up, as it were, on the posterior declivity of the optic (and post optic) lobes, firmly tied down to these by the arachnoid. In birds the lip becomes reflected from the highest point, and descends backwards.

The highest form of the second variety is found in the Alligator, where in the adult and in larger specimens, though not in the one or two-year-olds, there are distinct transverse sulci. In the sea-turtle an indication of transverse sulci is observed in hardened specimens; they may be artifacts, however.

8. An important feature of the reptilian brain are the lateral eminences of the Oblongata, which, from their connection with the eighth pair of cranial nerves, merit the designation of *eminentiæ acusticæ*. A reliquary fragment in the mammalia constitutes the Fasciola cinerea. But the greater portion of this, in reptiles (Alligator, Iguana) exceedingly complicated body seems to be a sort of herald of a higher cerebellar development, and the very similar lateral bodies of the human embryonic Oblongata appear to be swallowed up in the cerebellar mass. Future research must determine whether the *nuclei dentati* are derivable from these masses or whether some of the lesser cerebellar lobules monopolize them. In the Alligator they closely simulate cerebellar *folia*, and consist of gray and white substance. It is from them that arises the *eminentia transversa ventriculi quarti* so well developed in the Iguana and Alligator. In the latter the acoustic convolvuli are in morphological connection with the lateral kink of the cerebellum.

9. On comparing a series of animals beginning with the Amphibia, passing thence to the Sauropsida and ending with the mammalia, we find that there is this close correspondence to a series of mammalian embryonic and fœtal brains, that while in the lowest types the nerve fibres of the spinal cord are well provided with *myelin*, and the Oblongata presents the same maturity of structure, that it is only in higher types that the Cerebellum and Mesencephalon show the same or an approximate histological advance, which involves the Thalamus and Cerebrum in their entity only in the very highest types. This is an important confirmation of the laws laid down by Flechsig and Meynert.

ASTRONOMY.

The Morrison Observatory.

The Morrison Observatory—the gift of Miss Morrison, a former resident of Glasgow—was built at Glasgow, Missouri, in 1875. The building is well adapted to the purpose it is intended to serve, and was constructed under the supervision of Prof. C. W. Pritchett, who consulted several of the leading astronomers of the country in preparing his plans.

The position of the observatory is, latitude, 39° 16′ 16.8″ north. Longitude $1^h\ 3^m\ 5.93^s$ west of Washington. The latitude was obtained from observations recently made with the Transit Circle, and discussed by Prof. H.S. Pritchett; the longitude from an exchange of signals made with the United States Naval Observatory in 1880.

For instrumental equipment, the Morrison Observatory possesses one of Clark's finest 12¼ equatorials. It is of 17 feet focal length, and has already been the means of discovering a number of faint double stars. In 1877 and again in 1879, a large number of observations of the satellites of Mars were obtained. *Mimas* has been ob-

served on at least three occasions, and has been suspected, without being positively identified, a much larger number of times.

The Transit Circle was made by Troughton and Simms, London, in 1876 and was mounted in 1877. The construction of the instrument and the method of mounting are quite similar to the instruments in use at Greenwich, and Harvard College Observaory.

The telescope has a clear aperture of 6 inches, and a focal length of 6 feet 4 inches. The axis is cast in a single piece, into which fit the steel pivots, 3.50 inches in diameter. The Y's are of gun metal, and their bearing surfaces 2.50 inches long, 0.74 inches wide. The piers are of iron, and are firmly bolted to heavy stone caps which rest upon brick foundations. The circles are 24 inches in diameter, divided to 5′, and read by four microscopes each.

The reticule in the focus of the telescope carries 15 vertical and 5 horizontal threads—the vertical threads being all carried by the Right Ascension micrometer screw, and the horizontal threads by the declination screw. There are no fixed threads in the field.

The Transit Circle is furnished with two collimators having object glasses of 4 ft. 3 in. focal length and 4.33 in. aperture. The distance between the bearing points of the collimator Y's is 3 ft. 10 in. In the focus of each collimator are fixed two close vertical threads (about 5.3″ apart) and one horizontal thread. In the ordinary time observations it is customary to observe for collimation immediately before the observations of star transits, and then set the micrometer so as to destroy the error in collimation.

The Standard Sidereal Clock of the Observatory is Frodsham No. 1369. It was mounted in 1877, and has been running for two years past on a very small and constant rate.

In addition to these instruments, the Observatory is furnished with an excellent 4-in. Clark Comet Seeker, an Altazimuth by Gasella, and the usual barometers, thermometers, etc.

The work now being carried on is chiefly equatorial, and may be divided into two parts, as follows:

1. Double Star Work. A list consisting chiefly of binaries which have been neglected for some years (some of them for ten or twenty, or even thirty years) and will well repay observation. Besides these, a selected list of Burnham's stars, which are suspected of binarity, or which are quite new and have not been observed. Most of these stars are in the southern sky, and including the list for personal equation, will make a total of about 500 doubles. This work is well under way and will probably be concluded within a year.

2. The second part of the equatorial work consists of observations, descriptive and micrometric, upon planets and their satellites, and includes a series of observations extending over several years, upon the satellites of Saturn, and observations upon the red spot of Jupiter since its discovery at Glasgow in 1878.

With the Meridian Circle, no work is done beyond the ordinary observations for time.

The Time-Service of the Observatory, inaugurated within the past year by Prof. H. S. Pritchett, has met with well deserved success, and its value is fully appreciated by the people of the State. Two time balls are dropped by the Observatory clock—one in St. Louis and one in Kansas City—and the clock signals are regularly distributed over a large and constantly increasing area. Owing to its position—almost exactly one hour west of Washington—the Morrison Observatory will doubtless be largely depended upon in regulating the time of the Mississippi Valley, if any of the schemes for "Uniform Time" which have recently been proposed are ever adopted.

Though so well equipped instrumentally, Morrison Observatory, like many a similar institution of longer standing, is sadly crippled for want of funds: its income being barely sufficient for the support of a director without assistance. It is greatly to be regretted that one of the most promising observatories in the country should be thus curtailed in its usefulness, merely for want of proper financial support. W. C. W.

DISCOVERY OF AN ASTEROID.

The Smithsonian Institution has received from M. Foerster, of Berlin, the announcement of the discovery by M. Palisa, at Pola, on the 20th of May, 1881, of a planetoid of the thirteenth magnitude, in

R. A.	15h	3m
Dec.	—23°	2′

with a daily motion of 8m *north.*

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

LOCUSTS AND SUN SPOTS.

To the Editor of "SCIENCE:"

SIR: It may concern some of your readers to know that I have just made the interesting discovery, that the multiplication and migration of the Rocky Mountain Locust (*Caloptenus spretus*), has been hitherto in exact agreement with the minima of Wolf's sun spot cycles as given (Mem. As. Soc. vols. XLII and XLIII), and its decrease has as nearly accorded with the maxima, there not being a year's difference. On European areas, it may be remarked, insect migration but *rarely agrees* with these maxima and minima, the chief periods being obtainable by counting the elevens since 1846. There likewise exists this marked difference, in that while the American locust spreads to the east and west of south, European migrants come north and east.

It would be important to determine the multiplication of the Corn Weevils in relation to the sun spots. Cannot the trade keep diaries? As the more destructive kind comes from the tropic, the minimum period should be dreaded. A. H. SWINTON.

GUILDFORD, ENG., *May*, 1881.

THE VIEWS OF DR. HOLMES UPON THE PROPOSED REVISION MODIFICATIONS OF ANATOMICAL NOMENCLATURE.

We are permitted to publish the following letter from Oliver Wendell Holmes to Professor B. G. Wilder respecting the articles on "Anatomical Nomenclature" which appeared in Nos. 38 and 39 of this journal. It may not be generally known to our readers that "The Autocrat of the Breakfast-table" has been for many years the Professor of Anatomy in the Harvard Medical School. BOSTON, May 3, 1881.

Dear Dr. Wilder:

I have read carefully your papers on Nomenclature. I entirely approve of it as an *attempt*, an attempt which I hope will be partially successful, for no such sweeping change is, I think, ever adopted as a whole. But I am struck with the reasonableness of the system of changes you propose, and the fitness of many of the special terms you have suggested.

The last thing an old teacher wants is, as you know full well, a new set of terms for a familiar set of objects. It is hard instructing ancient canine individuals in new devices. It is hard teaching old professors new tricks. So my approbation of your attempt is a *sic vos non vobis* case so far as I am concerned. There is one term which I do not quite fancy, *pero*, which you couple with *pes* in naming the rhinencephalic lobe. I should prefer the old term *bulbus* with *theca* unless there is some objection I do not see.

What you have to do is to keep agitating the subject,

to go on training your students to the new terms—some of which you or others will doubtless see reasons for changing—to improve as far as possible, fill up blanks, perhaps get up a small manual in which the new terms shall be practically applied, and have faith that sooner or later the best part of your innovations will find their way into scientific use. The plan is an excellent one, it is a new garment which will fit Science well, if that capricious and fantastic and old-fashioned dressing lady can only be induced to try it on.

Always very truly yours,

O. W. HOLMES.

A CURIOUS EGG.

E. E. BARNARD.

One of my hens of the "Dominico" breed is accountable for the presence to-day of a most remarkable egg, which was found in the hen's nest. This singular object measured about three inches in its longest diameter, a round oval in shape, not like the ordinary egg with a large and a small end. The shell was thin and soft to the touch, resembling the "skin" that is found inside an eggshell. Pressing on one end of the egg, a hard object was felt inside the shell. Opening the egg, by cutting with a sharp knife, two eggs were found, one perfect with a hard shell, slightly smaller than the ordinary egg, the other perfect in every respect, save that it possessed no shell. The egg with the shell was enclosed in the white of the other. These two eggs occupied the two ends of the original shell. Upon opening the one with the hard shell it was found to be perfect. Putting the two eggs in separate cups, the one which had the hard shell was slightly smaller and its yolk of a pale yellow; the yolk of the other was somewhat deeper in color.

Here we have a rare phenomenon; first a large egg with slightly soft shell; inside this two eggs, one perfect in a hard shell, the other without shell but otherwise perfect.

NASHVILLE, Tenn., *May* 9.

BOOKS RECEIVED.

A MEMORIAL OF JOSEPH HENRY. Published by order of Congress, Washington, 1880.

The present volume presents in a handsome and convenient form the historical facts relating to the career of Professor Joseph Henry, and a record of the various ceremonies and memorial exercises celebrated after his death in honor to his memory.

The memorial exercises at the Capital include addresses by President Garfield, Hannibal Hamlin, Robert E. Withers, Professor Asa Gray, William B. Rogers, General Sherman and others.

The concluding words of President Garfield's address may well be quoted as conveying the general esteem in which Professor Henry was held by all who knew him. "Remembering his great career as a man of science, as a man who served his Government with singular ability and faithfulness, who was loved and venerated by every circle, who blessed with the light of friendship the worthiest and the best, whose life added new lustre to the glory of the human race, we shall be most fortunate, if ever in the future, we see his like again."

NOTES.

RECENT experiments by M. Grehaut, prove that the quantity of carbonic acid exhaled by any one individual of an animal species is about constant. Fifty litres of air passed through the lungs of a dog, 9kg. weight, yielded 2.747 gr. of CO_2. Eight days after the experiment was repeated, and the CO_2 was 2.810 gr. In man, the same volume of air circulating through the lungs, receives 3.333 gr. of CO_2. Irritations and inflammations of the respiratory mucous membrane (*e. g.* through inhaling sulphurous acid), considerably decrease the exhalation of CO_2. The gas then tends to accumulate in the blood.

GALVANIC GILDING.—M. Rod gives the composition of a bath to be used at temperatures from 50° to 80° C. It consists of 60 parts crystalline phosphate of soda, 10 parts bisulphate of soda, 1 part cyanide of sodium, 2½ parts chloride of gold, and 1,000 distilled water. In order to prepare the bath the water is divided into three portions of 700, 150, and 150 respectively. The phosphate of soda is dissolved in the first lot, the chloride of gold in the second, and the other ingredients in the third. The two first portions are gradually mixed together, and the third is then slowly added. A platinum plate is used as anode.—*Le Monde de la Science.*

RADIATION THROUGH EBONITE.—Captain Abney exhibited at the Physical Society of London, a number of photographic negatives taken by himself and Colonel Festin by radiation through thin sheets of ebonite. The light from the positive pole of an electric lamp was sent through a thin sheet of ebonite $\frac{1}{64}$in. thick, and photographs taken showed the radiation to have a low wave-length, from 8,000 to 14,000. The carbon points of the lamp could be photographed through the sheet, and Colonel Festin observed the sun's disc through it. The ebonite showed a grained structure, and different samples of ebonite gave different results, but all gave some result, in course of time at least; old ebonite, like that used in some of Mr. Preece's experiments, scattering the light more than new ebonite. Dr. Moser exhibited the passage of the rays through the ebonite to the audience by means of a galvanometer. Professor Guthrie observed that Captain Abney had proved that light as well as heat traversed the ebonite, and Dr. Coffin stated that compositions of ebonite, apparently the same, might vary considerably.

PHOTOGRAPHIC PHOTOMETRY.—A promising application of photography to precise measurement of phenomena of light has been recently tried by M. Janssen. The method is advantageous in that photography reveals the action of the extremely weak luminous and the ultra-violet rays; but the chief advantage lies in the permanence of the results as against the fugitive nature of ordinary photometric comparisons, which, too, require the simultaneous presence of the two light sources. The various amounts of metallic deposit on the photographic plate cannot well be weighed, so M. Janssen measures by the degree of opacity produced. His photometer consists of a frame with sensitised plate, before which is passed at a known rate of uniform motion a shutter having a slit. If this slit were rectangular, a uniform shade would be produced on the plate; but by making it triangular he obtains a variation of shade, decreasing from the side corresponding to the base of the triangle to that corresponding to the apex. It is further proved that the photographic deposit does not increase as rapidly as the luminous intensity. Now, to compare the sensibility of two plates differently prepared, they have merely to be exposed successively in the frame under like conditions, and the points where they show the same opacity being compared to the points of the triangular slit corresponding to them, the ratio of the apertures at those points expresses the ratio of sensibility. Thus the new gelatino-bromide of silver plates are proved to be twenty times as sensitive as the collodion plates prepared by the wet process. Again, to compare two luminous sources, they are made to act successively on two similar plates in the photometer, and the points of equal shade in the plates indicate, as before, the relation sought. M. Janssen has compared the light of the sun and some stars on these principles, preparing from the former "solar scales" (with uniform degradation of shade), under exactly determined conditions as to sensitive layer, time of solar action, height of the sun, etc. Circular images of stars are obtained by placing a photographic plate a little out of focus in the telescope, and a series of these, got with different times of exposure, are compared with the scales obtained from sunlight. M. Janssen will shortly make known some of his results.

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JOHN MICHELS, Editor.

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SATURDAY, JUNE 11, 1881.

We have received a copy of the *Annuaire de l'Observatoire Royal de Bruxelles*—a book of nearly four hundred pages, published under the supervision of Dr. J. C. Houzeau. This number is the forty-eighth issue of the series, and contains the customary data regarding calendars; rising, setting and meridian passages of the sun, moon and planets; eclipses of the sun and moon, and transit of Mercury; occultations of stars by the moon; eclipses of the satellites of Jupiter; positions of fixed stars; elements of the planets and their satellites, and of the periodic comets; various data pertaining to weights and measures, geographical positions, etc. It is a noteworthy fact, that while the astronomical repertoire supplies a need for Belgium—as the similar *Annuaire du Bureau des Longitudes* does for France—we have no like publication in America. It must cost really very little to print it, and the expense of compilation can not be great. It is not a little remarkable that Americans generally should so long be content with dependence upon patent medicine almanacs for this class of information.

Among the appended articles, we note a few which carry more than a passing, special interest—*Le Globe Terrestre—Quel est le Climat le plus Favorable au Développement de la Civilisation?—L'Astronomie dans l'Antiquité—L'isthme de Panama.* Monsieur L. Niesten, a well known astronomer of the Royal Observatory, contributes no less than four articles to this issue of the *Annuaire*, two of which appear to have been prepared with great care, and are astronomically of much importance. The last transit of Mercury, May 6, 1878, was very fully observed everywhere, and M. Niesten deserves much credit for his well arranged digest of every sort of observation on that occasion. Those who are concerned with general relations on the rapidly multiplying group of small planets will get a deal of information from Niesten's article, *Les Astéroides*—which is, in fact, a comprehensive history of these bodies. An accompanying map serves to bring out some points which are made clearer by graphical representation. Astronomers and others will have frequent occasion to refer to an article (which it is remarkable should not have long ago been prepared by some one)—*Nomenclature des Observatoirés Astronomiques Existants, qui out la Caractére d'Etablissements Publics.* About 120 observatories are included in this list, and there are given, as far as known, the year of founding, the connection of the observatory, some brief description of the instruments, and the names of all the directors of each establishment, including the dates of their installation.

THE AMERICAN CHEMICAL SOCIETY.

The June meeting of the American Chemical Society was held Friday evening, the 6th inst., Prof. A. R. Leeds presided. Mr. A. P. Hallock was elected a regular member. The first paper before the Society was by Dr. Chas. A. Doremus, "On the Composition of Elephants Milk." The sample was obtained from the mother of the baby elephant "America" which is now on exhibition in this city. The baby weighed 213½ pounds at birth and at the end of a year turned the scales at 900 pounds. Considerable difficulty was experienced in procuring the sample, and but a very small quantity was obtainable. Three analyses were made and the figures are herewith given:

	I. April 5. Morning.	II. April 9. Noon.	III. April 10. Morning.
Quantity	19cc.	36cc.	72cc.
Cream, per cent	52.4	58.	62.
Reaction	Neutral.	Slightly alkaline.	Slightly acid.
Sp. Grav	....	......	1.0237
In 100 Parts by Weight.			
Water	67.567	69.286	66.697
Solids	32.433	30.714	33.303
Fat	17.546	19.095	22.070
Solids in fat	14.887	11.619	11 233
Casein	14.236	3.694	3.212
Sugar		7.267	7.392
Ash	0.651	0.658	0.629

It will be noticed from these analyses that the milk is peculiarly rich in the nitrogenized materials. The volume of cream compared with that obtained from an Alderney cow is also quite large. Under the microscope the milk globules appeared very uniform in size and were unusually clear. Although it is generally claimed that the fat when burned emits a peculiar odor by means of which it is possible to distinguish the animal from which it has been obtained, yet in the present instance no odor was perceptible from the fat which was separated from the milk. This is the only analysis of elephant's milk on record, and Dr. Doremus is certainly deserving of much credit for the interesting information which he has obtained. His entire paper will be published in the proceedings of the Society. An analysis of the milk of an hippopotamus is added for the sake of comparison:

Water	90.43
Solids	9.57
Fat	4.51
Casein, and milk sugar	4.40
Ash	0.11

SLEEP AND SOMNAMBULISM.*

By M. Regnard.

[Translated From the French by the Marchioness Clara Lanza.]

II.

Ladies and Gentlemen:—In the middle ages and up to the present century Somnambulists, together with epileptics and hysterical women, were classed with sorcerers and those supposed to be possessed by the devil. They were exorcised with others afflicted in similar ways and generally burned alive with great ceremony and rejoicing.

During this unenlightened era, one man of genius proved himself. in regard to Somnambulism, to be an exceptional observer and has left us a description of it which no modern neurologist would repudiate. His name, gentlemen, is upon your lips. It is Shakespeare who in his tragedy of *Macbeth* has given us a masterly description of automatism. You are all familiar, doubtless, with the sleep-walking scene, but I will recall a portion of it to your mind. After having committed her terrible crimes Lady Macbeth has attacks of Somnambulism. One of her ladies in waiting informs the court physician and both sit up at night to await the coming of the queen.

"*Gentlewoman*—Lo you, here she comes! This is her very guise; and, upon my life, fast asleep. Observe her: stand close.

Doctor.—How came she by that light?

Gentlewoman.—Why it stood by her: she has light by her continually: 'Tis her command.

Doctor.—You see her eyes are open.

Gentlewoman.—Ay, but their sense is shut.

Doctor.—What is it she does now? Look how she rubs her hands.

Gentlewoman.—It is accustomed action with her, to seem thus washing her hands; I have known her continue in this a quarter of an hour.

Lady Macbeth.—Yet here's a spot.

Doctor.—Hark! She speaks: I will set down what comes from her, to satisfy my remembrance the more strongly.

Lady Macbeth.—Out, damned spot! Out, I say!—one; two; why, then, 'tis time to do 't:—Hell is murky!—Fie, my lord, fie! A soldier and afeard! What need we fear who knows it, when none can tell our power to account?—Yet who would have thought the old man to have had so much blood in him?

Doctor.—Do you mark that.

Lady Macbeth.—The Thane of Fife had a wife; where is she now?—What, will these hands ne'er be clean? No more o' that, my lord, no more o' that; you mar all with this starting.

Doctor.—Go to, go to; you have known what you should not.

Gentlewoman.—She has spoke what she should not, I am sure of that; heaven knows what she has known.

Lady Macbeth.—Here's the smell of blood still; all the perfumes of Arabia will not sweeten this little hand. Oh! Oh! Oh!

Doctor.—What a sigh is there! The heart is sorely changed.

Gentlewoman.—I would not have such a heart in my bosom for the dignity of the whole body.

Doctor.—Well, well, well.

Gentlewoman.—Pray God it be, sir.

Doctor.—The disease is beyond my practice; yet I have known those which have walked in their sleep, who have died holily in their beds.

Lady Macbeth.—Wash your hands, put on your night-gown; look not so pale—I tell you yet again, Banquo's buried; he cannot come out of his grave.

Doctor.—Even so.

Lady Macbeth.—To bed, to bed; there's knocking at the gate. Come, come, come, come, give me your hand; what's done, cannot be undone. To bed, to bed, to bed.
Exit.

Doctor.—Will she go now to bed?

Gentlewoman.—Directly.

Doctor.—Foul whisperings are abroad; unnatural deeds
Do breed unnatural troubles. Infested minds
To their deaf pillows will discharge their secrets.
More needs she the divine than the physician.
God, God, forgive us all! Look after her:
Remove from her the means of all annoyance,
And still keep eyes upon her. So good night;
My mind she has mated, and amazed my sight;
I think but dare not speak.

Gentlewoman.—Good night, good doctor.

Gentlemen, do you not think this fine description contains all the details I previously gave you, and that Shakespeare has shown himself scientifically superior to all who have hitherto attempted to represent this singular nervous affection?

I have now finished what I had to say about natural Somnambulism and find myself confronted by the most difficult point of my subject, provoked or induced Somnambulism—Magnetism if you insist upon my employing that detestable word.

It is quite possible by means of various practices which I shall make known to you later, to produce a nervous affection very similar to Somnambulism, but yet differing from it in several ways. The effects obtained depend of course upon the subject and the methods employed, and the conditions resulting from these may be divided into three, all of them however, being sometimes induced in a single person. These three states are:

1. Hypnotism.
2. Sleep.
3. Catalepsy.
4. Automatism.

Gentlemen, during the latter part of the foregoing century an Austrian physician of great repute, seemingly, arrived in Paris. His name was Mesmer and he had discovered the means, by a purely physical process, of producing certain effects upon the human organism which were considered to be perfectly prodigious. Mesmer appeared first about the time when great excitement was being caused by the first discoveries in electricity, made by the Abbè Nollet, and when the singular action produced upon a magnetized needle by a fluid apparently permeating the earth, attracted universal attention. Mesmer announced that he was master of another fluid which was but a modification of the terrestrial one and which operated upon the vital forces, and when properly directed could become a most important curative means.

He made an offer to the government to sell his secret which he estimated to be worth several million francs. The French ministers, however, were prudent and allowed Mesmer to keep the great mystery to himself.

His method had nothing about it resembling real magnetism. His performances took place in a partially darkened room in the middle of which was placed a large tub generally covered. A number of rods were placed crosswise on the top around which the people seated themselves. Soon the sound of a piano was heard, while the atmosphere grew heavy with perfumes. Mesmer walked about the room with a prophetic air, touching the forehead of each person, and executing a series of theatrical gestures. The subjects then fell into a comatose state. They remained in ecstasy, almost entirely deprived of sensibility and movement, and only recovered under the influence of broad daylight and fresh air.

There was not a bit of Magnetism in all this. The subjects were generally hysterical women. Their imagination was greatly excited and the same thing recurred to them as now happens to those persons we hear of as being afflicted with religious mania, etc.—they were hypnotized.

*A lecture delivered before the *Association Scientifique de France.*

To Mesmer we cannot even give the credit of invention, for hypnotism or uncompleted Somnambulism or ecstatic sleep, as you choose to call it, occupies still as it did then a high place among certain religious sects. It is nothing more than *ecstacy*, where exterior comprehension is lost and replaced by a series of visions *en rapport* with the preoccupation of the subject. I will show you presently that although ecstasy is generally of a religious character, there are many exceptions and that in fact any vivid mental emotion can provoke it.

The fakirs of India frequently induce the condition, not by absorbing themselves in some holy or poetical idea, but simply by gazing fixedly at space or some bright object or spot; some of them look at the end of their nose. The Grecian monks are also celebrated for being able to produce Hypnotism by looking steadily at a certain point or thing, and will remain insensible for hours. The result of this is that they enjoy the reputation of either holiness or witchcraft, according to the form of the delirium which usually follows.

At all times that which was called *contemplative asceticism* has been produced by fixing the gaze upon some brilliant or shining object to which was attributed some particular virtue or sacredness. These contemplations, together with violent mental excitement, were rapidly succeeded by hallucinations, apparitions, and in short, ecstasy.

Mahometanism even, although not particularly mystical, has likewise produced special forms Hypnotism. A prolonged and monotonous sound in these cases was more effectual than a fixed gaze.

Among the disciples of Hussein, the martyr, ecstacy is induced by means of tambourines beaten incessantly in a rapid and monotonous manner, accompanied by measured chanting. This ceremony frequently occurs at night, and in a short time the subjects are in a state of ecstacy, in which cutaneous insensibility is so marked that all the tortures undergone by the martyr can be likewise inflicted upon them without eliciting a cry or groan.

But these phenomena are shown in still more intense a manner in the sect of Aissaoua, many representations of which can be met with in our Algerian colony. Those who have had the rare good fortune of witnessing one of their ceremonies have been struck with the degree of anæsthesia which seems to affect these people.

The ceremony takes place at night, generally in some deserted plain. The tambourines keep up a constant monotonous sound. The subjects seat themselves about an immense fire and gradually fall into a condition of ecstacy. Some of them writhe convulsively and utter prolonged cries. Anæsthesia becomes complete and then some can be seen applying their tongues to bars of red hot iron, while others eat Barbary figs, the long thorns of which come directly through their cheeks from the inside, causing their faces to stream with blood. Still others swallow live spiders and scorpions, which remarkable feats often result very seriously.

In short, all Hypnotics proceed precisely the same way, by fixing the eyes, generally squinting, upon a certain point, or else listening attentively to a monotonous sound.

These methods which have been and always are employed to produce the phenomena, are, as we shall see, quite determined.

We are indebted to Braid for the first well regulated and experimental work upon Hypnotism, and in 1841, this English surgeon, after having witnessed so-called magnetic experiments, discovered that the prolonged fixture of the eye or hearing, and not a mysterious fluid, was the source of the incontestable phenomena he had observed. Scientific Magnetism, we may say, began with Braid.

He knew a series of experiments, for the most part extremely curious, which had just been made in France by Dupotet and Puységur. These two men, who were imbued with Mesmer's ideas, had wondered if the tub were really necessary, and if the magnetic fluid we all possess could not be transmitted from one person to another. They therefore procured a number of nervous persons and endeavored by a series of motions which nowadays we designate as passes, to realize some palpable effects. By this means sleep was produced much more rapidly than by Mesmer's method. Magnetism had been effected by communication, and it exists to this day, considerably augmented and enriched by all sorts of inconceivable folly.

Braid asked himself whether passes did not constitute a simple hypnotic process, and whether the contemplation of a fixed or moving point would not produce the same result as all these absurd magnetic gestures. His experiment was crowned with success, and his subject fell into the hypnotic sleep by simply looking at a metal ball. The magnetic fluid had been overturned!

The condition formed in this purely physical manner was such, and the insensibility so complete, that Braid was able to operate upon the subjects, and even amputate their limbs. His experiments were repeated in France by Broca, Verneuil and Laségne, the same results making themselves apparent.

Unfortunately, hypnotism cannot be induced with everybody. A number of unsuccessful attempts have always been observed, and then came the introduction of chloroform and ether. Braid's experiments were lost in oblivion until a courageous French *savant*, Professor Charcot, took them up, and brought them to points, which I shall proceed to demonstrate.

But first of all, let me show you some experiments in hypnotism. Animals can be hypnotized by Braid's process as well as human beings.

Here is an old experiment borrowed from Father Kircher. I take a hen and place it upon this black table in a sitting position, its head resting on the table. I then trace a chalk line from the end of its beak, upon which its eyes are instantly fixed. I remove my hands, and you see the hen remains motionless. I can pinch it and burn it, still it does not move. If I replace the chalk line by an electric light, the effect will be still more intense. This fact is equally noticeable in man, a sudden surprise can produce the same effect. I seize the chicken brusquely and place it rudely upon the table. It is motionless, hypnotized, Preyer says cataleptic, the word is, however, of no consequence. The same experiment is very successful, you also see, with a sparrow. If the bird's head be placed beneath his wing, the hypnotic sleep lasts a very long time.

A Guinea pig can be easily hypnotized. I take one of these little animals, a female, for M. Laborde has shown that the experiment is only successful with this sex, and I extend it brusquely upon its back. You see that it remains as I have placed it without moving, and that it is insensible, for I pinch it with all my strength.

Here is another one upon whose ears I hang some brilliant bits of steel. It turns its head from side to side to look at them, and now has fallen asleep so soundly that I cannot wake it. I fire a pistol so close to its ear that its moustache is singed, but it does not move.

These animals are hypnotised; their condition consists in a total loss of sensibility. But they are not asleep, they do not dream, they are not somnambulists.

Hypnotism can be produced in almost any one who makes himself perfectly passive. But if you experiment upon one of those persons whom we call hysterical you will obtain quite a different condition. The same means bring you to *artificial somnambulism*. The difference in the subject produces the difference in the effects. Here it is that M. Charcot's experiments and the Salpêtrière investigations begin, in which I was kindly permitted to assist.

I must first of all tell you what a hysterical subject is and what constitutes the principal phenomena she presents, for we shall see that her condition of Somnam-

bulism is a mere modification, sometimes a simple reproduction of them.

A hysterical woman at first sight cannot be distinguished from any other, unless we except a rather strange expression of face and a peculiarity of dress. These persons always cover themselves with several loud colors which do not harmonize in the least. I shall soon tell you the reason of this.

The first thing to be observed in them is *anæsthesia;* hysterical women are sometimes paralyzed on one side of the body and sometimes on both. They can then be pierced with long needles without feeling anything whatever, and fall into all sorts of singular errors as one side of their body seems to be dead. They do not know where their arms or legs are unless they look at them. Sometimes they allow themselves to be burned without percieving it. One day, a hysterical patient at La Salpêtrière found a hole in the stocking she was about putting on. She sewed it up, and walked about all day. On going to bed that night she was unable to remove the stocking, and on calling for help it was discovered that she had sewed it to her foot.

A French physician, M. Bureq, has shown that the application of metal to the insensible parts render them sensible. This is called *metallo-therapy*, and singularly enough, the committee who examined this phenomenon affirmed that while sensibility returned to one arm, for example, it disappeared in the other at precisely the same point, so that the subject was in no wise benefited.

Anæsthesia of the skin also extends to the other senses. Hysterical women do not hear well, their sight is defective and, generally speaking, they are unable to distinguish colors; sometimes with one eye and often with both they are *achromatopsic;* everything looks gray to them. Their senses are therefore in a state similar to sleep, from which certain exciting influences such as metals, electricity, etc., can rouse them temporarily.

Their muscles are frequently paralyzed. There is nothing in fact, more common than a hysterical paralytic. Sometimes the muscles are violently contracted, and remain thus for years. An intense emotion can suddenly stop the paralysis in contraction. I need not tell you how this is achieved.

These contractions also, can be induced easily. It is only necessary to rudely sieze the arm of a hysterical woman and it will remain contracted in whatever position you place it. In short, these people have periodical attacks in which they reproduce nearly everything that we can obtain from them by magnetism.

When a hysterical woman is about to have one of these attacks, the first thing she experiences is a certain uneasiness and discomfort, as though a ball rose from her stomach and remained stuck in her throat. This is nothing more than muscular contraction of the œsophagus. Suddenly, she utters a loud cry and falls backwards. Her eyes roll wildly and a sort of foam appears upon her lips. Simultaneously, her arms are violently extended and her clenched hands turned towards the inside. The entire body becomes as rigid as in an attack of Tetanus. Then the patient utters a prolonged scream, bends her body in the form of an arch in such a way that her weight is sustained solely upon the head and heels. This period is succeeded by all kinds of disordered movements which last from two to three minutes. Then contraction begins. Sometimes the whole body contracts, sometimes only a portion. In this way, the contraction of the arms frequently places the patient in the attitude of the crucifixion and this last generally for days accompanied by complete insensibility. Then intervenes a period of repose. One would say that it was all over and that the patient slept. But indeed it is but the beginning of the final and most interesting period of all, the ecstasy which M. Charcot has termed *attitudes passionnelles.* The patient absolutely ignorant of all her surroundings, neither perceiving sound or light, begins to follow out a dream which has the peculiarity of being always the same and is the reproduction of some event, or series of events, belonging to her existence. My friend M. Bourneville, physician to L'Hospice de Bicêtre, and myself have published a book wherein all these facts are minutely described. It is called the *Sconographic photographique de la Salpêtriere* and comprises the entire study of hysteria as well as Somnambulism. The descriptions are completed by a series of pictures produced by an instantaneous photographic process, and these I shall now proceed to show you.

In the *attitudes passionnelles,* the hysterical patient is really a spontaneous and automatic somnambulist. You will now understand why it will be so easy presently to put her in a condition of artificial Somnambulism. I will show you some *attitudes passionnelles.* The patient sees some frightful object as you may imagine by her terrified position. But see, her features relax and here we have religious ecstasy. Once more the scene changes to give way to this when she keeps time to music which she thinks she hears.

The young girl represented in these photographs has been subject to these attacks for six years. Her hallucination or dream has never changed in a single detail, and there are a hundred more precisely like her in Paris.

Gentlemen, you will probably ask if this terrible disease, so much talked of at the present day, is new—if it is a production of this "nervous century," if I may so express myself, or whether it is of ancient date. My reply is a simple one. Hysteria is as old as humanity itself. No matter how far back you may travel in the history of the world, you will always find it. What, indeed, were the pythonesses, the ancient sibyls, the sorceresses and possessed of the middle ages, if not somnambulists and hysterical women? The descriptions of their paroxysms cannot leave us in doubt, for their characteristics are plainly shown. Do we not know that they were pricked and burned without being aware of it. And did not this very fact prove that the devil had set his stamp upon them, and did it not invariably result in their being butchered alive? Better still, painting assists us to form a vivid impression of these attacks. Look at the "possessed" which figure in the works of Rubens, Raphael, Jordaens and Breughel, and you will immediately recognize the attitudes which I have just shown you in the photographs. Here are some copies of these famous pictures. Look at them and see if you can doubt for a moment.

This long diversion I have made purposely, that you might fully comprehend the precise ground upon which we stand. The means employed to produce Hypnotism can induce hysterical manifestations similar to those produced spontaneously. These manifestations are *artificial Somnambulism, Catalepsy* and *Automatism.*

To provoke Somnambulism requires a very simple mode of operation. It is the same as that employed to induce Hypnotism. You can make the person fix her eyes upon a bright object. Ordinarily, however, you seat yourself directly in front of her and tell her to look at you steadily. After a minute or two has elapsed, you see her eyes assume a vague expression, then fill with tears, and finally, in a short time, varying from a minute to a quarter of an hour, according to the subject, they close, the head falls and sometimes a little foam appears upon the lips. Sleep is produced, real sleep accompanied by total loss of sensibility. This is, therefore, more than Hypnotism.

If the subject is restless, her thumbs can be held in the closed hand. As for passes, I have always observed that they retard the sleep instead of promoting it. M. Richet, on the contrary, places great faith in these movements.

You see, gentlemen, that nothing can be more simple. A little patience the first few times and the thing is done. There is no fluid, be it understood; the *magnetizer* has

nothing individually to do with the phenomenon. All that takes place originates with the *subject* whose brain is actually annihilated and brought to such a condition that any dream can be provoked by *suggestion.* We have in fact, an automaton similar to that which I called your attention to in Natural Somnambulism, only while the latter merely obeys the impulse of memory, the former is subject to our will.

Hypnotism can also be produced by simply placing the thumbs gently upon the closed eyelids of the subject, allowing the hands in the meanwhile to rest upon his temples and press upon the eye-balls. This process is very effectual with some subjects. A person accustomed to be hypnotized can be put into the condition by having some one shout suddenly and authoritatively in his ear, "Sleep!" A theatrical gesture accompanying the command makes it more effective. The Abbé Faria, a celebrated charlatan who completely mystified the world about twenty years ago, always adopted this method. The other ways, however, are preferred at La Salpêtrière, and also at Breslau by the well-known Professor Heidenhaim.

All that I have just said refers to the first experiments made with subjects. After they have once been hypnotized, however, the state can be induced much more easily. Here it is that Imagination steps in and mountebanks are allowed the utmost liberty of action. The mere idea that he is about to be put to sleep causes the subject to fall asleep almost immediately. If, in addition, he is made to think that the operator possesses some secret influence, or supernatural power, you will soon see what may happen.

A patient at La Salpêtriére, who had firmly persuaded herself that I had a peculiar influence upon her, fell into a hypnotic condition every time she saw me, independent of the locality. She often became hypnotized upon the staircase or in the middle of the courtyard. One day some one said to her jokingly that she could be hypnotized simply *by the will* in the midst of a public ceremony which was to take place a few hours later, and she actually refused to appear on this occasion so fully was she convinced that what had been mentioned would really occur. In such cases, the imagination is everything. The subject alone is responsible for all that happens. A few examples will make you thoroughly understand what I mean. I have actually succeeded in persuading patients that they could not leave the room because I had magnetized the door-knobs. They would hesitate for a long time before approaching them, and ss soon as they touched them they became hypnotized. Need I tell you that nothing whatever had been magnetized? This experience is important, for by means of it we can explain cases in which the subjects fall into the condition while drinking a glass of *magnetized water*, or while lying down beneath a *magnetized tree.*

Magnetic experiments made at a distance belong to the same category. How often we have read of magnetizers who have succeeded in putting subjects into a deep sleep while the former is in one room and the latter in another. Here again the subject alone is the agent, I have frequently tried this experiment. A patient named P—— was told, "M. Regnard is in the next room and he is magnetizing you." She would instantly exhibit great uneasiness and then fall into a deep sleep. This even happened when I was not in the next room or even in France, and when, I am free to confess, I was thinking of anything rather than her.

On another occasion I said to a patient that I would magnetize her at three o'clock in the afternoon, and ten minutes after making this remark I had forgotten all about it. On the following day, however, I learned that she had fallen asleep precisely at three o'clock.

The immense number of absurdities which go to compose magnetizer's guide books can be explained in this way. The imagination of the subject is vividly affected and sleep is produced subjectively and without the intervention of any exterior influence. No matter what manner of *Magnetism* is employed the result is always precisely the same—the subject remains inert.

Different peculiarities are then observable, the most important of which is hyper-muscular excitability. In a normal condition our muscles are very susceptible. Any violent check causes them to contract, and the same effect is often produced by reflex action.

In artificial Somnambulism, the action of the spinal cord being no longer moderated by the brain which is annihilated. the muscles contract by reflex action beneath the very smallest influence. Pass your finger as lightly as possible over the forearm of a sleeping hysterical woman and you will immediately perceive muscular contraction. Charlatans obtain this effect by gently touching the muscles while apparently only making passes. By causing the muscles of the back to contract subjects can be made to assume positions which appear incompatible with the equilibrium. Here are a couple of photographs taken of two somnambulists. One of them, you see, has her head thrown back until it nearly touches her waist, while the other rests with her feet on the back of one chair and the nape of her neck on the other, her body bent in the form of an arch. I show you these two positions, so frequently exhibited by would-be performers of miracles, simply that I may explain to you how I obtained them.

All the results reached so easily in magnetic sleep are nothing more than hysterical muscular contraction. This can be proved by the fact that if the patient is awakened during this state, the contraction remains indefinitely, and in order to remove it she must be put to sleep again, and antagonistic muscles contracted.

The study of this important branch of the subject led M. Charcot and his students to the investigation of a most curious thing, and one which has helped to calm the fears of people, who, without witnessing the experiments performed, denounced the whole affair as an imposition.

Gentlemen, there are about two thousand persons in this room. With the exception of a few physicians who are present, it is probable that none of you know the action of the muscles as described by Duchenne, of Boulogne, nor yet the distribution of the nerves situated in the arm. Do you believe that a girl who can neither read nor write, and who comes from the most obscure portion of Brittany, could be versed in the details of this delicate physiology? For my part, I do not believe it. If she is an imposter, we shall soon discover it. Here she is; let us hypnotize her, and then excite the cubital nerve at the elbow, and see if she makes a wild gesture. Not at all; she merely bends her little finger, the third finger and thumb. The cubital nerve therefore, only affects these three fingers. Many medical students of my acquaintance are ignorant of this fact. Let us now excite the sterno-mastoid muscle, this diagonal cord which appears upon the neck when the head is turned. You see, she turns her head towards the opposite side. Better still, let us excite the face muscles with this pencil, and you perceive the same effects appear as were obtained by Duchenne with electricity, such complex effects, too, that even physiologists have difficulty in remembering them. If this girl is only pretending, she is exceedingly clever. I shall have finished my remarks upon sleep, after having told you that it is quite possible, at this period, to make the subject rise and follow you, and utter loud cries, should any one come between you and her.

The second state which can be produced by Hypnotism upon hysterical subjects, is Catalepsy. This *bizarre* condition, of which I shall attempt to give you an idea, exists normally in the patient, and the processes employed only serve to develop it. Sometimes it appears without any provocation whatever. Nothing is easier than to make the subject pass from a sleeping state to a cataleptic one. It is only necessary to open his eyes suddenly, and he will then remain as though transfixed. His eyes assume a

set stare, and whatever attitude you cause him to take, he will remain in indefinitely. He can be placed in the most trying postures, and he will stay just as you have put him, and as long as you choose. I have here some photographs of several people taken while they were in this state. You can see how impossible and extraordinary the postures appear, and how they can be maintained for a great length of time. I may say, however, that nothing can be easier than than this kind of photography. The subjects never make the slightest movement, and it is even pretended that the celebrated sculptors of antiquity made use of cataleptics as their models. This may not be true, but it is quite possible.

There are other ways of inducing Catalepsy. Do you recollect the process generally employed to produce sleep? It was the sight of a brilliant object, or the prolonged noise of a monotonous sound. The same means are physically made use of to induce catalepsy. Let us suppose, for instance, that a hysterical subject is made to listen to the prolonged vibrations of a single octave struck upon the piano. Nothing is more irritating than this monotonous sound. The subject rapidly falls into a cataleptic state, and singularly enough, remains in it as long as the octave is struck. As soon as the sound ceases however, the catalepsy disappears.

What is thus produced by a sound can also be caused by intense light. Here are a few subjects whom I will place directly in front of this electric light. You see they become cataleptic instantly. If the light is extinguished you perceive they will fall backwards into a non-cataleptic sleep. A sudden noise or an instantaneous flash of light can produce the phenomena equally well. I remember witnessing a curious scene one day at La Salpêtrière. It was during some public ceremony, and a military band was playing in the court yard of the establishment. One of the patients under the care of M. Charcot listened to the music with the most intense delight. Suddenly there came a clash from the brass instruments which made us all tremble, but the patient fell into a cataleptic state and had to be carried from the room. A short time after another patient went during a holiday to a concert. No doubt on that occasion the musicians performed some "music of the future," for the patient suddenly fell into a cataleptic condition, and had to be removed.

It is very easy to reproduce these phenomena. It can be done by beating a Chinese gong unexpectedly in the subject's presence. You all know what a disagreeable sound it is. The patient makes a gesture of fright and remains rooted to the spot in a state of catalepsy. A sudden explosion of gunpowder is equally effective. I must tell you however, gentlemen, that this last experiment has its disadvantages. Catalepsy produced in this way often terminates with an attack of hysteria. On one occasion it was followed by a sort of frenzy which lasted five days and then stopped spontaneously.

While in a state of catalepsy the subject is not cognizant of his surroundings. He neither sees nor hears, neither does he speak, differing in this latter respect from the somnambulist or hypnotic subject. The muscles, moreover, are not hyper-excitable. It is singular, however, that while in this condition it is exceedingly easy to provoke automatism by *suggestion*. Take for instance a cataleptic subject. Place him in an attitude expressive of anger, love, expectation or prayer. His face will immediately assume the expression required to complete the effect.

The second degree of automatism is a little more complicated, and will recall to your mind the effect obtained with somnambulists when an idea suggested to them produces others. Veritable hallucinations can be formed in this way.

To obtain this result, place yourself in front of a subject who is in a cataleptic state and endeavor to attract his attention. This is the difficult point inasmuch as nearly all his senses are annulled. When you have succeeded, however, make a motion for example, as if you were trying to catch a bird. This gesture will immediately suggest an idea to the subject and be followed by a series of conceptions. The catalepsy ceases instantly and is succeeded by automatism. The subject rises, begins to run rapidly. His mind gradually wakens, a dream commences and generally speaking nothing is more curious than to watch its development. Sometimes he appears to be fleeing from a serpent, at others from an apparition, and so real do the hallucinations appear to him, that he would dash through a glass door or out of a window while attempting to escape from or follow his illusion. I may add that if the act suggested be quickly realized, the subject will repeat it indefinitely. If I place a cake of soap in his hands he will go through the motion of washing them interminably. One patient that I had continued for three hours and would have gone on still longer had I not stopped him.

I have shown you how you must proceed to induce the condition. Now I must tell you how to dismiss it. It is very simple. Magnetizers make passes, physicians merely tap the subject's cheek lightly with their fingers, or sprinkle a few drops of water upon the face. I must also tell you that it is not desirable or prudent to allow the state to continue for a long time. Two subjects I have seen nearly died in consequence of remaining in a cataleptic condition twenty-four hours. Respiration nearly ceased, the heart beat almost imperceptibly and asphyxia followed. Death, undoubtedly was not far off.

Gentlemen, I am done. I have told you all that I know, all that I have seen in regard to this famous *animal magnetism*. I have said nothing, however, about reading with bandaged eyes or by means of second sight, nor have I spoken of divination or the art of curing disease by magnetism. Such things have no place in Science. They are not mentioned at Sorbonne. Our asylums of Bicêtre and Charenton, or our court-rooms seem to me the only places where they may be discussed from time to time.

After all it is not, perhaps, astonishing that such *bizarres*, physiological facts as those I have just demonstrated, should have tempted charlatans and deceived imbeciles.

Before I leave you, gentlemen, let me tell you what I fear and what I wish.

I fear that while speaking to you so earnestly about sleep I have performed my best experiment. Do you recollect?—words succeeding words like the monotonous tic-tac of a clock, and only when the sound ceases does the audience awake with a start.

But away with this fatal thought, and allow me to tell you what I wish. I hope that I have succeeded in convincing you that all the astounding facts connected with Magnetism and Somnambulism are merely pathological exaggerations, diseases of sleep. That they are absolutely determined, that they can be produced when and how we choose upon particular subjects, without any magnetic fluid and without the aid of superior or supernatural forces. If I have persuaded you upon these points, I have destroyed one of the most laughable superstitions which still exist in the world, and this evening cannot be considered as thrown away as concerns either you or me. For my part, I shall always look upon it as one of the happiest and most profitable of my life.

VOLTA'S BATTERY.—To render Volta's battery constant and depolarized, Count Mocenigo fixes twelve couples with their elements on a horizontal axis, a trough of acidulated water having twelve compartments is then brought up by a lever motion so as to cover a good third of the surface of the battery, and a rotatory movement is communicated to the axis.

FOSSIL ORGANISMS IN METEORITES.*

By Geo. W. Rachel.

Dr. Hahn's work, of which "SCIENCE" gave a short notice in its last issue, promises to revolutionize many views which have heretofore been believed to be firmly and irrevocably established. It is not at all necessary to accept all the conclusions and agree with all the various lines of reasoning, into which the author has been led by his results, but nobody will fail to perceive the portentous meaning of the results with which his untiring efforts in this important matter have been rewarded.

There has been formerly a manifest tendency to belittle small things and apparently insignificant phenomena, and bestow the greatest attention on those matters which impress the observer by their magnitude. Modern science has done away considerably with this erroneous method and has taught us that it is the little things which achieve great results in nature, as a rule. To this class of phenomena, which has been habitually underrated until a comparatively recent time, belong the meteorites, shooting stars and meteoric dust generally. Chladni's view that they fall from the skies, pronounced in 1795, was ridiculed by the learned men of the times. One member of a committee sent by the French Academy to investigate the fall of a meteorite in the neighborhood of L'Aigle, Le Luc, declared that he would really be forced to believe what the people who witnessed the fall said, if he did not know that such a thing was *utterly impossible*.

It was not long, however, until the celestial origin of these bodies was universally recognized, several other falls of large meteorites occurring during the first decade of the present century, which could no longer be explained away. After this various stones that were known to have fallen upon the earth were examined and described, and a good many more which were recognized to be of celestial origin. The number of all the various specimens thus investigated has gradually become very large. Kesselmayer, in his great work on the subject, describes 647 distinct falls.

It is not now necessary to recall the several results of these investigations, nor to describe the peculiar properties of meteorites on which the resemblances and differences between those celestial minerals and our terrestial rocks are based. Suffice it to state that between the two types which have been recognized, viz: those consisting exclusively of iron, and those which are composed of certain silicious minerals, such as Augite, Bronzite, Olivine, Anorthite and other Feldspars, there are all the possible combinations of both; the ferrous meteorites predominate, however, those with a considerable percentage of silicious constituents being comparatively rare, and the purely silicious still more so.

It is the latter, the silicious material, which has been examined with such remarkable results by Dr. Hahn. This occurs usually in light-colored spherical or pear-shaped masses (χονδροὶ) similar to the nests of crystals (druses) which are a well-known occurrence in crystalline rocks. These peculiar forms consist principally of Bronzite and Enstatite, which to the naked eye show an appearance graphically described by Kesselmayer twenty years ago.

Prof. Gümbel, of Munich, in a report made to the Royal Bavarian Academy of Sciences has described them, on the basis of Kesselmayer's book and his own researches, as follows:

"Longitudinal sections show columns and fibres, composed of small polyhedra, which in cross sections look like irregular polygons. These polygons often show a sort of radiating arrangement in their interior, issuing from what appears to be an ill-defined nucleus; this nucleus seems to have been changing its place gradually, for the radii show an irregularity such as would be produced by such change of site. The fibres, for that is what these structures look like, are not of equal size throughout, but taper off into points and occasionally even send off branches. This is especially visible in cross-sections where one set is apparently replaced by others, these in turn by others, and so on. All the fibres consist, as has been stated, of a light centre, and a dark enclosing substance."

This description was given in 1878, and it certainly reads like what Hahn has proved it to be: *fossil organisms!*

This successful amateur, for such he was before he succeeded in gaining his present reputation by his participation in the debate on the "*Eozöon canadense*," and then resigned his government position to pursue this peculiar line of research at his leisure—this "Gerichts-Referendarius, a D." has by an ingenious application of the comparatively new method of making transparent sections of these meteorites accomplished results of which many a specialist might be proud. In order to exclude the error to which human vision and draughtsmanship might be liable, he has prepared photographic reproductions of his specimens, and on 32 excellent plates he presents the scientific world with 142 of these highly interesting preparations. Most of the fossil structures thus revealed belong to the animal world, indeed, Hahn himself professes that he is unable to find evidences of vegetable organisms; these, however, since the appearance of his work in February, have been recognized by Prof. Karsten, of Schaffhausen, Switzerland, in sections prepared by him from a portion of the very meteorite in his possession which has furnished a considerable number of Hahn's specimens. Two of these Prof. Karsten has drawn, and the cuts are published in an exhaustive paper on Hahn's book, together with his own observations and those of others on this very subject in the German Journal "Die Natur," edited by Mr. Karl Mueller, of Halle, Prussia.

As to the genuineness of Dr. Hahn's discovery there can be no possible doubt, and it has been generally admitted—reluctantly by some, it is true—that these "Chondrites" consist almost exclusively of fossil organisms. Dr. D. F. Weinland, a member of the Academy of Sciences, of Philadelphia, where he formerly resided, has also published a review of Hahn's book in the "Ausland," edited by Friedrich Von Hellwald, of Tübingen, Wurtemberg, in which he states that by the kindness of the author he has had the opportunity of examining these specimens, and although this examination has not given exactly the same results in regard to the determination of the particular kind of organism, he cheerfully admits that they are organisms, and this fact will not be doubted by any one who scans the plates published by Dr. Hahn.

In a postscript to this review, Dr. Weinland informs the reader that the author has entrusted to him the difficult task of classifying all the fossil organisms in more than three hundred of his specimens—of which Hahn has prepared over six hundred—and Dr. Weinland who is a competent naturalist, gives a few of his preliminary results. He compares the material which these sections display to the detritus of which the youngest coral lime and sandstone (coralline crag) consist such as is found on the shores of the Mexican Gulf. He furthermore states that complete forms are rarely found, but that the material is sufficiently abundant to construct many complete species, in the manner usually applied to fossil remains.

The number of the various species of polypi, crinoids, spongiæ and algiæ which are united by a silicious material, Dr. Weinland estimates after a cursory examination at about fifty.

One of the corals is set down by various observers as

* Hahn, O. Die Meteorite (Chondrite) and ihre Organismen; Laupp; *Tübingen*, 1881, with 32 photolithographic plates.

resembling to the *Favosites Goldfussi* * from the Silurian *Grauwacke*,' another is compared to the *Calamopora Naumanni* from the same strata.

The structure of these corals is excellently preserved; the columnar structure, the stomata, the rays in the cells, indicating the partitions between the columns in cross-sections, in short, all the various parts can be perfectly well demonstrated.

Of Spongiæ Dr. Weinland has already determined three different genera. Of a peculiar bluish-colored sponge he says he could draw a perfect picture, so numerous are the various longitudinal and cross-sections in which it occurs, it would be as easy as it would be to draw it from a living sponge.

Algæ have also been recognized as forming part of this intricate network of fossils. Dr. Weinland has determined several as belonging to the *Cocconeïs*, while Prof. Karsten describes others belonging to the genera, *Leptothrix*, *Leptomitus* and *Hysterophyma*. (The latter gentleman reminds the reader of the fact, that Reinsch has lately demonstrated the existence of these and other Algæ in coal, some of his specimens containing as much as twenty per cent of such organisms.

But what is the most interesting feature of all the organisms thus ingeniously and unexpectedly brought to light in meteorites is their Lilliputian size. The coral-tree, above referred to as a *Favosites*, presents itself to the naked eye as a white spot on the section, not larger than a pin's head. Its greatest diameter measures nine-tenths of a millimeter, and the single cells not more than about five one-hundreths of a millimeter. All the other organisms detected show the same pygmean proportions, the spiculae of sponges. for instance, being absolutely indefinable to the naked eye.

The origin and formation of these celestial fossils could not possibly have been different from what we know it to be with our terrestrial specimens. They tell us of a planet, on which aquatic life was sufficiently developed to produce them and to preserve them after death by a process of infiltration with silicious material, which dissolved the lime of which these structures must have consisted as far as their inorganic constituents are concerned, and supplanted it by the various kinds of silicious minerals, filling up also the interstices and openings which had formerly contained organic substance. This planet, therefore, must have had a comparatively long period of existence; it must have had an atmosphere and its surface must in whole, or in part, have been covered by water. What the cause has been of its destruction and its utter disintegration we are, certainly, unable to tell; but the meteoric stones which formed part of it have happily crossed the orbit of our planet and thus enabled us to divine its history, at least in part.

In connection with this subject, it may not be amiss to give a short synopsis of the history of our knowledge of organic constituents in meteoric stones.

The first to detect the existence of organic substance in meteorites was the great Woehler. In the meteorite which fell on April 17th, 1857, near Kaba in Hungaria, he found unmistakable traces—while analyzing it—of a combination of Carbon and Hydrogen. Then the fact was remembered that on Oct. 13th, 1835, a fire ball had exploded in the neighborhood of Bokkeveld, Cape Colony, scattering a great number of soft, black stones over the fields, weighing, as far as could be judged, several hundred pounds. These stones emitted a strong ammoniacal smell and were found to be impregnated with water and bitumen. Woehler obtained one of these meteoric stones and found that it contained, besides one and two-thirds per cent of carbon, a quarter of one per cent of organic matter proper.

* A drawing of this fossil coral is given by DANA in his *Textbook on Geology*, on page 111. (Ed. 1868.)

Referring to this discovery, Friedrich Mohr* wrote, sixteen years ago:

"This is sufficient proof that there was present in this meteorite a carbo-hydrate similar to our ozocerite, idrialite, seberrerite, mineral wax, etc. According to our terrestrial experience we must therefore conclude that on the planet of which they formed part, there must have existed organisms, at least plants, which are the real cause of the many deoxidized combinations which we find in meteorites. The existence of plants would evidently condition the presence of free oxygen, which does not speak against the presence of these products of deoxidation, since the plants themselves require oxygen for completing their cycle, in so far as they are ultimately (by decomposition), re-transformed into carbonic acid, without which condition a long, unbroken chain of vegetable life would be inconceivable. But the water must be liquid in order to act, and this implies that this planet must have had a certain size to enable it to be sufficiently warmed by the sun. The small meteorites, as they come to us, must in spite of their being exposed to the sun's rays, have the temperature of cosmic space, since they are, just as are high mountain peaks, too insignificant to become heated by insolation alone. Only an enlargement of size enables a celestial body to develop heat enough to produce a warm atmosphere. This circumstance supports strongly the view, that meteorites have not been formed independently, but that they have formed part of a larger body, on which processes, similar to those obtained on our planet, have been going on."

This is certainly interesting reading to-day, knowing as we do that the planet in question has also been an abode of animal life.

Other meteorites containing organic substances have been recorded since then. Thus at Orgueil, France, 1864; at Knyahinya, Hungary, June 9, 1866. This phenomenon is the most important since very many of the most convincing specimens, prepared by Dr. Hahn, have been obtained from a stone weighing 27 lbs., which formed part of the 600 lbs. that fell in that particular locality on that day.

The most curious meteoric shower, however, was observed in 1870 in Sweden. Black pieces, consisting almost exclusively of mold, descended on a snow-field, and could thus be easily collected. Mold is always the result of some organic process, and living particles play the efficient part in its production.

Since bacteria are known to be able to withstand a temperature of —100° C, without losing vitality, the Thompson–Richter hypothesis of the propagation of life through the universe in this manner becomes almost a tangible reality. But, we forbear! The perspective opened by Dr. Hahn's discovery is too grand to be discussed in the brief space, allowed this notice. It is only to be regretted that the favored discoverer seems inclined to tamper with his good fortune in so far as he draws conclusions from his newly established facts which few will be willing to admit. He thinks it possible that the formation of living matter may have begun in cosmic space, that cells were developed from Chaos and a certain vegetative process could have gone on in the gaseous and liquid masses supposed to have been the formative matter of our sola-system, etc. Prof. Karsten is even of the opinion that meteorites might form in the upper strata of our atmosphere. As proof he adduces the few recorded showers of polygonal hail-stones and especially the two cases of ice-meteorites. On May 28th, 1802, there fell near Puztemischel, Hungary, a block of ice weighing 1200lbs. and Hayne in his "Tracts historical and statistical on India" reports the fact that near Seringapatam a mass of ice fell from heaven, as large as an elephant, which took, in spite of the tremendous heat, over two days to melt.

**Geschichte der Erde*, 1866, p. 500.

If we should be asked our opinion as to what the origin of these ice-meteorites may have been, we should be inclined to answer that they are very probably a small part of the collections of water (oceans?) which, we know, must have existed on the disintegrated planet to which our stone and iron-meteorites once belonged.

The various theories which have been held to explain certain well-known facts about meteoric bodies, notably Schiaparelli's ingenious hypothesis connecting comets with meteorites, the fact that most comets give a spectrum, closely resembling that of carbon, and many others will have to be revised in the light of this discovery, and it may be safely claimed that Dr. Hahn's book will prove to be one of the most important contributions to natural science of the present time.

ASTRONOMY.

Prof. Mark W. Harrington, of Ann Arbor Observatory, announces, in a private letter to the editor, the variability of star D. M. + 0° .2910, the position of which for 1855.0 is

A. R. 12h. 6m. 28.4s. Decl. + 0° 23.5′

It reached its minimum on May 22 or 23, when it was of the magnitude of D. M. + 0° .2914, which is given by Argelander as 8.7. It is now increasing in brightness at the rate of a tenth of a magnitude a day. The star, in the same right ascension and in 15′ south of the variable (D. M. + 0° .2911), is of a fine orange color, and should be put in the list of red stars.

Observers desiring information, charts, or comparison stars, for use in observing the variable, will be cheerfully assisted by Prof. Harrington or the editor.

M. Eugene Block, of the Observatory of Odessa, Russia, has communicated the following observations and elements of Comet (a), 1881, Swift:

Odessa M. T.	App. α.	App. δ.
1881 *d. h. m. s.*	*h. m. s.*	° ′ ″
May 4 14 50 15	0 15 26.53	+ 33 25 3.7
5 14 28 12	0 19 1.00	+ 32 24 36.7
7 14 36 2	0 26 35.05	+ 30 15 5.9

ELEMENTS.

$T = 1881$, May 20.8294.

$$\begin{aligned} & \quad\ ^\circ \quad\ ' \quad\ '' \\ \pi &= 299 \ \ 47 \ \ 53 \\ \Omega &= 123 \ \ 59 \ \ 25 \\ i &= \ \ 79 \ \ 33 \ \ \ 0 \\ \log.\, q &= 9.76570. \end{aligned}$$

The comparison with the middle place gives

Obs. $- c$, $\delta\lambda \cos.\beta = -27''$
$\delta\beta = +\ 3''$

Careful search has been made at Boston, at Cambridge by Mr. Wendell, at Clinton, N. Y., by Prof. Peters, and by others, for Barnard's Comet, but without success.

SCIENCE OBSERVER,
Special Circular No. 13.

BOSTON, *June* 2, 1881.

UNDERGROUND WIRES IN PARIS.—The Municipal Council of this city are contemplating adding to their funds by taxing wires placed in the sewers. The proposed tax will be 20fr. per kilometre up to 500, 30fr. from 500 to 1,000, 40fr. from 1,000 to 1,500, and so on, with an increase of 10fr. for each 500 kilometres. *L'Electricité* says that the number of kilometres of wire placed in the sewers being about 7,000, the Compagnie des Téléphones will have to pay something like 59,500fr. It adds that the company make no objection to this tax.

BOOKS RECEIVED.

SECOND REPORT OF THE UNITED STATES ENTOMOLOGICAL COMMISSION, for the years 1878 and 1879, relating to the Rocky Mountain Locust, and the Western Cricket, etc., with illustrations, Washington 1880.

This volume will be read with interest by naturalists, and the facts and statistics relating to the ravages of locusts, and the laws and characteristics governing their migrations are very complete.

The interesting chapter entitled "The Brain of the Locust" opens with these lines. "In order to appreciate the habits, migratory, reproductive, etc., of the locust, and to learn something of its general intelligence as an insect, and as compared with other insects, it is necessary for us to study with a good deal of care the organ of the locust's *mind, i. e.*, its nervous system, comprising its nervous centres and the nerves arising from them. The present chapter will be devoted to a study of the brain."

It may be confidently affirmed that with methods far subtler and reasoning much more profound, than any employed by the author of this chapter, we shall always fail to find in the structure of the nervous system any explanation of the migratory and reproductive or of any other habits *as habits* in any animal. A large wing-ganglion means a flying insect—of course, a large optic ganglion means that vision is a powerful sense in the animal in which it is found; an atropic olfactory bulb, in man the monkeys and seals, means that the sense of smell does not play so important a role in these animals as in the fox, dog, lion, camel and opossum, where the bulb is large. The preponderance of the brachial enlargement of the cord in the mole and bat is related to the preponderance of the anterior extremities over the posterior in these animals, but it no more serves to explain the difference in psychical habits existing between the two, nay it does so to a less degree even than the external structure. There are species of locusts which are not migratory and a study of their brains should be made if Mr. Packard wishes to draw inferences as to habits from the cerebral structure; in other words, if he would trace out the line of demarcation between a "migratory" and a "non-migratory" brain.

We believe that the clause in question has been inserted with the purpose of indicating that there *is* a connection between the chapter it opens and the general purposes of the Report. If so, if it was the writer's object to lead the lay mind to look upon his paper as pointing out methods by which, through a careful pursuit of the logical lines and the ratiocination passing through the cells or nerve-tracts of the locust's nervous system, we should in course of time be enabled to overreach and anticipate him by our superior reasoning power, in a manner comparable to that followed by a detective shadowing a forger, we can only say that it might have been omitted. Science needs no apology and the excellent plates accompanying this part of the Report alone justify the expense incurred by Government in getting them up.

We consider it unfortunate that in a chapter not likely to be perused by the lay reader at all, so much matter of a semi-popular character should have been included. It is the attempt to popularize the distinction between the brain of insects and of vertebrates (p. 224) that has led Mr. Packard to the commission of actual errors. Thus speaking of the nervous system of vertebrates, he says: "The gray matter is situated in the centre and consists largely of nerve or so-called 'ganglion cells,' while the external white matter of the brain or cord is composed of a mass of nerve fibres." This is correct only as applying to the very lowest vertebrates; in man, the mammalia and reptilia, the gray matter is more or less near the surface, in some centers altogether *cortical*, while the white matter is internal. Mr. Packard adds, as another

discrimination: "moreover the entire brain of an insect is white, as are all the ganglia."

On page 226, he says that the outer part of the brain is made up of a "slightly darker, usually pale grayish, white portion"—, where the tissue consists of small ganglion cells, it is naturally . . . . rather darker than in those regions where the tissue consists of the more loosely disposed, large ganglion cells."

So that we have a fundamental contradiction in reference to an alleged fundamental distinction, quite aside from the notorious fact that in the lowest vertebrates the nervous system is as "white" as in insects, and that the convoluted "mushroom" body or "cerebrum" of the ant contains sharply demarcated gray and white substances.

The chapter is accompanied, as stated, by plates of great value, most of these being fac similes of sections prepared by Mr. Norman J. Mason. On the whole, nothing new is added to our knowledge of the adult insectean brain in general, or the locust's in particular, that has not been carefully reported by Floegel, Newton and Michels. But through the great patience and skill of Mr. Mason, Professor Packard has been enabled to study sections from the embryo brain, a subject not yet worked up, owing to the difficulty of preparing the specimens. The most important results obtained is that the nerve-fibres develop from an originally finely granular substance, thus confirming the observations of Schmidt and Hensen for the mammalian embryo.

In view of the loudly trumpeted theory recently revived by Dr. J. J. Mason, after having repeatedly received the *coup de grace* at the hands of Stieda, Meynert and others, that large cells are motor, it is interesting to note that those of the optic ganglion in the locust are among the largest cells in its nervous system. R. C. S.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of "SCIENCE."

Limax maximas L. A specimen of this slug was brought me May 16. It came through a faucet connected with the water works. Being an introduced species and not frequently found, this fact may be of interest.

Polygala pancifolia, wild. Specimens with pure white flowers have been sent from Lunenburg, Mass., two years in succession. J. H. PILLSBURY.

SPRINGFIELD, *May* 27, 1881.

SPECTRUM ANALYSIS.

At a meeting of the Royal Astronomical Society held on the 13th of May, Mr. Norman Lockyer asked permission to offer the following address. He said:

"The chemical constitution of the heavenly bodies is one that demands some attention from astronomers. Twenty years ago the observations of Kirchoff and Stokes enabled us to get some glimpses into the chemical constitution of the sun. Kirchoff's view was that substances with which we are acquainted exist in the atmosphere of the sun, and that their presence was demonstrated by an exact matching both with respect to wave-length and intensity of the lines of certain chemical elements. Before his time Frauenhofer had noted the coincidence of the bright yellow line of sodium with the D line in the solar spectrum, but Kirchoff showed that also in the case of iron, magnesium, cobalt and several other substances there were coincidences between lines, which went to show that what was true with respect to sodium was true with respect to these other bodies. Nine years ago, we had not merely the opportunity of comparing these bright lines in the spectrum of the sun's atmosphere, as revealed to Frauenhofer, but we had the opportunity of studying the spectra obtained from very small portions of the sun's atmosphere, in regions where we should expect an exceedingly high temperature—namely, in the regions of spots and in the regions of prominences. When we began to examine these spectra, we found that the lines were thickened, and the question appeared much less clear than it did before. Of 460 iron lines recorded by Kirchoff, only three were observed in the prominences, and these were not the lines that were seen thickened in spots; so that a great many fresh questions were raised, and the idea of the decomposition of the iron by the high temperature was forced upon us. I wish to bring before you to night the results of some purely astronomical inquiries, lately undertaken by the Solar Physics Committee with respect to the behavior of the lines in the spectra of spots and prominences. We had before us the admirable work undertaken by Prof. Young in 1872, on the spectra of the prominences; but his observations only lasted for a month, and we felt that we wanted more facts, so what we have been doing at Kensington during the last two and a half years, has been to obtain and tabulate the spectra of a hundred sunspots, and these we have compared with the Italian observations of prominence lines. It was impossible to note and map down the behavior of all the lines in the spot spectra. The Committee, therefore, attempted something which was more modest, and contented themselves with observing twelve lines in the most easily visible part of the spectrum, between F and D (pinned to the blackboard was a diagram with the spectra observed placed one beneath the other, at the top were the iron lines of the Frauenhofer spectrum stated by Angstrom to be coincident with the bright lines of iron). The first point which strikes one on examining this diagram is the enormous number of iron lines, both in the solar spectrum and in the iron spectrum, as mapped by Angstrom, who used an electric arc of thirty or more Bunsen cells. They remind one of a great piano, only a few notes of which are played over and over again in the spot spectra, but always producing a different tune. If you examine the lines individually, you will find that every line has been seen with every other line. One is struck by the marvellous individuality, so to speak, of each. The lines do not go in battalions, or companies, or corporal's files, but in single units. The great importance of obtaining these observations is not so much for the observations themselves, as for the comparison they enable us to make with the observations of the lines in prominences, because the prominences are hotter than the spots. The spots are caused by down-currents where the solar atmosphere is brought down from cooler regions. They are opposed to prominences, which are ejections of heated matter from the interior of the sun. Here (pointing to the diagram) we have arranged the observations of prominences by Tacchini since 1872. What is the result? First of all, you will note a very great simplification; the brightest part of the sun has given the fewest lines. Next, there is not a single line common to the two series. In passing from the iron lines in the spots to the iron lines in the flames we pass from one spectrum to another, and the two spectra are as distinct from one another as the spectrum of magnesium is distinct from the spectrum of chlorine, or any other substance you please. These phenomena are the last we should expect. We can understand that a difference in the quantity of iron vapor present, might make a certain difference in the spectrum; but we are driven to something quite independent of any change corresponding to quantity. We see that as the temperature is increased the simplicity of the spectrum is increased; just as a chemist finds with regard to the substances which he has under his control, the function of temperature is to simplify. Why, then, if this is the result of working with increased temperature here, should not the simplification be due to the breaking

up of the iron into simpler constituents? Mr. Lockyer went on to state that the probability that the elements are so broken up by the intense heat of the lower regions of the solar atmosphere is increased by finding that many of the lines seen in the lower regions are common to more than one element. He did not believe that the bright lines seen at the beginning and end of totality which are frequently spoken of as belonging to the reversing layer correspond to the dark lines of the Frauenhofer spectrum. In witnessing another total eclipse he should concentrate his attention on two of the basic iron lines, and note their behavior at the instant of totality.

Mr. Ranyard said: It is some years since we have seen Mr. Lockyer at a meeting of the Society. I am glad to see him here again, not only for the sake of the very eloquent lecture which he has given us, but also because of the influence which a Society like this is likely to have on those who read papers before it. It gives an opportunity of criticising theories and of asking questions, which is no doubt beneficial to the person who brings the theories forward. Mr. Lockyer has referred to a theory, which he has very widely discussed, with regard to the non-elementary nature of the elements, and the evidence to be derived from solar observations, I understood him to say that he would expect a greater heat to give us a less complex spectrum.

Mr. Lockyer: I never said anything of the kind.

Mr. Ranyard; I was about to say that the reverse appears to be the case. I hope that Mr. Lockyer will afterwards take the opportunity of explaining what he means. The spectrum of the photosphere is very complicated as compared with the spectrum of sunspots and prominences. If any fact needs dwelling upon with respect to the sun, it is the number of lines which cannot be matched with terrestrial elements, and the complication of the spectrum increases as you proceed downwards to the sun's limb; that is, as you proceed from cooler to warmer regions. In the region of the Corona, very few lines have been observed; that may be, it is true, because of their faintness; but with the exception of the hydrogen lines, the lines seen in the spectrum of the Corona, which, of course, is much cooler than the region of the chromosphere, do not correspond to known lines of any terrestrial element. There is, of course, an enormous field for study here; but the fact which I want to point out, is that you do not get a simplified spectrum in the sun with greater heat, and if the facts which Mr. Lockyer has referred to with regard to the common lines in the spectra of different elements are to be relied upon, it will not follow that the common lines correspond to the similar parts of the two elements, and that the other lines correspond to mere overtones, given out with greater heat. But I should like to ask Mr. Lockyer whether he has taken note of the observations of Professor Young, who has examined these lines common to two or more elements in the solar spectrum with great dispersion, and has found that they nearly all break up into double lines or groups of lines. I think out of fifty-seven lines all but four were shown to be thus broken up, and there was some doubt about these four.

Mr. Christie said: Similar observations to those which Mr. Lockyer has described with regard to the spectra of Sunspots have been made at Greenwich, and without adopting his theory, I may say that our observations agree with those which have been made by Mr. Lockyer. We have not confined our attention merely to the iron-lines which are thickened in the spot spectrum. But we perfectly confirm what Mr. Lockyer says, namely that in the spectrum of one spot there is one group of iron-lines thickened, while in the spectrum of another spot, there will be an altogether different group affected.

TERRESTRIAL MAGNETISM.—The French Government are about to establish an observatory for terrestrial magnetism at Cape Horn.

A NEW DISCOVERY IN PHOTOGRAPHY.

At the last meeting of the Photographic Society of Great Britain, Mr. L. Warnerke described the discovery he has recently patented. The discovery he said consisted in the fact that a gelatine plate submitted to pyrogallic acid became insoluble in those parts acted upon by light, exactly in the same way as gelatine was acted upon by chromic salts, the insolubility being in propoition to the amount of light and the thickness of the gelatine. This property he proposed to utilize in various ways. The drawback in the ordinary gelatine process was that unless the exposure were very accurately timed there was considerable danger of over-exposure, and, as intensification was very difficult, pictures by the gelatine process were often interior to those by collodion. By the new process he was, however, able not only to intensify, but also to overcome the drawbacks arising from over-exposure. The latter he effected by using the emulsion on paper. He had found that no matter how much the paper was over-exposed the picture—provided the developer was restrained sufficiently—was not injured, while in the case of the emulsion on glass there was not only halation of the image, but a reversal also. The transfer of the image from paper on to the glass was a very easy matter. The paper was immersed in water and placed in contact with a glass plate. The superfluous moisture was removed by a squeegee, and the paper could then be stripped off, leaving the tissue on the glass. Hot water was then applied, which dissolved all the gelatine not acted on by light, together with the free bromide or soluble salts, and the image was left upon the glass in relief. Intensification he effected by mixing with the emulsion a coloring non-actinic matter, which was not affected by silver. Aniline colors he had found answered the purpose, and in that way special emulsion for special purposes could be prepared. That method of preparation he thought would be especially suitable for magic-lantern slides. He claimed for his discovery that by it relief could be obtained far more easily than by the ordinary bichromatised gelatine, and therefore it was especially suitable for the Woodburytype process. By mixing emery-powder with the emulsion it was rendered fit for engraving purposes, and by a combination with vitrified colors the image could be burnt in and so was adapted for enamels. In the ordinary methods of producing enamels from carbonised gelatine the latter, from the difficulty of burning it without the formation of bubbles, was a great source of trouble. By using a suitable emulsion, however, so little gelatine might be employed that this drawback was overcome. The process could also be adapted for collotype printing. In the course of his remarks, Mr. Warnerke demonstrated the removal of a gelatine picture produced by his method from paper on to glass, and showed that the mere immersion and washing in hot water fixed the picture by the dissolving of the gelatine unacted upon by light, which thus carried away the free bromide of silver. In conclusion, he stated that the sensitive paper could be used in the camera in lengths wound on rollers, and exhibited a camera which he had made for the purpose.

Captain Abney, after some remarks in reference to halation and reversal of the image, remarked that in the production of enamels by Mr. Warnerke's process there was some danger of the silver producing the well-known yellow colour which spoilt so many vitrified photographs. The discovery made by Mr. Warnerke was a most importan one, and in regard to Woodburytype, really opened up quite a new era. Mr. W. S. Bird endorsed Captain Abney's remarks as to the value of the process. To be able to produce gelatine negatives without the fear of the yellow stain was a great boon, and the only point was whether photographers would take the trouble and risk in the necessary transfers. As to its adaptability to Woodburytype, there could not be the slightest doubt. The great difficulty was to obtain the necessary relief, and he knew of a company which had recently gone to a great expense to fit up the necessary machinery, when Mr. Warnerke was able to give them what they wanted at a merely nominal cost.

Mr. T. Sebastion Davis also referred to the importance of the discovery, and suggested that by the use of the emulsion on paper a landscape might be photographed in which the clouds and the foreground might be rendered with equal truth, instead, as was too often the case, of the sky

being over-exposed. Mr. T. Bolas inquired whether Mr. Warnerke had tried adding bichromate of potash to his emulsion. The addition of bromide of silver in the case of a carbon print was supposed to increase its sensitiveness, but whether it did so he could not say. Mr. Warnerke in the course of his reply, said he had not found the yellow colour spoken of by Captain Abney, in the enamels which he had made. It was possible to eliminate all the silver by the use of ferric salts. With regard to Mr. Davis's suggestion, he was afraid he must throw cold water upon it, for he did not think it could be realized unless he used a developer for the clouds different from that used for the foreground. He had not tried bichromate of potash as mentioned by Mr. Bolas.

ESTIMATION OF FAT IN MILK.

The plan I adopt is as follows:—10 grms. of milk are evaporated in a platinum boat (of suitable construction), to near dryness (to complete dryness if you wish to determine the total solids) in the water-bath; the boat is now inserted into the extraction tube (which is plugged with a little cotton-wool and contains a stopper in the narrow part of the tube), and then connected to an upright Liebig's condenser. A small tarred flask is now fixed on to the end of the extraction tube (50 to 100 c. c. capacity) containing ether. The ether is evaporated by means of hot water, and when sufficiently condensed in the tube above, so as to completely cover the platinum boat, the stopper of the extraction tube is turned and the ether allowed to remain for about six hours or all night if convenient. All that now remains to be done is to cautiously open the stopper and allow the ether and oil to flow into the tarred flask; boil the ether repeatedly until extraction is complete. Disconnect the flask, evaporate the ether dry, and weigh the oil. The platinum boat may also be taken from the extraction tube, dried in water-bath, and weighed, which will give the solids not fat, then ignited and weighed, and we have the ash. If there is any doubt in the mind of the operator that the ether has not been able to penetrate the residue, after there have been several extractions made, the boat may be withdrawn from the extraction tube, the residue detached from its sides by means of a small platinum spatula, and the whole again returned to the extraction tube, and the operation of extraction repeated. When the extraction has been conducted as described, there is no fear of any fat being left undissolved in the residue. The following duplicate analyses are the results I have just obtained from a sample of milk I have reason to believe is genuine or unadulterated. The amount of milk operated upon was 10 grms. Specific gravity, 1027·3.

Total solids	10.2440	10.2448
Fat	1.9940	2.0001
Solids not fat	8.2500	8.2447
Ash	0.6940	0.6960

WILLIAM JOHNSON, F.I.C., F.C.S., &c.

THE ELECTRIC RAILWAY.

One of the novelties at the Crystal Palace, London, on Easter Monday, was the opening of an electrical railway, constructed by the Société Anonyme d'Electricité of Brussels, on the Siemens system. On the upper terrace of the Palace grounds, overlooking the charming scenery of Sydenham, a miniature circular line of railway, consisting of three lines of metals, has been laid down, surrounding one of the ornamental ponds, and a small wooden hut erected beside it as a passenger station. On this railway, which is about 300 metres in length, and has a gauge of about 50 centimetres, or 19 inches, between the outer rails, stands the electrical locomotive. Its length is about four feet; its breadth about a metre; its height about as much, and its weight some three-quarters of a ton. It is, in fact, a Siemens dynamo-electric machine, neatly boxed in, and mounted on a truck with four metal wheels, and provided with a break and alarm bell for its control by the man in charge. A stationary engine of about eight horse-power nominal, in a shed about thirty yards from the railway line, drives a stationary dynamo-electric machine, from which the electro-motive current is primarily obtained. Two wires are connected with this fixed dynamo-machine. By one of them the current flowing out is conveyed to the mid-rail of the railway, to which it is attached by an iron plate bolted on. The second or return wire is attached to the exterior rail of the railway. The mid-rail is supported upon wood blocks, and is thus in a certain degree insulated. Beneath the electrical locomotive a brush of iron wires sweeps the mid-rail, and the electrical current is thus taken up into the locomotive, where it passes through the mounted Siemens machine within it, the large bobbin of which is thereby caused to revolve, and the current passing away by the wheels of the truck to the exterior rails of the road, is conveyed back to the stationary dynamo-machine. As the current thus circulates, and the bobbin of the mounted machine revolves, it drives the four wheels of the truck as the locomotive moves on, hauling after it a load of nearly three tons with ease at the speed we have named.

NOTES.

INTERNAL DISCHARGES OF ELECTRIC CONDENSERS.—B. Villari.—The author's conclusions are that the heat evolved by the internal discharge may be neglected in case of feeble discharges; beyond certain limits it manifests itself and increases very rapidly with the discharges themselves; thus the first means to augment this internal heat is to make use of jars charged to a very high potential. The internal discharge is sensibly augmented if the exterior spark is produced between two small balls of 20 to 30 mm. in diameter; it decreases, on the contrary, by almost one-half if the spark is taken from a point and one of the balls. The inverse is the case for the heat produced by the external exciting spark. For a given charge the internal discharge increases if the inner coating of the jar is diminished.

RESEARCHES ON THE CHANGE OF STATE IN THE NEIGHBORHOOD OF THE CRITICAL POINT OF TEMPERATURE.—L. Cailletet and P. Hautefeuille—The authors remark that near the critical point there are witnessed for very slight variations of temperature, phenomena which have led Andrews to regard the gaseous and the liquid states as distant terms of one and the same state of matter, which may pass from one to the other by a continuous series of changes. It is impossible to know what is the state of the matter which gives rise to the moving and wavey striæ which displace each other above the mercury on operating in the vicinity of the critical point. A slow decrease of pressure often shows if a tube is filled with a liquid or a gas, for in the latter case the release gives rise to a general mist and to liquid drops; but this procedure furnishes no clue to the nature of these striæ. The authors have overcome this difficulty by coloring carbonic acid with the blue oil of galbanum. They have found that these undulating striæ dissolve the oil, and are consequently produced by liquefied carbonic acid. They conclude that matter does not pass by insensible degrees from the liquid to the gaseous state.

ON THE ACTION OF THE SELENIUM RADIOPHONE.—M. E. Mercadier observes that the sounds produced in the selenium receivers which he has studied result chiefly from the luminous radiations. The rays of the spectrum act from the limit of the blue, on the indigo side, as far as the extreme red, and even a little beyond the red. The indigo, violet, and ultra-violet rays are without perceptible action in the conditions under which he has experimented. The maximum effect is always produced in the yellow portion of the spectrum. Radiophones with glass tube-receivers containing air, in contact with a smoked surface, give a different result, the action being principally thermic.—*Comptes Rendus*.

LAW RELATING TO CABLES.—*L'Electricité* says that there is some idea of appointing a commission to inquire into the state of international law relating to submarine cables. The Minister for Foreign affairs in France, M. St. Hilaire, has stated that, in case the forthcoming Congress of Electricians should arrive at any decision on the subject, he will send a circular to the various Governments suggesting the holding of an international conference.

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AT the last meeting of the "*American Chemical Society*," Professor A. R. Leeds called attention to the reported adulteration of certain articles of food, and made special reference to the adulteration of sugar and syrups, with glucose.

The result of Dr. Leeds' examination of sugar shows, that it was of excellent quality and almost free from any adulteration, and that he was enabled, after investigations, to "contradict with equal decisiveness, the notion that table syrups are largely, almost universally, adulterated with glucose syrups."

As Dr. Leeds stated that one of the objects of his paper was to correct, what he calls, sensational reports of adulteration, and to place on record his own scientific work as evidence that adulterations to a large extent do not exist, it may be prudent to test the integrity of his work, by comparing it with results achieved by another chemist, having a high reputation as an analyst, who appears to have made investigations covering the same ground, as that instituted by Professor Leeds. We allude to Professor Harvey W. Wiley, whose paper on "Glucose and grape-sugar" appears at an opportune moment. According to Professor Wiley, the manufacture of glucose is conducted on a scale which will result in eleven million bushels of corn being used for that purpose during the present year, and as a bushel of corn will produce about 30 pounds of glucose, it would appear that over three hundred million pounds of glucose will be placed on the market during the year 1881, with every indication that the quantity will be doubled in 1882.

What becomes of all this glucose? Professor Wiley states that some of it is used for brewing beer, taking the place of malt; it is also given as a food for bees; "all soft candies, waxes and taffies, and a large proportion of stick-candies and caramels are made of glucose"; but "A VERY LARGE PROPORTION OF ALL THE GLUCOSE MADE IS USED FOR THE MANUFACTURE OF TABLE SYRUPS." * * * "When these syrups are sent into the shops, they are sold to consumers under such altisonant names as Maple Drip, Bon Ton, Upper Ten, Magnolia, Extra Choice, Golden Drip, White Loaf Drip," etc., etc. * * * "Dealers tell me that these syrups, by their cheapness and excellence, have driven all others out of the market. So much is this the case that it is no longer proper to call glucose the 'coming syrup.' It is the syrup which has already come."

"Grape sugar is used chiefly for the adulteration of other sugars. When it is reduced to fine powder; it can be mixed with cane sugar in any proportion, without altering its appearance. Since the grape-sugar costs less than half the price of cane sugar, this adulteration proves immensely profitable."

We do not propose to decide upon the issue thus raised by Professors Leeds and Wiley, but as both admit to have spoken after a full investigation, it is difficult to discover how results so different were arrived at. We believe that Professor Leeds reported correctly on the samples as he found them, but if Professor Wiley is correct, the former must have been very fortunate, or, perhaps, unfortunate, in the selection of his samples.

We are in receipt of a communication, stating that glucose sugar has now an immense sale, and that in the West, nine-tenths of the syrups on the market have but 5 to 15 per cent. of cane sugar.

Possibly in first-class stores in New York City, the sugars and syrups offered for sale are genuine, but it appears folly to shut our eyes to the immense use of glucose and grape-sugar for mixing purposes.

If Professor Leeds wishes his future communications on adulteration to be read with "vivid interest," or his reports to reach what he terms, "a commanding position in the literature of adulterations," he will offer some evidence that Professor Wiley is in error, while a few facts, showing the destination of the 500 tons of glucose and grape-sugar manufactured every day, will be timely and welcome.

We find that the first cost of glucose and grape-sugar is about one cent a pound, and that it is sold direct for three to four cents a pound. The manufacture therefore of glucose is a profitable industry, and one likely to be conducted with spirit and enterprise.

Is glucose wholesome? It may be early to answer this question, as some physicians are opposed to its use, but, as an article of food, it is now generally acknowledged to be a wholesome product, and if carefully and properly made, free from any deleterious substances. We therefore fail to find any reason why this thriving industry should not be conducted openly, and the product sold on its merits, thus escaping the odium which is cast on all counterfeit substances.

THE latest number of the journal of the *Royal Microscopical Journal* is largely occupied with papers discussing the question of angular aperture; that by Mr. Frank Crisp disposes of 60 pages, and another by Professor E. Abbe occupies 30 pages.

The editor of the *American Journal of Microscopy* proposes to offer the whole of Mr. Crisp's paper in a forthcoming number; those, therefore, who are interested in the subject can read it there in its integrity; in the meantime, the résumé to be found in another part of this issue, may be found useful. We may remind our readers that this discussion has continued for the last ten years, with the prospect of a settlement of the question as remote as ever.

Probably the Counsel for Cadet Whittaker, at the recent court-martial, was not aware of the magnitude of the question when he asked Professor Piper, of Chicago, "What is Angular Aperture?" Perhaps Mr. Park Benjamin, who is said to have prompted the question, will himself answer the question.

A WRITER in "*The Journal of Science*" defends the old system of "Weights and Measures" as against the metric system. He admits that in refined scientific investigations the metric system has advantages, but he is opposed to it for purposes of daily life and retail trade. He maintains that the nomenclature and the notation of the metric system requires reorganizing, with plain, simple and short names for its various grades, to be expressed in such a manner as to banish the decimal point beyond all ordinary transactions.

It appears to us that the metric system requires little apology for its defects, when, as the writer admits, the old system is complicated, and has a total want of unity in its weights and measures. In England, a peck of potatoes, apples, etc., is 20 lbs. in Lancashire, 21 lbs. in Sheffield, 14 lbs. in Huddersfield, and 16 lbs. in Halifax. A stone of anything is in some districts 14, and in others 16 lbs. A gill in the north of England is half, but in the south only a quarter, of a pint. Almost every county has its peculiar acre, and these examples might be multiplied.

A WRITER in "*The Astronomical Register*" draws attention to an error in the "*Memoir*" of Sir William Herschell, and repeated by Professor Holden, in "*Sir William Herschell, his Life and Works*," in styling Sir William a baronet.

We find Mr. James L. McCance is correct in making the inference that Sir William Herschell was created a knight, only. His son, Sir John Frederick William, was created a baronet in 1838.

We notice that Burke's Peerage affords little information on the subject, giving no date when the great astronomer was created a knight. Professor Holden mentions the year 1816 as the date of that event.

THE UNITY OF NATURE.

By THE DUKE OF ARGYLL.

VIII.

THE ORIGIN OF RELIGION CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

If any one were to ask what is the origin of hunger or what is the origin of thirst, the idleness of the question would be felt at once. And yet hunger and thirst have had an origin. But that origin cannot be separated from the origin of Organic Life, and the absurdity of the question lies in this—that in asking it, the possibility of making such a separation is assumed. It involves either the supposition, that there have been living creatures which had no need of food and drink, or else the supposition, that there have been living creatures which, having that need, were nevertheless destitute of any corresponding appetite. Both of these suppositions, although not in the abstract inconceivable, are so contrary to all that we know of the laws of Nature, that practically they are rejected as impossible. There always is, and there always must be, a close correspondence between the intimations of sensibility and the necessities of Life. Hunger is the witness in sensation to the law which demands for all living things a renewal of force from the assimilation of external matter. To theorize about its origin is to theorize about the origin of that law, and consequently about the origin of embodied Life. The Darwinian formula is not applicable here. Appetite cannot have arisen out of the accidents of variation. It must have been coeval with organization, of which it is a necessary part. The same principle applies to all elementary appetites and affections, whether they be the lower appetites of the body or the higher appetites of the mind. They exist because of the existence of certain facts and of certain laws to which they stand in a relation which is natural and necessary, because it is a relation which is reasonable and fitting. Really to understand how these appetites and affections arose, it would be necessary to understand how all the corresponding facts and laws came to be. But in many cases—indeed in most cases—any such understanding is impossible, because the facts and the laws to which every appetite corresponds are in their very nature ultimate. They are laws behind which, or beyond which, we cannot get. The only true explanation of the appetite lies in the simple recognition of the adjusted relations of which it forms a part; that is to say—in a recognition of the whole system of Nature as a reasonable system, and of this particular part of it as in harmony with the rest. Any attempted explanation of it which does not start with that recognition of the reasonableness of Nature must be futile. Any explanation which not only fails in this recognition, but assumes that the origin of anything can be interpreted without it, must be not only futile but erroneous.

Men have been very busy of late in speculating on the origin of Religion. In asking this question they generally make, often as it seems unconsciously, one or other of two assumptions. One is the assumption that there is no God, and that it must have taken a long time to invent Him. The other is that there is a God, but that men were born, or created, or developed, without any sense or feeling of His existence, and that the acquisition of such a sense must of necessity have been the work of time.

I do not now say that either of these assumptions is in itself inconceivable, any more than the supposition that at some former time there were creatures needing food and drink and yet having no appetites to inform them of the fact. But what I desire to point out is, first, that one or other of these assumptions is necessarily involved in most speculations on the subject, and secondly, that, to say the least, it is possible that neither of these assumptions may be true. Yet the method of inquiry to be pursued re-

specting the origin of Religion must be entirely different, according as we start from one or other of these assumptions, or as we reject them both. If we assume that there is no God, then the question how Mankind have come so widely to invent one or more of such imaginary Beings, is indeed a question well worthy of our utmost curiosity and research. But, on the other hand, if we start with the assumption that there is a God, or indeed if we assume no more than that there are Intelligences in the Universe superior to Man, and possessing some power greater than his own over the natural system in which he lives, then the method of inquiry into the origin of Religion is immensely simplified. Obviously the question how Man first came to recognize the existence of his Creator, if we suppose such a Being to exist, becomes in virtue of that supposition relegated to the same class as the question how he first came to recognize any other of the facts or truths which it concerns him most to know. Indeed from its very nature this truth is evidently one which might be more easily and more directly made known to him than many others. The existence of a Being from whom our own being has been derived involves, at least, the possibility of some communication direct or indirect. Yet the impossibility or the improbability of any such communication is another of the assumptions continually involved in current theories about the origin of Religion. But no such assumption can be reasonably made. The perceptions of the Human Mind are accessible to the intimations of external truth through many avenues of approach. In its very structure it is made to be responsive to some of these intimations by immediate apprehension. Man has that within him by which the Invisible can be seen, and the Inaudible can be heard, and the Intangible can be felt. Not as the result of any reasoning, but by the same power by which it sees and feels the postulates on which all reasoning rests, the Human Mind may from the very first have felt that it was in contact with a Mind which was the fountain of its own.

No argument can be conducted without some assumptions. But neither ought any argument to be conducted without a clear understanding what these assumptions are. Having now cleared up the assumptions which are usually made, we can proceed with greater confidence in the discussion of the great problem before us. The origin of particular systems of religious belief is, of course, a mere question of fact. A few of these systems belong to our own time; others have arisen late in the historic ages and in the full light of contemporary evidence. Some, again, are first recognized in the dawn of those ages, and their distinctive features can only be dimly traced through evidence which is scanty and obscure. Religion is the origin of all these systems of Belief, but no one of them represents the origin of Religion. None of them throw any other light on the origin of Religion than as all exhibiting the one essential element in which all Religion consists. And it would be well if men, before philosophizing on the origin of Religion, had a more accurate conception of what they mean by it. The definitions of Religion have been even worse than the definitions of Morality. Just as the attempt is made to account for morals apart from the sense of duty or of obligation in conduct, so is the attempt made to account for Religion apart from the sense of Mind or Will in Nature. The great effort seems to have been to try how the essential idea of Religion could be either most completely eliminated or else most effectually concealed. For example, a feeling of absolute dependence has been specified by Schleiermacher as the essence of Religion. Yet it is evident that a sense of absolute dependence may be urgent and oppressive without the slightest tincture of religious feeling. A man carried off in a flood, and clinging to a log of wood, may have, and must have, a painful sense of absolute dependence on the log. But no one would think of describing this sense as a feeling of Religion. A savage may have a feeling of absolute dependence on his bows and arrows, or on the implements of his chase; or disease may bring home to him a sense of his absolute dependence on the organs of his own body, which alone enable him to use his weapons with success. But it does not follow that the savage has any feeling of Religion towards his bow, or his arrow, or his net, or his fishspear, or even to his own legs and arms. Any plausibility, therefore, which may attach to the proposition which indentifies Religion with the mere sense of dependence, is due entirely to the fact that when men speak of the sense of dependence they suggest the idea of a particular kind of dependence—namely, dependence upon a Being or a Personality, and not dependence upon a thing. That is to say, that the plausibility of the definition is entirely due to an element of thought which it is specially framed to keep out of sight. A sense of absolute dependence on purely physical things does not necessarily contain any religious element whatever. But, on the other hand, a sense of dependence on Personal or Living Agencies, whether they are supposed to be supreme or only superior religions to our own, is a feeling which is essentially religious. But the element in that feeling which makes it religious is the element of belief in a Being or in Beings who havePower and Will. When we say of any man, or of any tribe of men, that they have no Religion, we mean that they have no belief in the existence of any such Being or Beings, or at least no such belief as to require any acknowledgment or any worship[1].

The practice of worship of some kind or another is so generally associated with Religion, that we do not usually think of it otherwise than as a necessary accompaniment. It is a natural accompaniment, for the simple reason that in the very act of thinking of Superhuman Beings the mind has an inevitable tendency to think of them as possessing not only an intellectual but a moral nature which has analogies with our own. It conceives of them as having dispositions and feelings as well as mere Intellect and Will. Complete indifference towards other creatures is not natural or usual in ourselves, nor can it be natural to attribute it to other Beings. In proportion therefore as we ascribe to the Superhuman Personalities, in whose existence we believe, the authorship or the rule over, or even a mere partnership in, the activities round us, in the same proportion is it natural to regard those Beings as capable of exercising some influence upon us, whether for evil or for good. This conception of them must lead to worship—that is to say, to the cherishing of some feeling and sentiment in regard to them, and to some methods of giving it expression. There is, therefore, no mystery whatever in the usual and all but universal association of worship of some kind with all conceptions of a religious nature.

It is to be remembered, however, that, as a matter of fact, the belief in the existence of a God, or more Gods than one, has come, though rarely, to be separated from the worship of them. Among speculative philosophers this separation may arise from theories about the Divine nature, which represent it as inaccessible to supplication, or as indifferent to the sentiments of men. Among savages it may arise from the evolution of decay. It may be nothing but "a sleep and a forgetting"—the result of the breaking up of ancient homes, and the consequent impossibility of continuing the practice of rites which had become inseparably associated with local usages. Among philosophers this divorce between the one essential element of Religion and the natural accompaniments of worship, is well exhibited in the Lucretian conception of the Olympian gods, as well as in the condition of mind of many men in our own day, who have not rejected the idea of a God, but who do not feel the need of addressing Him in the language either of prayer or praise. Of this same

[1]Professor Tiele's definition of Religion corresponds with that here given:—"The relation between Man and the Superhuman Powers in which he believes." ("Outlines of the History of the Ancient Religions," p. 2.)

divorce among savages we have an example in certain Australian tribes, who are said to have a theology so definite as to believe in the existence of one God, the omnipotent Creator of heaven and of earth, and yet to be absolutely destitute of any worship.[2] Both of these, however, are aberrant phenomena—conditions of mind which are anomalous, and in all probability essentially transitional. It has been shown in the preceding pages how impossible it is to regard Australian or any other savages of the present time as representing the probable condition of Primeval man. It needs no argument to prove that it is equally impossible to regard speculative philosophers of any school as representing the mind of the earliest progenitors of our race. But neither of savages nor of philosophers who believe in a God but do not pray to Him, would it be proper to say that they have no Religion. They may be on the way to having none, or they may be on the way to having more. But men who believe in the existence of any Personal or Living Agency in Nature superior to our own, are in possession of the one essential element of all Religion. This belief is almost universally associated with practices which are in the nature of worship—with sentiments of awe, or of reverence, or of fear.

It is not inconsistent with this definition to admit that sects or individuals, who have come to reject all definite theological conceptions and to deny the existence of a living God have nevertheless been able to retain feelings and sentiments which may justly claim to be called religious. In the first place, with many men of this kind. their denial of a God is not in reality a complete denial, What they deny is very often only some particular conception of the Godhead, which is involved, or which they think is involved, in the popular theology. They are repelled, perhaps, by the familiarity with which the least elevated of human passions are sometimes attributed to the Divine Being. Or they may be puzzled by the anomalies of Nature, and find it impossible to reconcile them intellectually with any definite conception of a Being who is both all-powerful and all-good. But in faltering under this difficulty, or under other difficulties of the same kind, and in denying the possibility of forming any clear or definite conception of the Godhead, they do not necessarily renounce other conceptions which, though vague and indefinite, are nevertheless sufficient to form the nucleus of a hazy atmosphere of religious feeling and emotion. Such men may or may not recognize the fact that these feelings and emotions have been inherited from ancestors whose beliefs were purely theological, and that it is in the highest degree doubtful how long these feelings can be retained as mere survivals. It is remarkable that such feelings are even now artificially propped up and supported by a system of investing abstract terms with all the elements of personality. When men who profess to have rejected the idea of a God declare, nevertheless, as Strauss has declared, that "the world is to them the workshop of the Rational and the Good," —when they explain that "that on which they feel themselves to be absolutely dependent is by no means a brute power, but that it is Order and Law, Reason and Goodness, to which they surrender themselves with loving confidence," we cannot be mistaken that the whole of this language, and the whole conceptions which underlie it, are language and conceptions appropriate to Agencies and Powers which are possessed of all the characteristics of Mind and Will. Order and Law are, indeed, in some minds associated with nothing except matter and material forces. But neither Reason nor Goodness can be thus dissociated from the idea of Personality. All other definitions which have been given of Religion will be found on analysis to borrow whatever strength they have from involving, either expressly or implicitly, this one conception. Morality, for example, becomes Religion in proportion as all duty and all obligation is regarded as resting on the sanctions of a Divine authority. In like manner, Knowledge may be identified with Religion in proportion as all knowledge is summed up and comprehended in the perfect knowledge of One who is All in All. Nor is there any real escape from this one primary and fundamental element of Religion in the attempt made by Comte to set up Man himself—Humanity—as the object of religious worship. It is the Human Mind and Will abstracted and personified that is the object of this worship. Accordingly, in the system of Comte, it is the language of Christian and even of Catholic adoration that is borrowed as the best and fullest expression of its aspirations and desires. Such an impersonation of the Human Mind and Will, considered as an aggregate of the past and of the future, and separated from the individual who is required to worship it, does contain the one element, or at least some faint outline and shadow of the one element, which has been here represented as essential to Religion—the element, namely, of some Power in Nature other than mere brute matter or mere physical force—which Power is thought of and conceived as invested with the higher attributes of the Human Personality.

Like methods of analysis are sufficient to detect the same element in other definitions of Religion, which are much more common. When, for example, it is said that "the Supernatural" or "the Infinite" are the objects of religious thought, the same fundamental conception is involved, and is more or less consciously intended. The first of these two abstract expressions, "the Supernatural," is avowedly an expression for the existence and the agency of superhuman Personalities. It is objectionable only in so far as it seems to imply that such agency is no part of "Nature." This is in one sense a mere question of definition. We may choose to look upon our own human agency as an agency which is outside of Nature. If we do so, then, of course, it is natural to think of the agency of other Beings as outside of Nature also. But, on the other hand, if we choose to understand by "Nature" the whole system of things in which we live and of which we form a part, then the belief in the agency of other Beings of greater power does not necessarily involve any belief whatever that they are outside of that system. On the contrary, the belief in such an agency may be identified with all our conceptions of what that system, as a whole, is, and especially of its order and of its intelligibility. Whilst, therefore, "the Supernatural," as commonly understood, gives a true indication of the only real objects of religious thought, it complicates that indication by coupling the idea of Living Agencies above our own with a description of them which at the best is irrelevant, and is very apt to be misleading. The question of the existence of Living Beings superior to Man, and having more or less power over him and over his destinies, is quite a separate question from the relation in which those beings may stand to what is commonly but variously understood by "Nature."

The other phrase, now often used to express the objects of religious thought and feeling, "the Infinite," is a phrase open to objection of a very different kind. It is ambiguous, not merely as "the Supernatural" is ambiguous, by reason of its involving a separate and adventitious meaning besides the meaning which is prominent and essential; but it is ambiguous by reason of not necessarily containing at all the one meaning which is essential to Religion. "The Infinite" is a pure and bare abstraction, which may or may not include the one only object of religious consciousness and thought. An Infinite Being, if that be the meaning of "the Infinite," is indeed the highest and most perfect object of Religion. But an infinite space is no object of religious feeling. An infinite number of material units is no object of religious thought. Infinite time is no object of religious thought. On the other hand, infinite power not only

[2] "Hibbert Lectures," by Max Müller, 1878, pp. 16, 17.

may be, but must be, an object of religious contemplation in proportion as it is connected with the idea of Power in a living Will. Infinite goodness must be the object of religious thought and emotion, because in its very nature this conception involves that of a Personal Being. But if all this is what is intended by "the Infinite" then it would be best to say so plainly. The only use of the phrase, as the one selected to indicate the object of Religion, is that it may be understood in a sense that is kept out of sight. And the explanations which have been given of it are generally open to the same charge of studied ambiguity. "The Infinite" has been defined as that which transcends sense and reason,—that which cannot be comprehended or completely and wholly understood, although it may be apprehended or partially conceived.[3] And no doubt, if this definition be applied, as by implication it always is applied, to the power and to the resources, or to any other feature in the character of an Infinite Being, then it becomes a fair definition of the highest conceivable object of religious thought. But, again, if it be not so applied,—if it be understood as only applying to the impossibility under which we find ourselves of grasping anything which is limitless,—of counting an infinite number of units,—of traversing, even in thought, an infinite space,—of living out an infinite time,—then "the Infinite" does not contain the one essential element which constitutes Religion.

Similar objections apply to another abstract phrase, sometimes used as a definition of the object of religious feeling, namely, "the Invisible." Mere material things, which are either too large to be wholly seen, or too small to be seen at all, can never supply the one indispensable element of Religion. In so far, therefore, as invisibility applies to them only, it suggests nothing of a religious nature. But in so far as "the Invisible" means, and is intended to apply to, living Beings who are out of sight, to Personal Agencies which either have no bodily form, or who are thought of and conceived as separate from such form—in so far, of course, "the Invisible," like "the Infinite," does cover and include the conception without which there can be no Religion.

Definitions of meaning are more or less important in all discussions; but there are many questions in which they are by no means essential, because of the facility of which we refer the abstract words we may be using to the concrete things,—to the actual phenomena to which they are applied. When, for example, we speak of the religion of Mahomet, or of the religion of Confucius, or of the religion of Buddha, we do not need to define what we mean by the word "Religion," because in all of these cases the system of doctrine and the conceptions which constitute those religions are known, or are matters of historical evidence. But when we come to discuss the origin, not of any particular system of belief, but of Religion in the abstract, some clear and intelligible definition of the word Religion becomes absolutely essential, because in that discussion we are dealing with a question which is purely speculative. It is idle to enter upon that speculative discussion unless we have some definite understanding what we are speculating about. In the case of Religion we cannot keep our understanding of the word fresh and distinct by thinking of any well-known and admitted facts respecting the beginnings of belief. There are no such facts to go upon as regards the religion of Primeval Man. Those, indeed, who accept the narrative attributed to the inspired authority of the Jewish Lawgiver have no need to speculate. In that narrative the origin of Religion is identified with the origin of Man, and the Creator is represented as having had, in some form or another, direct communication with the creature He had made. But those who do not accept that narrative, or who, without rejecting it altogether, regard it as so full of metaphor that it gives us no satisfying explanation, and who assume that Religion has had an origin subsequent to the origin of the species, have absolutely nothing to rely upon in the nature of history. There is no contemporary evidence, nor is there any tradition which can be trusted. Primeval man has kept no journal of his own first religious emotions, any more than of his own first appearance in the world. We are therefore thrown back upon pure speculation — speculation indeed, which may find in the present, and in a comparatively recent past, some data for arriving at conclusions, more or less probable, on the conditions of a time which is out of sight. But among the very first of these data, if it be not indeed the one datum without which all others are useless, is a clear conception of the element which is common to all religions as they exist now, or as they can be traced back beyond the dawn of history into the dim twilight of tradition. Of this universal element in all religions "the Infinite" is no definition at all. It is itself much more vague and indefinite in meaning than the word which it professes to explain. And this is all the more needless, seeing that the common element in all religions, such as we know them now, is one of the greatest simplicity. It is the element of a belief in superhuman Beings—in Living Agencies, other and higher than our own.

It is astonishing how much the path of investigation is cleared before us the moment we have arrived at this definition of the belief which is fundamental to all religions. That belief is simply a belief in the existence of Beings of whom our own Being is the type, although it need not be the measure or the form. By the very terms of the definition the origin of this belief is and must be in ourselves. That is to say, the disposition to believe in the existence of such Beings arises out of the felt unity of our own nature with the whole system of things in which we live and of which we are a part. It is the simplest and most natural of all conceptions that the agency of which we are most conscious in ourselves is like the Agency which works in the world around us. Even supposing this conception to be groundless, and that, as some now maintain, a more scientific investigation of natural agencies abolishes the conception of design or purpose, or of personal Will being at all concerned therein,—even supposing this, it is not the less true that the transfer of conceptions founded on our own consciousness of agency and of power within us to the agencies and powers around us, is a natural, if it be not indeed a necessary conception. That it is a natural conception is proved by the fact that it has been, and still is so widely prevalent; as well as by the fact that what is called the purely scientific conception of natural agencies is a modern conception, and one which is confessedly of difficult attainment. So difficult indeed is it to expel from the mind the conception of personality in or behind the agencies of Nature, that it may fairly be questioned whether it has ever been effectually done. Verbal devices for keeping the idea out of sight are indeed very common; but even these are not very successful. I have elsewhere pointed out[4] that those naturalists and philosophers who are most opposed to all theological explanations or conceptions of natural forces do, nevertheless, habitually, in spite of themselves, have recourse to language which derives its whole form as well as its whole intelligibility, from those elements of meaning which refer to the familiar operations of our own Mind and Will. The very phrase "Natural Selection" is one which likens the operations of Nature to the operations of a mind exercising the power of choice. The whole meaning of the phrase is to indicate how Nature attains certain ends which are like "selection." And what "selection" is we know, because it is an operation familiar to ourselves. But the personal element of Will and of purpose lies

[3] Max Müller, "Hibbert Lectures," 1878.

[4] "Reign of Law," Chaps. I. and V.

even deeper than this in the scientific theory of Evolution. When we ourselves select, we may very often choose only among things ready made to our hands. But in the theory of Evolution, Nature is not merely represented as choosing among things ready made, but as at first making the things which are to be afterwards fitted for selection. Organs are represented as growing in certain forms and shapes "in order that" they may serve certain uses, and then as being "selected" by that use in order that they may be established and prevail. The same idea runs throughout all the detailed descriptions of growth and of development by which these processes are directed to useful and serviceable results. So long as in the mere description of phenomena men find themselves compelled to have recourse to language of this sort, they have not emancipated themselves from the natural tendency of all human thought to see the elements of our own personality in the energies and in the works of Nature. But whether the attempt at such emancipation be successful or not, the very effort which it requires is a proof of the natural servitude under which we lie. And if it be indeed a natural servitude, the difficulty of getting rid of it is explained. It is hard to kick against the pricks. There is no successful rebellion against the servitudes of Nature. The suggestions which come to us from the external world, and which are of such necessity that we cannot choose but hear them, have their origin in the whole constitution and course of things. To seek for any origin of them apart from the origin of our whole intellectual nature, and apart from the relations between that nature and the facts of the universe around us, is to seek for something which does not exist. We may choose to assume that there are no Intelligences in Nature superior to our own; but the fact remains that it is a part of our mental constitution to imagine otherwise. If, on the other hand, we assume that such Intelligences do exist, then the recognition of that existence, or the impression of it, is involved in no other difficulty than is involved in the origin of any other part of the furniture of our minds. What is the origin of Reason? The perception of logical necessity is the perception of a real relation between things; and this relation between things is represented by a corresponding relation between our conceptions of them. We can give no account of the origin of that perception unless we can give an account of the origin of Man, and of the whole system to which he stands related. What, again, is the origin of Imagination? It is the mental power by which we handle the elementary conceptions derived from our mental constitution in contact and in harmony with external things, and by which we combine these conceptions in an endless variety of forms. We can give no account of the origin of such a power or of such a habit. What is the origin of Wonder? In the lower animals a lower form of it exists in the shape of Curiosity, being little more than an impulse to seek for that which may be food, or to avoid that which may be danger. But in Man it is one of the most powerful and the most fruitful of all his mental characteristics. Of its origin we can give no other account than that there exists in Man an indefinite power of knowing, in contact with an equally indefinite number of things which are to him unknown. Between these two facts the connecting link is the wish to know. And, indeed, if the system of Nature were not a reasonable system, the power of knowing might exist in Man without any wish to use it. But the system of Nature, being what it is—a system which is the very embodiment of wisdom and knowledge—such a departure from unity is impossible. That unity consists in the universal and rational correspondence of all its essential facts. There would be no such correspondence between the powers of the human mind and the ideas which they are fitted to entertain, if these powers were not incited by an appetite of inquiry. Accordingly, the desire of knowledge is as much born with Man as the desire of food. The impression that there are things around him which he does not know or understand, but which he can know and understand by effort and inquiry, is so much part of Man's nature that Man would not be Man without it. Religion is but a part of this impression—or rather it is the sum and consummation of all the intimations from which this impression is derived. Among the things of which he has an impression as existing, and respecting which he desires to know more, are above all other things, Personalities or Agencies, or Beings having powers like, but superior to his own. This is Religion. In this impression is to be found the origin of all Theologies. But of its own origin we can give no account until we know the origin of Man.

I have dwelt upon this point of definition because those who discuss the origin of Religion seem very often to be wholly unconscious of various assumptions which are necessarily involved in the very question they propound. One of these assumptions clearly is that there was a time when Man existed without any feeling or impression that any Being or Beings superior to himself existed in Nature or behind it. The assumption is that the idea of the existence of such Beings is a matter of high and difficult attainment, to be reached only after some long process of evolution and development. Whereas the truth may very well be, and probably is, that there never was a time since Man became possessed of the mental constitution which separates him from the brutes, when he was destitute of some conception of the existence of living Agencies other than his own. Instead of being a difficult conception, it may very well turn out to be, on investigation, the very simplest of all conceptions. The real difficulty may lie not in entertaining it, but in getting rid of it, or in restraining its undue immanence and power. The reason of this difficulty is obvious. Of all the intuitive faculties which are peculiar to Man, that of self-consciousness is the most prominent. In virtue of that faculty or power, without any deliberate reasoning or logical process of any formal kind, Man must have been always familiar with the idea of energies which are themselves invisible, and only to be seen in their effects. His own loves and hates, his own gratitude and revenge, his own schemes and resolves, must have been familiar to him from the first as things in themselves invisible, and yet having power to determine the most opposite and the most decisive changes for good or evil in things in themselves invisible, and yet having power to determine the most opposite and the most decisive changes for good or evil in things which are visible and material. The idea of Personality, therefore, or of the efficiency of Mind and Will, never could have been to him inseparable from the attributes of visibility. It never could have been any difficulty with him to think of living Agencies other than his own, and yet without any form, or with forms concealed from sight. There is no need therefore to hunt farther afield for the origin of this conception than Man's own consciousness of himself. There is no need of going to the winds which are invisible, or to the heavenly bodies which are intangible, or to the sky, which is immeasurable. None of these, in virtue either of mere invisibility, or of mere intangibility, or of mere immeasurableness, could have suggested the idea which is fundamental in Religion. That idea was indeed supplied to Man from Nature; but it was from his own nature in communion with the nature of all things around him. To conceive of the energies that are outside of him as like the energies that he feels within him, is simply to think of the unknown in terms of the familiar and the known. To think thus can never have been to him any matter of difficult attainment. It must have been, in the very nature of things, the earliest, the simplest, and the most necessary of all conceptions.

The conclusion, then, to which we come from this analysis of Religion is that there is no reason to believe, but on the contrary many reasons to disbelieve, that there

ever was a time when man with his existing constitution, lived in contact with the forces and in the face of energies of Nature, and yet with no impression or belief that in those energies, or behind them, there were Living Agencies other than his own. And if man, ever since he became Man, had always some such impression or belief, then he always had a Religion, and the question of its origin cannot be separated from the origin of the species.

It is a part of the Unity of Nature that the clear perception of any one truth leads almost always to the perception of some other, which follows from or is connected with the first. And so it is in this case. The same analysis which establishes a necessary connection between the self-consciousness of Man and the one fundamental element of all religious emotion and belief, establishes an equally natural connection between another part of the same self-consciousness and certain tendencies in the development of Religion which we know to have been widely prevalent. For although in the operations of our own mind and spirit, with their strong and often violent emotions, we are familiar with a powerful agency which is in itself invisible, yet it is equally true that we are familiar with that agency as always working in and through a body. It is natural, therefore, when we think of Living Agencies in Nature other than our own, to think of them as having some form, or at least as having some abode. Seeing, however, and knowing the work of those Agencies to be work exhibiting power and resources so much greater than our own, there is obviously unlimited scope for the imagination in conceiving what that form and where that abode may be. Given, therefore, these two inevitable tendencies of the human mind—the tendency to believe in the existence of Personalities other than our own, and the tendency to think of them as living in some shape and in some place—we have a natural and sufficient explanation, not only of the existence of Religion, but of the thousand forms in which it has found expression in the world. For as Man since he became Man, in respect to the existing powers and apparatus of his mind, has never been without the consciousness of self, nor without some desire of interpreting the things around him in terms of his own thoughts, so neither has he been without the power of imagination. By virtue of it he re-combines into countless new forms not only the images of sense but his own instinctive interpretations of them. Obviously we have in this faculty the prolific source of an infinite variety of conceptions, which may be pure and simple or foul and unnatural, according to the elements supplied out of the moral and intellectual character of the minds which are imagining. Obviously, too, we have in this process an unlimited field for the development of good or evil germs. The work which in the last chapter I have shown to be the inevitable work of Reason when it starts from any datum which is false, must be, in religious conceptions above all others, a work of rapid and continuous evolution. The steps of natural consequence, when they are downward here, must be downwards along the steepest gradients. It must be so because the conceptions which men have formed respecting the Supreme Agencies in Nature are of necessity conceptions which give energy to all the springs of action. They touch the deepest roots of motive. In thought they open the most copious fountains of suggestion. In conduct they affect the supreme influence of Authority, and the next most powerful of all influences, the influence of Example. Whatever may have been false or wrong, therefore, from the first in any religious conception must inevitably tend to become worse and worse with time, and with the temptation under which men have lain to follow up the steps of evil consequence to their most extreme conclusions.

Armed with the certainties which thus arise out of the very nature of the conceptions we are dealing with when we inquire into the origin of Religion, we can now approach that question by consulting the only other sources of authentic information, which are, first, the facts which Religion presents among the existing generations of men, and, secondly, such facts as can be safely gathered from the records of the past.

On one main point which has been questioned respecting existing facts, the progress of inquiry seems to have established beyond any reasonable doubt that no race of men now exists so savage and degraded as to be, or to have been when discovered, wholly destitute of any conceptions of a religious nature. It is now well understood that all the cases in which the existence of such savages has been reported, are cases which break down upon more intimate knowledge and more scientific inquiry.

Such is the conclusion arrived at by a careful modern inquirer, Professor Tiele, who says: "The statement that there are nations or tribes which possess no religion, rests either on inaccurate observations or on a confusion of ideas. No tribe or nation has yet been met with destitute of belief in any higher Beings, and travelers who asserted their existence have been afterwards refuted by facts. It is legitimate, therefore, to call Religion, in its most general sense, an universal phenomenon of humanity."[5]

Although this conclusion on a matter of fact is satisfactory, it must be remembered that, even if it had been true that some savages do exist with no conception whatever of Living Beings higher than themselves, it would be no proof whatever that such was the primeval condition of Man. The arguments adduced in a former chapter, that the most degraded savagery of the present day is or may be the result of evolution working upon highly unfavorable conditions, are arguments which deprive such facts, even if they existed, of all value in support of the assumption that the lowest savagery was the condition of the first progenitors of our race. Degradation being a process which has certainly operated, and is now operating upon some races, and to some extent, it must always remain a question how far this process may go in paralyzing the activity of our higher powers or in setting them, as it were, to sleep. It is well, however, that we have no such problem to discuss. Whether any savages exist with absolutely no religious conceptions is, after all, a question of subordinate importance; because it is certain that, if they exist at all, they are a very extreme case and a very rare exception. It is notorious that, in the case of most savages and of all barbarians, not only have they some Religion, but their Religion is one of the very worst elements in their savagery or their barbarism.

Looking now to the facts presented by the existing Religions of the world, there is one of these facts which at once arrests attention, and that is the tendency of all Religions, whether savage or civilized, to connect the Personal Agencies who are feared or worshipped with some material object. The nature of that connection may not be always—it may not be even in any case—perfectly clear and definite. The rigorous analysis of our own thoughts upon such subjects is difficult, even to the most enlightened men. To rude and savage men it is impossible. There is no mystery, therefore, in the fact that the connection which exists between various material objects and the Beings who are worshipped in them or through them, is a connection which remains generally vague in the mind of the worshipper himself. Sometimes the material object is an embodiment; sometimes it is a symbol; often it may be only an abode. Nor is it wonderful that there should be a like variety in the particular objects which have come to be so regarded. Sometimes they are such material objects as the heavenly bodies. Sometimes they are natural productions of our own planet, such as particular trees, or particular animals, or particular things in themselves inanimate, such as springs, or streams, or

[5] "History of Religion," p. 6.

mountains. Sometimes they are manufactured articles, stones or blocks of wood cut into some shape which has a meaning either obvious or traditional.

The universality of this tendency to connect some material objects with religious worship, and the immense variety of modes in which this tendency has been manifested, is a fact which receives a full and adequate explanation in our natural disposition to conceive of all Personal Agencies as living in some form and in some place, or as having some other special connection with particular things in Nature. Nor is it difficult to understand how the embodiments, or the symbols, or the abodes, which may be imagined and devised by men, will vary according as their mental condition has been developed in a good or in a wrong direction. And as these imaginings and devices are never, as we see them now among savages, the work of any one generation of men, but are the accumulated inheritance of many generations, all existing systems of worship among them must be regarded as presumably very wide departures from the conceptions which were primeval. And this presumption gains additional force when we observe the distinction which exists between the fundamental conceptions of religious belief and the forms of worship which have come to be the expression and embodiment of these. In the Religion of the highest and best races, in Christianity itself, we know the wide difference which obtains between the theology of the Church and the popular superstitions which have been developed under it. These superstitions may be, and often are, of the grossest kind. They may be indeed, and in many cases are known to be, vestiges of Pagan worship which have survived all religious revolutions and reforms; but in other cases they are the natural and legitimate development of some erroneous belief accepted as part of the Christian creed. Here, as elsewhere, Reason working on false data has been, as under such conditions it must always be, the great agent in degradation and decay.

METEOROLOGICAL ELECTRICITY.

Ciel et Terre gives a description of a cyclone which passed over Japan on the night of the 3d or 4th of October, 1880. At Tokio a rapidity of 45 metres per second has been observed, but this had only a rapidity of 10 metres; its diameter was not very considerable, 240 kilometres. The fall of the barometer, though rapid, was far from being as prompt as that occurring eight days before on the coasts of the Island of Formosa, where a depression of 73 millimetres in 4 hours, or 18 millimetres per hour, was observed. These indicate that the old theory of whirlwinds is perfectly useless to account for meteorological phenomena.

THE APERTURE OF MICROSCOPE-OBJECTIVES.

The last number of the *Journal* of the Royal Microscopical Society is largely occupied with a discussion of this question by Prof. E. Abbe, of Jena, and Mr. Frank Crisp, one of the secretaries of the Society.

The subject appears to have been again brought up by a paper by Mr. G. Shadbolt (President of the Society in 1856), who claimed to have "demonstrated beyond dispute that no objective could have an aperture of any kind in excess of 180° angular in air." The grounds on which Mr. Shadbolt rested his demonstration are disposed of in detail in the papers now published; but with this aspect of the matter we do not propose to deal, confining ourselves to the more general consideration of the subject, apart from any controversial matter.

The proper definition of the aperture of a microscope-objective was, for a long time, as is well known, a very vexed one among microscopists. The astronomer has always a ready definition for the telescope, the aperture of which was simply estimated by the absolute diameter of the object-glass. No such absolute measure is, however, possible in the case of the microscope-objective, as the lenses of which it is composed vary in diameter within considerable limits, and the larger lens is by no means the larger aperture, as is readily seen by the comparison of the large lenses of the low powers with the small lenses of the high powers, which yet much exceed the former in aperture.

In consequence of this difficulty, the angle of the pencil, as it emanates from the object, and prior to its transmission through the objective to the image, came to be very generally considered as the proper measure of the aperture of the objective. This was at a time when dry or air objectives were generally known, immersion objectives not having been brought into ordinary use.

But even with air objectives the angle of the radiant pencil did not afford a true comparison, which could only be made by the *sines* of the angles; but when immersion objectives were originated—that is, objectives in which water or oil replaced the air in front of the objective—the use of the angles became very misleading, for now three angles might all have the same number of degrees and yet denote very different values, according as they are in air, water, or oil.

It therefore became necessary to find a substitute for the angles in the comparison of apertures; for although it was no doubt possible to bear in mind that 82° in air was less aperture than 82° in water, and the latter less than 82° in oil, yet the use of the same figures inevitably tended to produce confusion in the minds of microscopists—so much so that it was stoutly maintained by one party that the apertures in the three cases we have referred to were identical because the angles were the same.

A solution of the difficulty was discovered by Professor Abbe, who pointed out that the true definition of aperture (in its legitimate meaning of "opening") was obtained when we compared the diameter of the pencil emerging from the objective with the focal length of the objective.

It will be desirable to explain somewhat more in detail how this conclusion is arrived at—as given in Prof. Abbe's paper.

Taking in the first case a *single*-lens microscope, the number of rays admitted within one meridional plane of the lens evidently increases as the diameter of the lens (all other circumstances remaining the same), for in the microscope we have at the back of the lens the same circumstances as are in front in the case of the telescope. The larger or smaller number of emergent rays will, therefore, be properly measured by the clear diameter; and as no rays can *emerge* that have not first been *admitted*, this must also give the measure of the admitted rays.

Suppose now that the focal lengths of the lenses compared are not the same,—what then is the proper measure of the rays admitted?

If the two lenses have equal openings but different focal lengths, they transmit the same number of rays to equal areas of an image at a definite distance, because they would admit the same number if an object were substituted for the image—that is, if the lens were used as a telescope-objective. But as the focal lengths are different the amplification of the images is different also, and equal areas of these images correspond to different areas of the object from which the rays are collected. Therefore, the higher-power lens, with the same opening as the lower power, will admit a *greater* number of rays in all from the same object because it admits the *same* number as the latter from a *smaller* portion of the object. Thus if the focal lengths of the two lenses are as 2:1, and the first amplifies N diameters, the second will amplify 2 N with the same distance of the image, so that the rays which are collected *to* a given field of 1 mm. diameter of

the image are admitted *from* a field of $\frac{1}{N}$ mm. in the first case and of $\frac{1}{2N}$ mm. in the second. Inasmuch as the "opening" of the objective is estimated by the diameter (and not by the area) the higher-power lens admits *twice* as many rays as the lower power, because it admits the *same* number from a field of *half* the diameter, and in general the admission of rays with the same opening, but different powers, must be in the inverse ratio of the focal lengths.

In the case of the single lens, therefore, its aperture must be determined by *the ratio between the clear opening and the focal length,* in order to define the same thing as is denoted in the telescope by the *absolute* opening.

Dealing with a *compound objective,* the same considerations obviously apply, substituting, however, for the clear opening of the single lens, the diameter of the pencil at its emergence from the black lens of the objective —that is, its clear effective diameter.

All equally holds good, whether the medium in which the objective is placed is the same in the case of the two objectives or different, as an alteration of the medium makes no difference in the power.

Thus we arrive at the general proposition for all kinds of objectives. 1st. When the power is the same, the admission of rays varies with the diameter of the pencil at its emergence. 2nd. When the powers are different the *same admission* requires *different openings* in the proportion of the focal lengths, or, conversely, with the *same opening the admission is in inverse proportion* to the focal length—that is, the objective which has the wider pencil relatively to its focal length has the larger aperture.

Thus we see that, just as in the telescope, the absolute diameter of the object-glass defines the aperture, so in the microscope, the ratio between the utilized diameter of the back lens and the focal length of the objective defines its aperture.

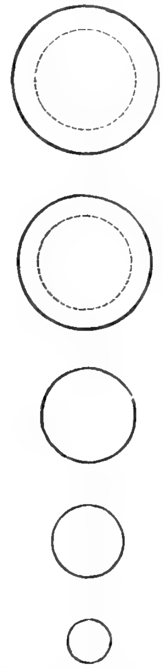
This definition is clearly a definition of aperture in its primary and only legitimate meaning as "opening"—that is, the capacity of the objective for admitting rays from the object and transmitting them to the image; and it at once solves the difficulty which has always been involved in the consideration of the apertures of immersion objectives.

So long as the angles were taken as the proper expression of aperture, it was difficult for those who were not well versed in optical matters to avoid regarding an angle of 180° in air as the maximum aperture that any objective could attain. Hence water-immersion objectives of 96° and oil-immersion objectives of 82° were looked upon as being of much *less* aperture than a dry objective of 180°, whilst, in fact, they are all *equal*—that is, they all transmit the same rays from the object to the image. Therefore, 180° in water and 180° in oil are unequal, and both are much larger apertures than the 180° which is the maximum that the air objective can transmit.

If we compare a series of dry and oil-immersion objectives, and, commencing with very small air-angles, progress up to 180° air-angle, then taking an oil-immersion of 82° and progressing again to 180° oil-angle, the ratio of opening to power progresses continually also, and attains its maximum, not in the case of the air-angle of 180° (when it is exactly equivalent to the oil-angle of 82°), but is greatest at the oil-angle of 180°.

If we assume the objectives to have the same power throughout, we get rid of one of the factors of the ratio, and we have only to compare the diameters of the emergent beams, and can represent their relations by diagrams. Our figure (which is taken from Mr. Crisp's paper) illustrates five cases of different apertures of ¼ in. objectives—viz., those of dry objectives of 60°, 97° and 180° air-angle, a water-immersion of 180° water-angle, and an oil-immersion of 180° oil-angle. The inner dotted circles in the two latter cases are of the same size as that corresponding to the 180° air angle.*

RELATIVE DIAMETERS OF THE (UTILIZED) BACK-LENSES OF VARIOUS DRY AND IMMERSION OBJECTIVES OF THE SAME POWER (¼) FROM AN AIR-ANGLE OF 60° TO AN OIL-ANGLE OF 180°.



Numerical Aperture 1.52 = 180° oil-angle.

Numerical Aperture 1.33 = 180° water-angle.

Numerical Aperture 1.00 = 180° air-angle. = 96° water-angle. = 82° oil-angle.

Numerical Aperture .75 = 97° air-angle.

Numerical Aperture .50 = 60° air-angle.

A dry objective of the full maximum air-angle of 180° is only able (whether the first surface is plane or concave) to utilise a diameter of back lens equal to twice the focal length, while an immersion lens of even only 100° (in glass) requires and utilises a *larger* diameter, *i. e.,* it is able to transmit more rays from the object to the image than *any* dry objective is capable of transmitting. Whenever the angle of an immersion lens exceeds twice the critical angle for the immersion-fluid, *i. e.,* 96° for water or 82° for oil, its aperture is in excess of that of a dry objective of 180°.

Having settled the principle, it was still necessary, however, to find a proper notation for comparing apertures. The astronomer can compare the apertures of his various telescopes by simply expressing them in inches; but this is obviously not available to the microscopist, who has to deal with the *ratio* of two varying quantities.

Prof. Abbe here again conferred a boon upon microscopists by his discovery (in 1873, independently confirmed by Prof. Helmholtz shortly afterwards) that a general relation existed between the pencil admitted into the front of the objective and that emerging from the back of the objective, so that the ratio of the semi-diameter of the emergent pencil to the focal length of the objective could be expressed by the sine of half the angle of aperture (u) multiplied by the refractive index of the medium (n) in front of the objective, or n sin. u (n being 1.0 for air, 1.33 for water, and 1.5 for oil or balsam).

When, then, the values in any given cases of the expression n sin. u (which is known as the "numerical aperture") has been ascertained, the objectives are instantly compared as regards their aperture, and, more-

* The explanation of the mistaken supposition that the emergent beam is wider in the case of the immersion objectives because the immersion-fluid abolishes the refractive action of the first plane surface of the objective (which, in air, reduces all pencils to 80° within the glass), belongs rather to the controversial branch of the matter. It is, however, fully dealt with in the papers referred to.

over, as $180°$ in air is equal to 1.0 (since $n = 1.0$, and the sine of half $180°$ or $90° = 1.0$), we see with equal readiness whether the aperture of the objective is smaller or larger than that corresponding to $180°$ in air.

Thus, suppose we desire to compare the relative apertures of three objectives, one a dry objective, the second a water-immersion, and the third an oil-immersion. These would be compared on the angular aperture view as, say, $74°$ air-angle, and $118°$ balsam-angle; so that a calculation must be worked out to arrive at a due appreciation of the actual relation between them. Applying, however, "numerical" aperture, which gives .60 for the dry objective, .90 for the water-immersion, and 1.30 for the oil-immersion, their relative apertures are immediately appreciated, and it is seen, for instance, that the aperture of the water-immersion is somewhat less than that of a dry objective of $180°$, and that the aperture of the oil-immersion exceeds that of the latter by 30 .

When these considerations have been appreciated, the advantage possessed by immersion in comparison with dry objectives is no longer obscured. Instead of this advantage consisting merely in increased working distance or absence of correction-collar, it is seen that a wide-angled immersion objective has a larger aperture than a dry objective of the maximum angle of $180°$; so that for any of the purposes for which aperture is desired, an immersion must necessarily be preferred to a dry objective.

The task of making an abstract of these papers was not a light one and we are indebted to the *English Mechanics* for the above résumé.

BOOKS RECEIVED.

Discovery of the Preglacial Outlet of the Basin of Lake Erie into that of Lake Ontario; with notes on the Origin of our Lower Great Lakes. By Prof. J. W. Spencer, B. A. Sc., Ph. D., F. G. S., Kings College, Windsor, N. S. 1881.

As one new branch of knowledge is raised to a science, there still seems to be some other rising to importance. For a long time the explanation of the Physical Features of America has been handed over to the rival Glacier and Iceberg theories, and though much good work has resulted, yet an almost unlimited amount of nonsense has been written, especially by the extreme or ultra-glacial school. During all these years comparatively little attention has been given to the subject of the river geology, more than that many buried channels have been recorded with but few attempts at the reduction of the abstract facts to a branch of Science. There has, however, been a very great difficulty, owing to the Preglacial valleys often being entirely obscured, or, if apparent, an absence of the knowledge of their depths has prevented generalization. In most of the cases recorded, the buried channels have not had courses greatly differing from those of modern times. It has been known for some time that the waters of most of the great lakes had southern outlets when at higher levels, and even to-day the drainage of Chicago passes to the Mississippi. It has been frequently suggested that Lake Ontario emptied by the Mohawk into the Hudson. This, however, was not the case. We are then compelled to place General G. K. Warren as the father of Fluviatile Geology, for he discovered that the Red River of the North (with Lake Winnipeg, the Saskatchewan, and other great rivers of the North West territories of Canada, as tributaries) discharged by the Minnesota river into the Mississippi, and thus produced a river to which no modern water is comparable. On further investigation Gen. Warren's views are found to require some modification, yet this does not detract from the position which may be fairly assigned to him. Dr. Newbury's observations in Ohio have also thrown much additional light on the subject, but a much more important work has been accomplished by Mr. J. F. Carll, of Pennsylvania, when from a careful study of the levels and borings for oil in that State, he discovered that the Upper Alleghany and several other rivers now flowing into the Ohio, formerly emptied into Lake Erie (or its basin).

But the most important contribution on the subject of Fluviatile Geology that has been made is the recent paper of the above title, by Professor Spencer, now of Kings College, Nova Scotia, but formerly residing in the lake region, in the Province of Ontario. The paper of the above title was read before the American Philosophical Society, of Philadelphia, and its publication will be found in the forthcoming proceedings of that Society. It is also being reprinted as an appendix to Report Q 4 of the Pennsylvania Survey, as shown by the maps which accompany the author's edition, of which we have just received a copy. The following is a synopsis of the principal points of the paper:

The Niagara escarpment bends abruptly at the western end of Lake Ontario, and has a height of about 500 feet above the lake. Through this limestone ridge the Dundas valley extends, and enters the extreme western end of the lake. At the narrowest portion of the valley the width is upwards of two miles, and the margins are those of the walls of a perfect *cañon*, 500 feet deep. But by boring near one of its margins, the buried channel is found to reach 227 feet below the surface of Lake Ontario, making a total depth of 743 feet, but with a computed depth in the central part of its course of not less than 1000 feet. The author first discovered that the ancient upper portion of the Grand River left its modern course south of Galt, and although a portion of the old bed is entirely obscured, yet by pursuing the course of the deep wells the ancient route can be traced through the drift to the western end of the Dundas *cañon* and Lake Ontario. In following up this subject Dr. Spencer discovered that the lower portion of the Grand River was formerly an outlet of the Erie basin, which discharged by a course from a point southward of Cayuga (Province of Ontario), and flowed to the westward of this town and entered the present valley, which is two miles wide and eighty feet deep, but underlaid deeply with drift. Westward of Seneca the ancient river left its modern course and passed into the Dundas valley. All these observations are elaborately worked out by levels, deep well borings, and deep ravines, with the one well in this course indicating a depth of 1000 feet of drift in the ancient valley, measuring from the limestone floor of the county.

The outlet of Lake Erie is directly opposite to that of the ancient Alleghany River.

Again, Dr. Spencer has made a study of the soundings of the lakes, and has discovered a long submerged escarpment extending along the southern side of Lake Ontario to near Oswego, at the foot of which the Ancient River from the Dundas Valley ran. The author has shown that an ancient, broad channel, extended from Lake Huron and entered Lake Erie between Port Stanley and Vienna, in the Canadian Province of Ontario. This channel has a marginal depth of 200 feet below Lake Erie, but with a probable depth sufficient to drain Lake Huron.

With regard to Lake Superior, Prof. Spencer shows that it formerly emptied into the northern end of Lake Michigan, and formed a river channel now represented by deep pot-holes. He brings forward some of the evidence showing that Lake Michigan emptied or was completely drained by the tributaries of the Mississippi, and that this lake was probably disconnected from Lake Huron. At the same time, he shows that Lake Superior (when it was at no higher level than at present) did not empty by the Green Bay and valley of the Fox and Wisconsin Rivers.

The author denies the hypothesis of the glacial origin of the Great Lakes, and brings forward strong evidence in support of his views. He correlates with his work and maps the buried channels discovered in Pennsylvania and

Ohio, the whole constituting the broadest study on the "Great River Age" that has been made. He considers the great lakes as largely valleys of subaërial erosion, traversed by the Grand River which he has worked out. The Ancient buried course of the Niagara, the author considers as interglacial, being formed and closed subsequent to the closing of the Dundas Valley. Of course, all this presupposes the action to have been going on when the continent was six hundred feet higher, and from the pot-holes in the New York Harbor, we know it to have had an altitude of at least 900 above the present elevation. To perfect the work there remains the discovery of the outlet of Lake Ontario, which was not by the Mohawk, as in its valley near Little Falls, it passes over hard rock. Yet Prof. Spencer insinuates, in this paper, that he is on the track of this discovery also, and that the study will be pursued during the coming summer. We wish the author every success, and if this ancient outlet be discovered, certainly he will have added much to his already most important discovery, and will fairly be considered as one of the founders of this new scientific development.

It must be further stated that the author does not consider all the ancient buried rivers now running southward, but formerly flowing northward, as having in any way been derived from glacier action, and more recently than the paper, which we are reviewing, a notice by him was read before the American Philosophical Society showing that the Monongahela flowed directly northward by the upper Ohio, Beaver, Mahoning and Grand Rivers of Ohio (the last three reversed in Preglacial times) to Lake Erie, thus adding another important tributary to the Erie Basin and further changing the physical features of the Continent.

This paper, which is the first preliminary notice of his work on the Great River Age, will do much to draw attention to the interesting subject which is destined to have an equal place with Glacial Geology, with the extreme views of which it will be found to conflict more or less.

ON M. C. FAURE'S SECONDARY BATTERY.

The researches of M. Gaston Planté on the polarization of voltameters led to his invention of the secondary cell, composed of two strips of lead immersed in acidulated water. These cells accumulate and, so to speak, store up the electricity passed into them from some outside generator. When the two electrodes are connected with any source of electricity the surfaces of the two strips of lead undergo certain modifications. Thus, the positive pole retains oxygen and becomes covered with a thin coating of peroxide of lead, while the negative pole becomes reduced to a clean metallic state.

Now, if the secondary cell is separated from the primary one, we have a veritable voltaic battery, for the symmetry of the poles is upset, and one is ready to give up oxygen and the other eager to receive it. When the poles are connected, an intense electric current is obtained, but it is of short duration. Such a cell, having half a square metre of surface, can store up enough electricity to keep a platinum wire 1 millim. in diameter and 8 centims. long, red-hot for ten minutes. M. Planté has succeeded in increasing the duration of the current by alternately charging and discharging the cell, so as alternately to form layers of reduced metal and peroxide of lead on the surface of the strip. It was seen that this cell would afford an excellent means for the conveyance of electricity from place to place, the great drawback, however, being that the storing capacity was not sufficient as compared with the weight and size of the cell. This difficulty has now been overcome by M. Faure: the cell as he has improved it is made in the following manner:

The two strips of lead are separately covered with minium or some other insoluble oxide of lead, then covered with an envelope of felt, firmly attached by rivets of lead. These two electrodes are then placed near each other in water acidulated with sulphuric acid, as in the Planté cell. The cell is then attached to a battery so as to allow a current of electricity to pass through it, and the minium is thereby reduced to metallic spongy lead on the negative pole, and oxidised to peroxide of lead on the positive pole; when the cell is discharged the reduced lead becomes oxidised, and the peroxide of lead is reduced until the cell becomes inert.

The improvement consists, as will be seen, in substituting for strips of lead masses of spongy lead; for, in the Planté cell, the action is restricted to the surface, while in Faure's modification the action is almost unlimited. A battery composed of Faure's cells, and weighing 150 lbs., is capable of storing up a quantity of electricity equivalent to one-horse power during one hour, and calculations based on facts on thermal chemistry shows that this weight could be greatly decreased. A battery of 24 cells, each weighing 14 lbs., will keep a strip of platinum ⅝ths of an inch wide, 1-32nd of an inch thick, and 9 feet 10 ins. long, red hot for a long time.

The loss resulting from the charging and discharging of this battery is not great: for example, if a certain quantity of energy is expended in charging the cells, 80 per cent of that energy can be reproduced by the electricity resulting from the discharge of the cells; moreover, the battery can be carried from one place to another without injury. A battery was lately charged in Paris, then taken to Brussels, where it was used the next day without recharging. The cost is also said to be very low. A quantity of electricity can be produced, stored, and delivered at any distance within 3 miles of the works for 1½d. Therefore these batteries may become useful in producing the electric light in private houses. A 1250 horse-power engine, working dynamo machines giving a continuous current, will in one hour produce 1000 horse-power of effective electricity, that is to say 80 per cent of the initial force. The cost of the machines, establishment, and construction will not be more than £40,000, and the quantity of coal burnt will be 2 lbs. per hour per effective horse-power, which will cost (say) ½d. The apparatus necessary to store up the force of 1000 horses for twenty-four hours will cost £48,000, and will weigh 1500 tons. This price and these weights may become much less after a time. The expense for wages and repairs will be less than ¼d. per hour per horse-power, which would be £24 per day, or £8800 a year; thus the total cost of one-horse-power for an hour stored up at the works is ¾d. Allowing that the carriage will cost as much as the production and storing, we have what is stated above, viz., that the total cost within 3 miles of the works is 1½d. per horse-power per hour. This quantity of electricity will produce a light, according to the amount of division, equivalent to from 5 to 30 gas burners, which is much cheaper than gas.—*Chemical News.*

MICROSCOPY.

We offer the following notes culled from the pages of the *Journal of the Royal Microscopic Society*:—

A singular species of Ncarus is described by A. D. Michael, found by him at Land's End, England. It belongs to the genus *Dermaleichus* (Koch) *Analges* (Nitsch) but does not fit into any of the five genera, or sub-genera, into which Robins has divided the group. The leading feature in this curious creature was that the male had the left leg of the second pair conspicuously larger than its fellow on the right side, had a totally different tarsus, and supported by a different and more powerful epimeral and sternal arrangement. This deformity makes this species entirely different to any other *Ncarus.*

Haustein has observed in the central cells of *chara*, chlorophyll-bodies containing starch which could not be regarded as the product of assimilation. C. Dehnecke has now investigated a number of similar instances, in which the starch contained within the chlorophyll-grains appears not to serve the purpose of immediate assimilation, but to be stored up as a reserve material.

A new stereoscopic eye piece has been arranged by Professor E. Abbe. The special feature of this instrument

is the ingenious arrangement whereby, by simply turning the caps with the diaphragms, orthoscopic or pseudoscopic effect can be produced instantly at pleasure. It is more particularly available for tubes of short length for which the Wenham prism is inapplicable.

Powell and Leland have completed a new 1-12 having two front lenses. The maximum numerical aperture is 1.43 (= 140° in crown glass of mean index 1.525), obtained by a front lens several degrees greater than a hemisphere, mounted on a plate of glass .003 inch in thickness, which is itself mounted in the usual metal work by the zone projecting beyond the circumference of the lens. With this front lens the focal distance from the exposed surface of the plate on which the lens is mounted is .007 inches. A second front, nearly a hemisphere, is mounted in the usual way by a burred edge of metal covering the extreme margin of the lens. This front gives a numerical aperture of 1.28 (= 115° in glass) and the focal distance is then 0.16 inch. The third front provides a numerical aperture of 1.0 (=82° in glass, as nearly as possible), and the working distance is then .024 inch – probably the greatest working distance hitherto obtained with a 1-12 of that aperture.

Dr. Reidel, an assistant to Professor Abbe, has found two new fluids suitable for homogeneous objectives. The first is a solution of Gum Damar dissolved in hot oil of cedar-wood. The oil which is obtained in Germany has a refractive index of 1.51 *only*, but by the Damar this can be raised to 1.54. If *carefully distilled* it becomes sufficiently pale and loses its stickiness. The other medium is a solution of *iodate of zinc* in Price's ordinary glycerine (n = 1.46). This salt is *very* soluble in glycerine, and a refractive index of 1.56 or more can be obtained, and therefore there is no difficulty in making a solution of 1.52 which is the standard index at 18° cent. Professor Abbe has furnished Mr. Zeiss with a new formula for homogeneous $\frac{1}{8}$, this having a numerical aperture of 1.40 and adjusted for the new fluids.

Mr. T. Charters White, R. M. S., calls for some re-agent suitable for mounting insects; carbolic acid renders the chitinous envelope transparent, but has the same effect on the internal organs also. Dr. Mathews also objected to carbolic acid, as it caused the abdomen of insects thus mounted to collapse. Those who have had some experience in making preparations for insect anatomy will perhaps have suggestions to make.

We lately called attention to infusoria found in cases of epidemic catarrh, called *Asthematos ciliaris*. Dr. Leidy doubted the character of this form and suggested its being a *ciliated epithelium*. Dr. Carter now maintains that it is correct to call it an infusorium, because by culture in mucus outside the body, they increase in number, and they are found in morbid secretions of the conjunctiva where no ciliated epithelia exist—moreover, those remedies only cure the disease which kill the *Asthematos*.

THE STEREORACHYS.

A new specimen of this gigantic and marvellous reptile from the permian schists of Igornay (Saône and Loire) has been presented by M. Gaudry, who gives an exceedingly interesting description of it. Among the results formulated by the learned paleontologist, one of the most striking is the continuity of life of the primary epoch to the secondary one. We are tending more and more to the idea of the slow modifications of terrestrial conditions, and are therefore receiving more and more from the gratuitous supposition of the revolutions of the globe.

To the Editor of "SCIENCE:"

DEAR SIR:—In the last number of your valuable periodical, at the close of a review of Professor Packard's work on the "Brain of the Locust," the writer states: "In view of the loudly trumpeted theory recently revived by Dr. J. J. Mason, after having repeatedly received the *coup de grâce* at the hands of Stieda, Meynert and others that large cells are motor, it is interesting to note that those of the optic ganglion in the locust are among the largest cells in its nervous system."

This is a complete error, so far as I am concerned. No such claim has ever been made by me in any form, by hint, inference or otherwise. In my last paper on the dimensions of nuclei there appears this sentence: "At the same time it may be true that all large cells connect with motor filaments. The sentence which immediately precedes this one clearly proves that I refer here exclusively to the spinal cord of turtles. This is reviving no theory.

Yours truly,

JOHN J. MASON.

NEWPORT, *June* 13, 1881.

SUN SPOTS.

The following record of observations, made by Mr. D. P. Todd, Assistant, has been forwarded by Prof. S. Newcomb, U. S. Navy, Superintendent Nautical Almanac Office, Washington, D. C., to Gen. H. B. Hazen.

DATE. APRIL, 1881.	NUMBER OF NEW		DISAPPEARED BY SOLAR ROTATION.		REAPPEARED BY SOLAR ROTATION.		TOTAL NUMBER VISIBLE.		REMARKS.
	Groups.	Spots.	Groups.	Spots.	Groups.	Spots.	Groups.	Spots.	
2, 9 a.m......	1	5	0	0	1	1	3	10	
3, 10 a.m......	2	11	1	1	1	2	4	†20	Few faculæ.
5, 8 a.m......	0	0	0	0	0	0	4	†20	Faculæ.
6, 7 a.m......	1	3	0	0	1	3	5	†18	Faculæ.
7, 7 a.m......	1	3	1	5	1	3	4	14	Faculæ.
10, 10 a.m......	0	3	..	..	0	3	3	8	Faculæ.
11, 8 a.m......	0	0	0	0	0	0	2	7	Faculæ.
14, 8 a.m......	2	15	..	..	..	..	3	†20	Faculæ.
15, 8 a.m......	0	†20	0	0	0	0	3	†40	Faculæ. Many of the spots small.
17, 7 a.m......	4	15	0	0	3	10	7	†55	
21, 9 a.m......	0	†60	..	..	..	..	6	†115	Faculæ. Many of the spots small.
23, 7 a.m......	0	0	1	†20	0	0	4	†85	Faculæ. Many of the spots small. Spots probably disappeared by solar rotation.
24, 9 a.m......	0	0	1	10	0	0	3	†60	Faculæ. Many of the spots small.
26, 8 a.m......	1	5	0	10	0	0	4	†55	
28, 7 a.m......	0	0	2	†45	0	0	3	10	Faculæ.
8 a.m......	0	0	0	0	0	0	3	10	
30, 9 a.m......	1	4	2	9	1	4	2	5	Faculæ.

† Approximated.

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JOHN MICHELS, Editor.

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NOTICE TO CORRESPONDENTS.

The writer of a paper "*On Ether*" will much oblige by forwarding his name and address.

THE DISCOVERY OF NEPTUNE.

The brilliant theoretical discovery of this planet by Leverrier and Adams, will be distinctly remembered by many of our readers. Soon after the publication of the mathematical investigation made by the two astronomers who had won so much glory, Professor Benjamin Pierce, of Harvard College, startled the scientific world by the announcement that after all this discovery was only a happy accident, and that the planet found by Galli, in accordance with the directions of Leverrier, was not the planet "to which geometrical analysis had directed the telescope." This statement by Professor Pierce has, we believe, found but little credence among European astronomers and mathematicians. Among those who were well qualified to judge, and who may be considered as free from from national prejudice on this question, we mention Hansen, the well-known theoretical astronomer of Germany, and Jacobi, one of the ablest mathematicians of the same country; both of whom expressed the opinion that Professor Pierce was himself mistaken. In a posthumous book recently published on "Ideality in the Physical Sciences," edited by his son, Professor J. M. Pierce, the present professor of mathematics in Harvard University, Professor Pierce reiterates his former opinion on the discovery of Neptune. It appears that a few years before his death he had made a careful review of his former investigations, and says, p. 173: "I strictly adhere to the correctness of my early statement." This opinion seems to be shared also by Professor J. M. Pierce, who says, p. 201 of the Appendix: "It is to be regretted that the correction of the error was not received, on the part of the French astronomer, with the magnanimity and fairness which it is always painful not to find associated with high intellectual power."

Intrinsically, the question raised by Professor Pierce is an interesting one, and the whole matter seems to us worthy of a new and careful discussion. It may well be doubted whether the argument used by Professor Pierce, that there is a change in the character of the perturbations near the distance of 35.3, will apply to the method employed by Leverrier and Adams in their discussion of the perturbations of Uranus. This method is so interesting that we invite the attention of students of theoretical astronomy to this question, which seems to us capable of a complete and definitive mathematical solution.

VIVISECTION.

Dr. Darwin in a letter to a friend has expressed his views upon vivisection. He writes:

"I know that Physiology cannot possibly progress except by means of experiments on living animals, and I feel the deepest conviction that he who retards the progress of Physiology commits a crime against mankind. Anyone who remembers, as I can, the state of this science half a century ago must admit that it has made immense progress, and is now progressing at an ever-increasing rate. What improvements in medical practice may be directly attributed to physiological research is a question which can be properly discussed only by those physiologists and medical practitioners who have studied the history of these subjects; but so far as I can learn, the benefits are already very great. No one, unless he is grossly ignorant of what Science has done for mankind, can entertain any doubt of the incalculable benefits which will be derived from Physiology, not only by man, but by the lower animals."

PROBABLE BRANCHIAL ORIGIN OF THE THYROID AND THYMUS GLANDS.

By S. V. CLEVENGER, M.D.

There are many reasons for believing that the thyroid and thymus are rudimentary gills, one of the main objections to the view being the structure of these bodies, but in the light of modern biology, structure is almost meaningless in homologizing, besides, the tissues of these parts are not the same in all animals. Owen (Vol. I. p. 565) says the thymus appears in Vertebrates with the establishment of lungs as the main or exclusive respiratory organ. In Siren and Proteus the thymus is wanting, as in all fishes. Gegenbaur (p. 554) speaks of the thyroid as an organ with unknown physiological relations, and that "in fishes it is placed not far from the point at which it was formed, that is, at the anterior end of the trunk of the branchial anterior and between it and the copula of the hyoid arch. In amphibia near the larynx, and is set on the inner surface of the posterior comna of the hyoid." Gegenbaur considers it as an organ of use among Tunicata. This latter idea, as well as the one I have advanced, needs verification. I am unwilling to devote more time to the subject until I can ascertain whether some one has not preceded me in announcing the homology, if it be really one. Much light can be thrown upon the disease known as Goître by clearing up this point.

THE ADDRESS OF THE PRESIDENT OF THE ROYAL MICROSCOPICAL SOCIETY.

LIONEL S. BEALE, F. R. S.

As it is usual on this occasion for the President to deliver an address, I venture to offer for your consideration this evening some suggestions in connection with a subject which in one or other of its aspects must needs be of great interest to every one who wishes to learn all he can about the wonderful changes which continually go on in all living things, some being within and some beyond the present limit of scientific investigation; and though I shall express some views with which perhaps many here will not agree, I trust my remarks may kindle interest and encourage discussion, rather than offend. Where wrong I shall be glad to be corrected, but I claim permission to speak freely what I think, and liberty to advance my conclusions, which, though not at present very popular, may yet be worthy of your consideration.

THE MICROSCOPIC LIMIT, AND BEYOND.

Increased skill and ever-extending knowledge may enable the scientific worker not only to reach the utmost limit of inquiry in his time, but possibly to gratify that constant desire to see into the limitless region which lies beyond the bounds of actual investigation. This is the hope which encourages the thoughtful observer; for who would not consent to spend years in patient research, if by so doing he could succeed, as it were, in projecting his intellect, were it ever so short a distance, beyond the circumscribed region in which the senses can alone operate? Failures and disappointments may be endured if only the observer's mind be buoyed up by the hope that ere his nerve-tissues grow too old, and begin to fail, the longing of his intellect will probably be gratified. To many, indeed, who are unable or unwilling to look into the secrets of nature, such hopes and desires will seem unintelligible or incredible. They will be regarded as the idle fancies of an idle mind; and the ardent scientific inquirer will be pitied or condemned as a weak, foolish person who, like a child, is unable to repress his morbid curiosity to peer into the unseen, and his craving to know the unknowable;—as one deserving to be classed with simpletons and madmen, on the ground that it is absurd to suppose that a really sensible person would spend his life in hard work without remuneration, in preference to doing that which would enable him to gain wealth, and to live at ease, if not in luxury and enjoyment. And certainly it must be confessed that in few departments of research is there less prospect of gaining by success such rewards as are generally sought for, than in the one to which we are attached.

The microscopist, like the astronomer, is ever longing to get a little beyond the point at which he has already arrived. Each new fact gained by research seems but to indicate the existence of more and more important things beyond. Limit is reached and then surmounted, but soon a new limit seems to rise from the mists in the distance, towards which the worker is impelled by new hopes and desires. It is this never halting progress which distinguishes scientific from every other kind of inquiry, and particularly microscopical investigation, for it can never be completed. It deals with the illimitable. The boundaries of to-day are found to have vanished tomorrow, and the eyes and understanding begin to penetrate into regions which but a short time before had been considered far beyond the range of possible investigation.

He only who was quite ignorant of the many and great improvements constantly being made in our methods of research, and in the instruments required in investigation, would think of fixing any limit to the advance of microscopical inquiry. The records of the work of this Society contain many examples of progress towards and advance beyond barriers regarded not very long before, and by considerable authorities, as insurmountable. I well remember the time when in many branches of inquiry, it might have been truly said that the optical instrument was in advance of the methods of examination; when our magnifying powers were higher than we could use without losing rather than gaining as regards the definition of delicate structure. As, however, time went on, this was changed. New and improved methods of examining tissues were discovered, and means adopted, by which excessively thin layers could be submitted to examination, and a power of five or six hundred diametres was no longer sufficient to enable the observer to see all that it was almost certain was to be seen. These remarks more particulary apply to a class of researches upon which I was engaged in 1856-60, concerning the structure and arrangement of the ultimate nerve-fibres in various tissues and organs. Indeed, I feel quite sure that at and before that time advance was actually retarded by the discouragement offered in some quarters, and the hypothetical objections raised to the use of very high powers, and more especially to the methods of preparation of the tissues that were necessary before they could with any advantage be submitted to examination.

Although at this time we can work easily with a twelfth and a twenty-fifth, the results of observation conducted with the aid of such powers are still regarded by some with doubt and incredulity; and if we draw attention to actual structure and arrangement discovered by the higher powers, which could not possibly be demonstrated with the aid of a more moderate lens, our statements may possibly be met with insinuations that what was advanced as the result of observation was, after all, discovered by the imagination only.

Our present limit of observation in investigations on the structure and action of the tissues of man and the higher animals, in my opinion, includes the use of magnifying powers of upwards of 2000 diameters. Objects considerably less than the hundred-thousandth of an inch in diameter can be studied with success, but how much less than these dimensions cannot, I think, be determined with accuracy at this time; for so much depends upon the character of the object, and a number of small points of detail as regards the mode of examination. All who are accustomed to work with high magnifying powers are well aware of the great advantages gained by some very slight change in the degree of illuminating power, the direction and concentration of the rays of light, and very slight and happy alterations in the focus, which may momentarily reveal to the mind new facts of the greatest importance after, perhaps, many hours of careful but almost profitless study.

But in other departments of microscopical research, our present means of investigation enable those familiar with the requisite methods of inquiry to demonstrate characteristics of structure far more intricate and minute than the remarks just made would lead you to infer. Various modifications in immersion lenses and in immersion media have greatly contributed to advance our knowledge of structure and action in the lower forms of life; and there is every reason to think that, as time goes on, methods of observation will be still improved and new methods discovered, and that in consequence conclusions already arrived at will have to be greatly modified or entirely changed. Not only so, but by the aid of photography things dimly seen by the eye may be very distinctly and correctly delineated, and with a perfection of accurate detail which a few years ago we should not have supposed to be possible. In all probability, the application of photography to investigations opon minute structural details will be carried far beyond anything yet reached, although it is really wonderful how much has been achieved up to this time

As regards direct observation, with the aid of very high magnifying powers, upon animal tissues, a department of

microscopical work which has engaged much of my attention during many years, I would remark that many observations have been made upon the structure and arrangement of the most delicate nerve-fibres less than the hundred-thousandth of an inch in diameter, and other tissue-elements of very small insects. With due care, facts are ascertained which could not have been demonstrated with the aid of object-glasses magnifying less than from 2500 to 3000 diameters. Not only is the demonstration of structure and arrangement satisfactory, but in many cases a conception of the action and working of the textures during life has been formed, which would not otherwise have been obtained. The exact relation of certain delicate nerve-fibres to the living matter of the nerve in special organs has been determined, and many elementary facts necessary for the determination of the changes constituting nerve action have been ascertained.

To my mind, however, the study, with the aid of high powers and various improved means of examination, of the phenomena which occur in living matter during life, transcends in importance at this time all other inquiries in which the Microscope takes a leading part. For these changes characterize every form of living matter at every period of its being, and in every condition of health and disease. In every form of living matter which exists or has ever existed, the great mystery of life and death is enacted under our very eyes, but we have not yet been able to discover the exact nature of the change, though we can prove most conclusively that it is not merely mechanical or chemical, as some pertinaciously insist. No chemist or physicist has been able to explain the changes which do occur, or has succeeded in imitating them out of the living body. The most diverse structures and the most widely different chemical compounds are produced by changes occurring in particles of living matter which could not be distinguished from one another, and which are equally devoid of color and structure. Many of the current theories on the nature of vital phenomena are in advance of some that were propounded two thousand years ago; and yet men occupying high scientific positions are found to defend them, and to repeat again and again statements concerning the relation between the living and non-living, which are at variance, not only with facts familiar to every one, but are contradicted by the experience and knowledge every person possesses concerning certain vital phenomena of his own organism

When a particle of living matter is increasing in size —is growing by taking lifeless matter into its substance, and without itself losing anything, is communicating to certain of the elements of this non-living matter, or to combinations of these, the marvellous powers it possesses —movements take place, it may be in every part of the original mass. These movements are, however, always most observable, most active, and most extensive at some part of the circumference. Occurring now on one side, now on the opposite, it is very improbable that the movements in question are determined by any changes occurring in, or by force belonging to, any non-living matter in the vicinity of the living mass. These remarkable movements are universal in the world of life. They are more accelerated in some kinds of living matter than in others, but they are present in all, and in most are discernible at some time or other during the course of existence. Parts of the living matter continually tend to move away and separate from the rest, not in consequence of any attraction between these and surrounding matters outside, nor from any repelling influence exerted by parts of the mass itself upon other parts. There seems to be an active tendency on the part of different portions of a living mass to move away from the rest and so to detach themselves, and, having acquired vital power, to become independent, and to increase and then divide. This remarkable tendency on the part of every kind of living matter to divide and subdivide begins to operate as soon as the original mass has attained a certain size, and it seems to increase in intensity as the living matter approaches its proper dimensions. Invariably when a certain size has been reached, which, however, is different for different kinds of living matter, division occurs. The size is always, within certain very moderate limits, fixed and definite for the living matter of each particular species of living being. Among the lowest forms of existence, however, no definite limit of size has to be attained before division can occur. Particles smaller than the smallest particles that can be seen with the aid of the highest magnifying powers freely divide and subdivide, and there is reason to think that under certain conditions the division and infinite multiplication of the animate particles may continue for a considerable time, none of them attaining their fully developed form or dimensions. In higher forms of life, premature division of a living mass before it has grown for a proper time and reached a certain size, is very detrimental, and in many cases disastrous; for it is associated with degradation or even complete loss of formative, constructive, and developmental power. In some cases, by the rapid multiplication and division of the particles, the well being of the whole organism is jeopardized, and death may be occasioned by the changes brought about by great increase and rapid growth and multiplication of certain particles of living matter belonging to the blood or to some of the tissues.

When a portion of a mass of living matter moves away from the rest, the moving portion invariably presents a convex surface, of which the portion in the exact centre is of course in advance of the rest and is the point towards which the movement of adjacent portions tends. It almost seems as if one minute portion had moved away from the rest and had dragged with it neighboring portions, the power of the particles constituting which was not strong enough to act in opposition to it or to resist its influence. These seem to yield and follow the one or few particles in which the movement is strongest, and which seem to act the part of leader. It may be that certain particles here and there, having attained a larger size, or from being more active than the rest, move forward and determine the direction which is to be taken by those near. As far as can be seen, multitudes of living particles stream in one direction, the greater number being either carried along by the very few or irresistibly drawn onwards by them. The direction taken by flocks of birds and clouds of insects in still air or upon the surface of smooth ground, and shoals of fishes in water, is evidently determined and often very quickly changed in obedience to impulses affecting a very few of the great multitude of individuals of which the whole body is composed. These movements cannot, however, correctly be compared with those of a mass of living matter, inasmuch as there is no reason whatever for supposing that, in the latter, one particle has the power of choosing and determining, much less of conveying to its neighbors the results of its decision or choice and the request to follow.

It will, of course, be said by some that the remarkable phenomena we are considering are comparable with the movements occurring among iron filings under the influence of the magnet, or with the Brush discharge of electricity, the movements of the streams of highly attenuated moving matter in vacuum tubes, and other changes in place affecting particles of non-living matter. Surely it must be obvious to any one who thinks over the facts of the case that no true analogy has been shown to exist between the movements of living particles and those of any form of non-living matter. Nevertheless, the existence of such analogy is still maintained by a few, although the fallacy of the arguments upon which it is supposed to rest has been many times pointed out.

I dare say that for some time to come it will be most difficult to get a hearing for any views not in accord with the

materialist tendencies of what is miscalled the *science* of our time. Thought is to be crushed, and any speculations are to be condemned which do not happen to favor the arbitrary dogmas of the purely physical school. But no doubt these attempts, like preceding ones of the same order made at different periods of history—although they may succeed for a time, and by them people may be driven away from the truth—will ere long be given up. They may be safely left to the gradual process of disintegration and ultimate dissipation by which these and such-like fancies of physical ingenuity will be disposed of.

As I have shown elsewhere, whenever tissue and other matters peculiar to living beings are to be formed, living matter undergoes change. In fact, the act of forming these things corresponds with the cessation of life in the particles.

Let us now consider the probable nature of the marvellous forces or powers which operate upon the material of the living matter, and determine the relations to one another of the elements or collections of elements of which it consists. By the relations established between the elements shortly before living matter dies, will depend the properties and composition of the resulting formed substances. The changes in each particular kind of living matter are somewhat different, but peculiar to and characteristic of that particular kind, and as regards it, constant and uniform. But no differences in the chemical composition or in any physical characters to be shown in different kinds of living matter, will in any way account for or explain the differences so remarkable in the formed material which is produced by or results from the death of the living matter. Nor do any properties of the particles yet discovered enable us to suggest a plausible physical hypothesis to account for the facts.

All those peculiarities in form, structure, and properties of tissue, which characterize the multitudinous forms of life around us, and which enable us to distinguish them from one another, are imposed upon the matter of the moment when it passes from the living to the formed state, or succeed as the result of a long series of changes then initiated. These peculiarities are not found in any ordinary matter, and can only be accounted for on the supposition that some force, property, or power exists which is peculiar and belongs to the matter only while its life lasts. This exerts but a temporary influence on the material particles, which are by it constrained to take up such prearranged positions with respect to one another as must necessarily result in the formation of definite compounds. To this prearranged disposition of the atoms of matter must every character of formed material and every distinctive property of tissue be traced back. This is, indeed, the cause of the varying form, structure, and property of every tissue and every living form in nature. The instant the influence of vital power in restraining the combination of atoms, ceases to be exerted, definite compounds are formed, but these are not living. The matter of which they consist has ceased to live. There are no phenomena occurring in non-living matter in any way comparable with these vital phenomena. Scientific opinion on these matters has lately been unduly influenced by a materialist party, which, like a political caucus, has assumed the right to direct thought and to promulgate the particular dogma which alone is to be accepted by the faithful.

If now I permit myself to pass beyond the point to which I have been led by actual observation,—if I try to advance beyond the present microscopic limit, travelling as it were upon the same lines as when observing within it, and try to realize the phenomena which occur during the early period of development of some comparatively simple vegetable tissue, a leaf for example,—I think the following description will not be far from the truth: A mass of living matter, endowed with special powers working under certain definite conditions, takes up certain materials and increases in size thereby. Imparting to the new matter its powers, unweakened in force, as it grows, it soon divides into several portions, each of which in like manner grows and divides. The arrangement of the several masses, though fixed within certain limits, is determined not by any forces, powers, attractions, or repulsions acting upon all of them, but simply by the rate of growth of each, and division of the several masses under then existing external conditions; the dimensions each was to attain, as well as its properties, composition, color and the like being due to the life, force, or power each separate mass derived from the parental one which gave it origin, and from which it had been detached. But while the above phenomena are proceeding, changes are also occurring on the surface of each mass. The living matter in this situation, whether from the particles first formed, and being therefore the oldest, reaching the surface, and coming to the end of their living existence, or from some other cause I cannot say—passes out of the living state, and the component particles or certain of them combine, assume a certain form, and acquire physical properties they never possessed before. The formed material thus produced owes its colour, chemical composition, physical characters, internal structure, and the like, to the vital force or property in obedience to which the elements of the matter were made to occupy such positions and assume such relations with respect to one another just before death as must ensure the formation of the particular substances which result.

From the moment when the formation of the formed material occurred, the relative position of the several masses probably altered little. Growth may no doubt take place in certain directions by outgrowths, but none of the elementary parts with its surrounding formed non-living material cannot move from its place and get before or above any of its neighbors, as must at least be held to have been possible up to the time when its movements were restricted by the formed substances on its surface.

I would here remark generally, that if very little non-living matter is associated with the living matter, the latter may move in any direction with equal facility, and one part of a mass may place itself above or in advance of another portion just as easily as it could descend below it. But when a layer of formed material has been produced on the surface of the living matter, the entire elementary part becomes subject to gravitation in consequence of the quantity of non-living matter that is formed.

There is not, I think, any good reason for accepting the conclusion that one of a collection of elementary parts, at any period of development, can sympathize or otherwise influence the actions of others, as Virchow seems to think. The suggestion that any force or power acting, as it were, from a centre, governs, regulates, and determines the changes taking place in surrounding and more or less distant particles, is, in my opinion, inadmissible. We might, with as much show of reason, refer the harmonious action of the several parts of the adult organism to some controlling or governing power situated we know not where, influencing, we know not how, actions of many different kinds occurring at considerable distances from the seat of its existence. Although very high authorities have given their sanction to views of the kind, and have advocated the existence in connection with each individual organism of some power or force capable of operating through material of even considerable thickness and of controlling matter at a distance, I venture to assert that the conclusions are not supported by the results of observation and experiment. The idea of one particle of living matter influencing other particles at a distance from it, much less sympathizing with or being affected by vital changes occurring in them, cannot, I think, be entertained by any one who has studied the phenomena as they occur in living beings.

One can indeed conceive tissues of the most elaborate character, and new matter of the most wonderful properties and most complex composition, being developed in the most regular and orderly manner without supposing that any governing or controlling power acts upon them at all, as it were, from a centre. That the most wonderful order is manifest in the arrangement of the component elementary parts, say, of a growing leaf, must be obvious to every one who has ever examined it; but I feel confident that as soon as each living particle has been detached from the mass which preceded, it is no longer influenced by the latter, and does not influence neighboring masses. Each may be pressed upon by its neighbors, and press upon them in turn during growth, but there is no reason to suppose that any one determines the composition, governs the motion, or regulates the action of others. The nutrient matter is distributed to all by vessels or channels running amongst the several collections. Those elementary parts farthest from the nutrient supply will grow more slowly than those nearest to it, but no formative or constructive of synthetic or analytic influence is exerted by the nutrient fluid upon the living matter, nor by the several elementary parts upon one another. Each is under the influence of the vital power associated with the matter of which it in part consists; and whether each can exist independently if separated from its neighbors, or dies soon after it is detached, depends not upon any influence exerted upon it by those neighbors, but simply upon the inherent capabilities of its own vital power, transmitted to it from the living matter which existed before it, and of which it once formed a part.

Nevertheless, each individual elementary part, say, of a leaf, or other organ or tissue, must not in any case be regarded as an individual, independent, or self-dependent organism, for it constitutes but a part of a highly complex whole which has been gradually formed in accordance with a definite structural plan and arrangement, foreseen and prepared for as it were from the very first.

It is only by attributing the observed phenomena to the operation of a special force or power, having no analogy whatever with any known inorganic forces or powers, that a reasonable explanation of the facts can be framed. The phenomena which have been referred to cannot correctly be compared to any processes or actions which occur independently of life, neither can any true analogy be pointed out between these and any physical or chemical changes or actions of which we have at this time any knowledge or experience. That the formation of all tissues and organs is governed by "law" is no doubt true, but the "law" is unknown, and whatever may be its terms, the mode of its operation upon matter is as different from that of any laws that are known to operate in the non-living, as are the known and observed facts of life from those of the inorganic matter of the world.

Now as regards the nature of the actual phenomena of living matter which are at present beyond the range of observation, at least two diametrically opposite ideas are entertained.

1. There is the commonplace notion that structure exists which will account for the actions which take place, but that the details of this supposed structure are too minute or too delicate to be demonstrated by any magnifying powers which have yet been constructed. For this idea there is no sufficient justification. It is one of those assumptions in elaborating which the modern materialist is so ingenious. In this way he struggles, and with some success, to postpone for a time the inevitable fall of the system he has endeavored to make popular in spite of the overwhelming evidence of facts against it. Here I must remark that the word "structure" as employed by physicists is used in a sense utterly distinct from that in which we use the word. This is evident enough if we consider what is understood by the "structure" of a crystal and the "structure" of an organ or tissue. The first "structure" at once disappears when the crystal is dissolved and reappears whenever it is formed. The other structure results, or, as some say, is evolved, only after many series of changes of a very complex character have been completed. Once destroyed, the structure of an organism can only be restored by a long course of similar developmental processes. In fact, there is not the faintest analogy between the structure of an organism and the structure of a stone—the structure due to the operation of living forces and the structure which is inherent with other properties in non-living matter.

2. There is the view supported by myself, and in favor of which I have adduced evidence which I believe to be perfectly convincing, that living matter has no definite structure whatever—that, in fact, its particles, and very probably their constituent atoms, are in a state of very active movement, which renders structure and fixity of arrangement impossible—this active movement being an essential condition of the living state, which latter ceases when the movement comes to a standstill. According to this view the idea of structure as belonging to living matter is inconceivable.

Now we know of no state in which non-living matter exhibits any analogy with matter in the living state, so that the cause of the state under consideration must have reference to the living state, and to that only; and to reassert, as many continue to do, that the phenomena manifested by living matter are to be accounted for by the properties of the material particles, is silly and perverse; and though the view of the peculiar nature of the vital power here put forward and based upon a consideration of the phenomena of living matter, may be ridiculed by materialists, every one who thinks over the matter will see at once why this course is taken by them.

Professor Huxley, in his article "Biology," in the "Encylopædia Britannica"—without defining what he means by the words "molecular" and "machine"—assures his readers that "a mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which, or its vital phenomena, depend, on the one hand, upon its construction, and, on the other, upon the energy supplied to it; and to speak of vitality as anything but the name of a series of operations, is as if one should talk of the 'horologity' of a clock."* This is the sort of teaching that has long retarded the progress of thought, and affords an example of the puerile objections palmed off on the public as scientific criticism, and supposed to be sufficient to controvert evidence founded upon observation, and arguments based on facts which any one may demonstrate. Is it not most wonderful that Professor Huxley can persuade himself that a single reader of intelligence will fail to see the absurdity of the comparison he institutes between the invisible, undemonstrable, undiscovered "machinery" of his supposititious "molecular machine" and the actual visible works of the actual clock, which any one can see and handle, and stop and cause to go on again?

Magnify living matter as we may, nothing can be demonstrated but an extremely delicate, transparent, apparently semi-fluid substance. But observations on some specimens under certain advantages of illumination, and with the aid of the very highest magnifying power that can be brought to bear, favor the conclusion that living matter should be regarded as consisting of infinite numbers of infinitely minute particles, varying much in size, and possibly capable of coalescing, free to move amongst one another, as they exist surrounded by a fluid medium which contains the materials in solution for their nutrition, and other substances. †

[To be concluded in our next issue.]

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* Huxley, Article "Biology," Encyc. Brit.

† From the Journal of the R. M. S.

TRICHINÆ IN MEAT.

A short time ago the public was greatly excited over the introduction into France of trichinated meat. It is remembered that the first case of trichina was noticed at Lyons on provisions exported from America. As regards the importance of this question, it is sufficient to state that, according to official documents, the importations of salt pork and of lard amounted in 1880 to 46,164,000 kilogrammes, of which 37,102,000 kilos. came from the United States. The prefecture of police ordered the inspectors of slaughter houses to examine all the incoming specimens of salt and smoked pork in the different stations and especially that of Batignolles. This station received indeed all the envoys of Havre, where four-fifths of American imports are disembarked. Unfortunately there was a great disproportion between the service of inspection and the continually increasing extent of the provisions. The result was an encumbering of provisions suspected of being contaminated at the points where the inspection took place. A certain number of merchants, moreover, desirous of entering into the possession of their merchandise, so as not to retard its delivery, have sought to evade inspection, and in order to obtain this end took advantage of the small number of inspectors, who, being grouped at certain points, thus allowed certain entries to be free from all verification; they have accordingly introduced their merchandise at these points.

The administration should put an end to this invasion. The Secretary-general of the police departments, anxious for the public health and its interests has, in concert with the Prefect of police, taken opportune measures and charged the municipal Laboratory to investigate the trichinated provisions that have passed the ports. The officers of the Laboratory, accompanied by a commissary of police, proceeded to those accused of introducing un-inspected provisions. Microscopic examination has in several cases shown the presence of trichinæ. We have, therefore, thought it useful to demonstrate the procedure, very simple, which is pursued in the investigation of this infection.

A specimen is taken from the muscular portion, and as much as possible in the vicinity of the bone or tendons, which is very easily accomplished by means of the instrument represented in figure 1. A small fragment of meat is

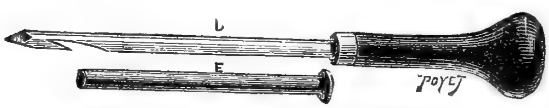

FIG. 1—INSTRUMENT FOR EXTRACTING SPECIMENS OF MEAT, AND ITS SHEATH.

cut with a scissors from the fibres, and stretched on a plate of glass. A drop of diluted alcohol is added or some potassa, and the preparation is levelled by covering it with a lamina of glass (fig. 2); then placed on the stage of the microscope. A magnification of 70 diameters is sufficient for seeing the trichina quite distinctly. It is preferable, nevertheless, in order to work more rapidly and to distinguish the trichinæ of the striated muscular fibres, to magnify it 140 diameters.

Fig. 3 represents one of these preparations seen under the microscope; two trichinæ are enclosed in their cyst and the third has free movement over the muscular tissue.

The parasites that are met with on the trichinated meats of America are not dead, as is proved by the following experiments. A cut is made is made into the trichinated meat; distilled water is used instead of alcohol, and we begin our search for a free trichina. The preparation is then placed upon Ranvier's hot platinum, and the temperature is raised to 40° C. At the end of a certain time, displacements of the trichina will be observed. These movements become stronger from 42° C. to 45° C, and at 50° C. the animal dies.

The municipal laboratory, in employing this very simple mode of investigation has, as we have mentioned, been able to detect contaminated provisions in Paris, and has caused their seizure.

Moreover, as soon as the invasion was known, the necessary precautions were announced to the public, and all the journals have reproduced the circular of the minister.

Trichinated meat, which has been sufficiently cooked in order that the central portions attain at the least a temperature of 60° C., can be eaten without danger; for this purpose, each kilogramme of meat should be cooked about an hour.

The municipal Laboratory adds that it would be advantageous to add vinegar during the cooking.

Such are the means taken at first by the administration to oppose the importation of trichinated provisions.

It remains still to give a decision in regard to provisions stopped at the port as suspected. The inspectors being few in number, rejected a whole chest as soon as they discovered a trichina. It was hardly possible to do otherwise, unless a veritable army of microscopists was at hand, as is done for instance, in Germany, or the pork shop keepers be responsible for the sale of trichinated pork—a procedure which would set 18,000 microscopes in pursuit of the infection.

M. Pasteur, who was consulted upon the question, whether all the provisions of a chest should be seized, if only a single trichinated fragment had been encountered, replied in the affirmative, basing his decision upon the following reasons:

"It is not because I believe in a direct contamination caused by the wanderings of trichinæ from one piece to another, but, in addition to the fact that this contamination can be made through the falling of trichinated *débris* upon those which are not, the separation becomes illusory through the difficulty of effecting it. Finally, a trichinated piece may appear healthy, even after a careful examination."

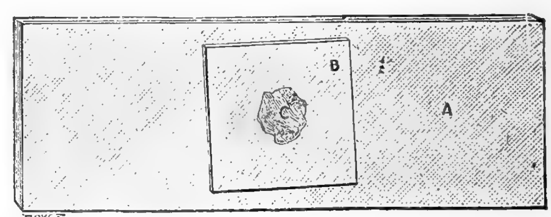

FIG. 2—TRICHINOUS MEAT PREPARED FOR MICROSCOPICAL EXAMINATION. A. GLASS SLIDE. B. THIN GLASS COVER. C. PORTION OF TRICHINOUS MEAT.

The "*Concile de l'Hygiène et de la Salubrité*" attached to the prefecture of police has noticed the impossibility of examining under the microscope all the salted provisions exported from America. As but a small quantity chosen from each cask can be examined, the Council has formed the opinion that the only efficacious means of saving the public health should be to prohibit absolutely the importation of American salted provisions into France. And to prevent the consumption of trichinated meats already introduced, the Council, adopting the view of M. Pasteur, has expressed the opinion that the admistration should destroy all the contents of casks enclosing several pieces that are known to be infected with trichina.

In this state of affairs, we believe that immediate prohibition would be the best course for the government to follow. We will recall, besides the fact that Spain, Portugal, Greece, etc., have prohibited American provisions; that Italy has prohibited all imports of pork; that Germany has interdicted the entrance of any minced pork meat coming from America, and finally that Hungary has taken like measures.

It has been objected that a prohibition would deprive the working class of a large quantity of cheap meat; another objection is the hinderance to commerce. These reasons do not appear valid to us, if there be real danger for the public health, and if there be, as we have said, and as the experiments of M. Chatin show still, living trichinæ in American hams. The danger resides especially in the use of strings preserved in brine, which enter into the composition of pig's cheese and of sausages and hashes of all kinds, such as the pork-seller delivers them, that is to say, always imperfectly cooked.

It is possible that a person may partake of trichinated meat, before its contamination has been noticed, and if a case of trachinosis be not remarked, it is only because the diagnostic of the disease is but little known, and has no positive character. Waiting until the labors of German physicians, which are but little spread in France, enlighten us on the symptoms of this affection, we deem it useful to examine under the microscope the faeces of those suffering from typhoid fever, in order to find out whether, under the cover of this malady, there is not, as often happens, a case in point of trichinæ eliminating themselves in part by this way.—*La Nature.*

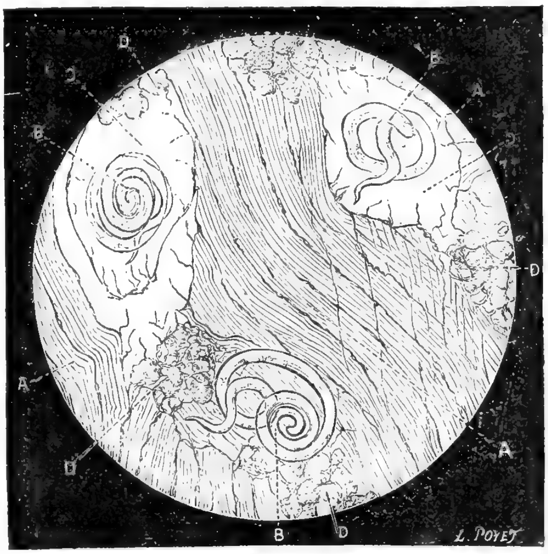

Fig. 3—Portion of Trichinous Meat as Seen Under the Microscope, Magnified 140 Diameters. A. A. A. Muscular Fibres—B. B. Encysted Trichinæ. C. C. Cyste. D. D. D. Fat Globules.

TRICHINOSIS.

Dr. E. C. Wendt presented to the New York Pathological Society, April 13th, specimens illustrating Trichinosis. The slides under the microscope showed muscular trichinæ in a free state. They exhibited different degrees of parasitic development, although they were all taken from the same woman. The infested muscles were obtained from a recent fatal case of the disease which had occurred in Hoboken. For the history of the case he was indebted to Dr. W. T. Kudlich of that city. The whole course of the malady, from the initial enteric symptoms through a typhoid stage with intense muscular pains to the lethal termination, was so typical that the detailed clinical account of this case might be omitted. It should be stated, however, that shortly after the young robust wife fell ill, the husband also took to his bed with well-marked symptoms of trichinosis. In view of the present agitation of the public mind over the wholesale prohibition of American pork by the Continental powers, it might be of interest to remember that in the present instance the disease was unmistakably traced to a home product. The living parasites were used for

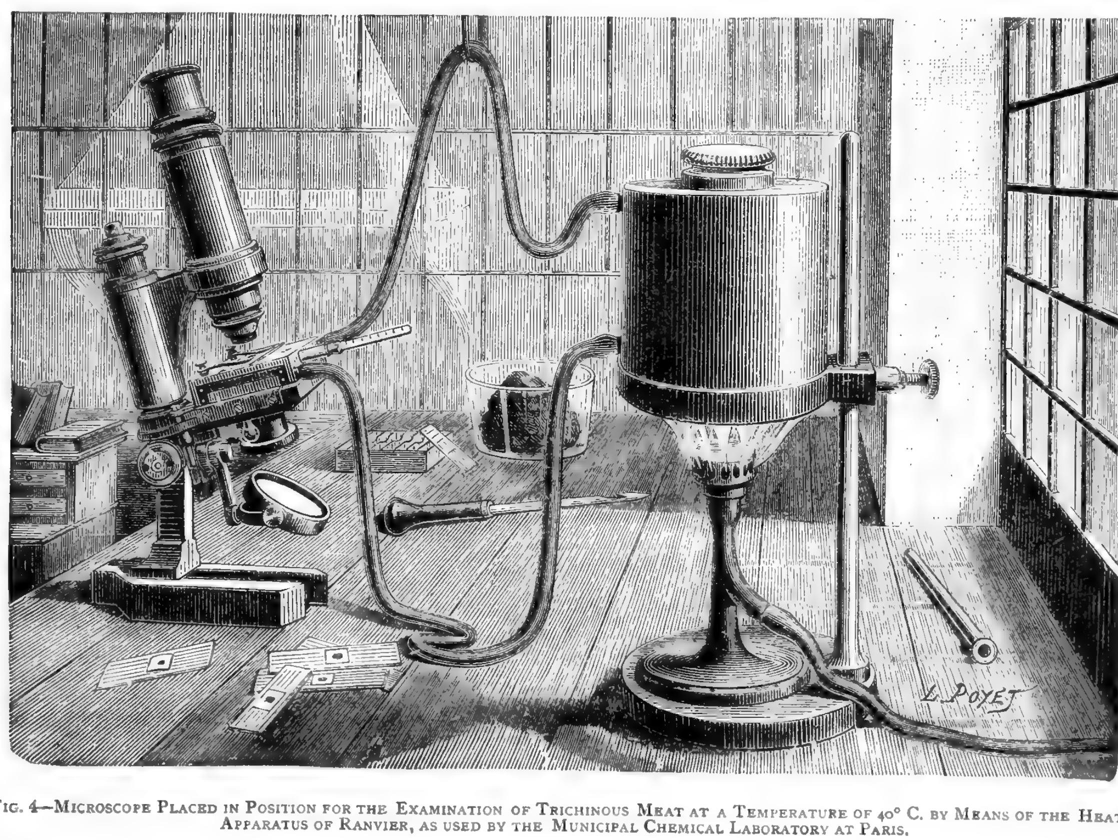

Fig. 4—Microscope Placed in Position for the Examination of Trichinous Meat at a Temperature of 40° C. by Means of the Heating Apparatus of Ranvier, as used by the Municipal Chemical Laboratory at Paris.

purposes of experimentation; and, while entirely new facts were not elicited, a few words might be said as to the results of various trials.

Encapsulated trichinæ were notoriously tenacious of life; but here were the immature, only recently emigrated parasites, still wandering about in the muscles. A few of the animals had indeed already assumed the position of a spiral coil, which was the preparatory stage of encapsulation. But the majority were either stretched out or twisted at either extremity. Little pieces of the woman's muscles were exposed to the action of cold, being several times frozen. Examination, four days afterward, found them apparently quiescent. A gradual elevation of temperature up to about 100° F. soon proved that life was not extinct, in so far, at least, as active motions can be interpreted as an indication of vitality. Ten days later the parasites were still alive. Some of the flesh was then allowed to undergo partial putrefaction. Even then the animals were living. This was thirteen days after the death of the woman.

On the day following the autopsy, some fresh muscle was teased, and, there being an abundance of living trichinæ, many thus became isolated. The animals were never seen to actually creep along in a definite direction. Their movements resembled the unfurling and recoiling of a pennon. Nevertheless, a change of place was now and then fortuitously effected. Next, the parasites were subjected to the action of different reagents. Saliva produced no visible effect upon them. Dilute acids resulted in increased activity of motions. Alkalies made them sluggish. Concentrated solutions of both rapidly killed them. In carbolic acid they squirmed and writhed before dying. Glycerine, contrary to what was supposed, did not immediately kill them. Some lived for ten minutes after its addition. Finally, however, the worms became shrivelled up into almost shapeless filaments. If previously heated, however, they retained their form to a great extent.

A little of the fresh muscle was submitted to artificial digestion by being placed in a suitable fluid and exposed for twelve hours to about body heat. The muscle was in great part dissolved at the end of this time, and many free parasites were found in the liquid. But they were, if anything, less active than they had been, and, as soon as the liquid was allowed to cool, their movements ceased, to be renewed, however, on reheating the slide. A noteworthy fact, and one of great interest, was that the trichinæ had unquestionably grown. But, though their size was now increased, and although indications of sex could be barely perceived, a distinct evolution into mature males and females was not obtained. It must be remarked, however, that future experiments at such artificial breeding may be more successful. Through an inadvertence the continuation of the artificial digestion was interfered with, the animals being killed by over-heating.

Portions of partially putrified muscle were placed in vials containing water, with the addition of a small proportion of glycerine, carbolic acid, and alcohol. In this liquid the parasites were maintained in a comparatively good state of perservation, showing the details of their interesting organization with satisfactory clearness. Permanent specimens, no matter what technique of preparation may be employed, were never found as perfect as recent ones made from bits of muscle thus kept. Of course, fresh would answer still better than partially decayed muscle.

Concerning the pathological condition of the infested muscles, the changes there found were the frequently described conditions of acute myositis accompanied by vitreous metamorphosis, cloudy swelling, and fatty degeneration. In some places the interfascicular hyperæmia and small-celled infiltration were beautifully seen.

The subject of trichinosis had occupied his attention for a number of years; but this was not the place to discuss the many questions which presented themselves; only one further remark as to the diagnostic value of examining small bits of muscle removed from accessible regions in patients suspected of trichinosis. If the animals were found, of course the evidence was incontrovertible. But, *vice versa*, a conclusion could not be arrived at. This he wished to emphasize, because a contrary opinion was prevalent in some quarters. In the present fatal case of trichinosis, small bits of the deceased woman's muscles were torn from the gastrocnemius and deltoid muscles; and while some specimens had contained numerous parasites, others had been found without them. In the diaphragm, intercostal muscles, and other well-known places of predilection, every examined specimen showed abundant parasites.

Dr. Carpenter's observation at the dead-house of Bellevue Hospital had been that encysted trichinæ were found more frequently in the pectoral muscles or the diaphragm than in the deltoids or the gastrocnemii.

The President (Dr. T. E. Satterthwaite), remarked that the subject of trichinosis was now of very great interest to the country at large, and we should be anxious to get all the light possible upon it. Though a vast amount of labor had been expended on the origin, clinical history, and treatment of trichinosis, we have good reason to suppose it was seldom recognized during life, and even after death would often escape notice, unless the examiner had his attention specially directed toward the possibility of its occurrence. Consequently our present statistics could not be relied upon in forming an opinion as to its prevalence. One of the points on which we needed more information was the period of incubation. This was variously placed at between ten and forty-two days; or, rather, according to our present ideas, it would take ten, but might take forty-two days for the young trichinæ to appear in the muscles after the infected meat had been eaten.

Now, it was just upon this variable period that the dealers relied chiefly when they were prosecuted for selling trichinous meat. As most infected persons are Germans, who are in the habit of eating uncooked meat, more or less continuously, it is generally easy for the accused to show that other hams or sausages were eaten during this period of forty-two days, and as statements are to be found that a limited number of living trichinæ have been eaten without harm, it is almost impossible to secure conviction. Dealers therefore do not ask for an examination of hogs or their products, nor are they afraid fo being convicted, even should they sell trichinous meat.

Now it is particularly important to determine whether or not there is in this variable time between the ingestions of the animal and the subsequent migration of the larval form, and more experiments should be made on animals to determine it.

Then another point is important, which is, How frequently is trichinosis met with? In Europe it is said to occur in from one to two per cent. of all cadavers. In this country we have as yet no trustworthy data; at least this conclusion may be drawn from the recent report of late Assistant-Surgeon Glazier, to the U. S. Marine Service. Still we know that eight hogs out of a hundred were once found trichinous in Chicago, and though this percentage has been once exceeded in Germany, it is a large one, and invites consideration.

Especially important was, he thought, the determination whether living trichinæ can really be swallowed with impunity, if in small numbers, and, if so, what quantity is necessary for infection; whether such trichinæ be the larval or fecundated forms; and whether emetics or purgatives were not afterward employed to expel them, as in this latter case it is reasonable to suppose they might have been removed without harm to the individual.

The President further remarked that there were now no specially appointed officials who made it their duty to inspect meat, as was formerly done. A diminished appropriation led to the suspension of this work.

BOOKS RECEIVED.

ISLAND LIFE; OR, THE PHENOMENA AND CAUSES OF INSULAR FAUNAS AND FLORAS, including a revision and attempted solution of the Problem of Geological Climates, by ALFRED RUSSEL WALLACE.—Harper Brothers, New York, 1881.

The authors of the theory of evolution undoubtedly feel the responsibility involved in revolutionizing one of the most important branches of Science; it is therefore natural to find both Dr. Darwin and Mr. Wallace devoting their best energies to aid evolutionists in placing "the theory" upon a sound basis.

Perhaps one of the strongest arguments that can be advanced in favor of evolution is the fact, that it offers a key for solving difficulties encountered by naturalists, which hitherto were inexplicable, or accounted for by a line of reasoning now shown to be erroneous.

The distribution of animal life upon the globe, which forms the subject of the present work, exhibits in a strong light some of the fallacies of the older naturalists, and the value of the theory of evolution in interpreting the work of Nature.

The old school of naturalists explained the geographical distribution of animals by believing, that "the several species of animals and plants" were special creations, and consequently assumed, that every animal was *exactly* adapted to the climate and surroundings amid which it lived, and that the only, or, at all events, the chief reason why it did not inhabit any other country was that the climate or general condition of that country was not suitable to it.

In the present state of knowledge respecting the fauna and flora of the whole world, it is not difficult to prove that other reasons must be found to account for the phenomena met with in the general distribution of animal life, and Mr. Wallace makes the case quite clear by giving some striking illustrations.

It is true that hot climates differ from cold ones in all their organic forms, but its effects are by no means constant, but are irregular and uncertain, and the contrast does not bear any proportion to the difference of temperature.

For instance, between frigid Canada and sub-tropical Florida there are less marked differences in the animal productions than between Florida and Cuba or Yucatan, which are much more alike in climate and so much nearer together. So the differences between the birds and quadrupeds of temperate Tasmania and tropical North Australia are slight and unimportant as compared with the enormous differences we find when we pass from the latter country to equally tropical Java, and if we compare corresponding portions of different continents, we find no indications that the almost perfect similarity and general conditions have any tendency to produce similarity in the animal world. The equatorial parts of Brazil and of the west coast of Africa are almost identical in climate and in luxuriance of vegetation, but their animal life is totally diverse. In the former we have tapirs, sloths, and prehensile-tailed monkeys; in the latter, elephants, antelopes, and man-like apes; while among birds, the toucans, chatterers and humming-birds of Brazil, are replaced by the plantation-eaters, bee-eaters and sun birds of Africa. Parts of South-temperate America, South Africa and South Austalia correspond closely in climate; yet the birds and quadrupeds of these three districts are as completely unlike each other as those of any parts of the world that can be named. The present work, although complete in itself, is one of a series prepared by Mr. Wallace to account for the geographical distribution of animal life, by the theory of evolution, and being the result of many years study by one of the most eminent of living naturalists, will command the attention of all who desire to find a true solution of the subject.

Some of the most remarkable and interesting facts in the distributions and affinities of organic forms are presented by *islands* in relation to each other and to the surrounding continents. Yet their full importance in connection with the history of the earth and its inhabitants has hardly yet been recognized; and in order to direct the attention of naturalists to this most promising field of research, Mr. Wallace has restricted himself in the volume now before us, to the elucidations of some of the problems there presented to us.

Such then is the scope and purpose of "Island Life, or the Phenomena and Causes of Insular Faunas and Floras," involving the study of a class of subjects embracing in their very nature the visible outcome and resudical product of the whole past history of the earth. There is no royal road to the acquisition of knowledge, and to prepare those readers who have not been trained in such studies to appreciate the conclusions drawn, Mr. Wallace in the first eight chapters devotes much space to the explanation of the mode of distribution, variation, modification and dispersal of species and groups, illustrated by facts and examples; of the true nature of geological change as affecting continents and islands; of changes of climates, their nature, causes and effects; of the duration of geological time and the rate of organic development.

The aim of Mr. Wallace in this work is the development of a clear and definite theory, and its application to the solution of a number of biological problems. That theory may be briefly stated as follows: That the distribution of the various species and groups of living things over the earth's surface, and their aggregation in definite assemblages in certain areas, are the direct result and outcome of a complex set of causes, which may be grouped as "*biological*" and "*physical*." The *biological* causes are mainly of two kinds—first, the constant tendency of all organisms to increase in numbers, and to occupy a wider area, and their various powers of dispersion and migration through which, when unchecked, they are enable to spread widely over the globe; and secondly, those laws of evolution and extinction which determine the manner in which groups of organisms arise and grow, reach their maximum, and then dwindle away, often breaking up into separate portions which long survive in very remote regions. The *physical* causes are mainly of two kinds. We have, first, the geographical changes which at one time isolate a whole fauna and flora, at another time lead to their dispersal and intermixture with adjacent faunas and floras, and here Mr. Wallace endeavored to ascertain and define the exact nature and extent of these changes, and to determine the question of the general stability or instability of continents and oceans; in the second place he also investigated the exact nature, extent and frequency of the changes of climate which have occurred in various parts of the earth, because, as it may be supposed, such changes are among the most powerful agents in causing the dispersal and extinction of plants and animals. The importance attached to the geological climates and their causes, induced Mr. Wallace to discuss this branch of the subject at some length, and the most recent investigations of geologists, physicists and explorers were fully called into requisition.

Mr. Wallace next applied these facts and theories to explain the phenomena presented by the floras and faunas of the chief islands of the globe, which are classified, in accordance with their physical origin, in three groups or classes, each of which is shown to exhibit certain well marked biological features.

Mr. Wallace then defines what are called "*areas of distribution*," as applied to species, genera and families, and, taking British mammals and land birds, he follows them over the whole area they inhabit, and obtains a foundation for the establishment of "*zoological regions*," and a clear insight is formed of their character as dis-

tinct from the usual geographical divisions of the globe.

All these facts are then shown by Mr. Wallace to be a necessary result of the "*law of evolution*." The nature and amount of "*variation*" are exhibited by a number of curious examples; the origin, growth and decay of species and genera are traced, and all the interesting phenomena of isolated groups and discontinuous generic and specific areas are shown to follow as logical consequences.

The remaining subjects discussed by Mr. Wallace carry him into the realm of fierce controversies, and relate to theories involving problems awaiting further investigations for their solution. One of these subjects —The Position of the Great Oceans and Chief Land Areas—is dealt decisively by Mr. Wallace, who claims that "on the whole they have remained unchanged throughout geological time." This declaration of the author has been already challenged, and we shall watch with interest if Mr. Wallace is capable of maintaining his position on this subject.

Perhaps the most valuable part ot this work is the discussion of the question of geological time as bearing on the development of the organic world, leading to an investigation as to the exact nature of past changes of climate.

In answer to those who may consider the subject last spoken of as unsuited to such a work as the present, the author claims that, although many of the causes introduced are far too complex in their combined action to enable us to follow them out in the case of any one species, yet their broad results are clearly recognizable, and we are thus enabled to study more completely every detail and every anomaly in the distribution of living things, in the firm conviction that by doing so we shall obtain a fuller and clearer insight into the causes of nature, and with increased confidence that the "mighty maze" of Being we see everywhere around us is "not without a plan."

No person should offer an opinion on the "theory of evolution" who has not studied this work of Mr. Wallace, for it forms an essential part of the literature ot the subject.

NOTE IN REGARD TO "PRIMITIVE DESIRES."

In a communication published in an earlier number of "SCIENCE," (No. 29, Jan. 15, 1881) Dr. Clevenger, of Chicago, discusses the relalation existing between the desire for food, and the desires connected with the multiplication of the species. He appears to draw the conclusion that hunger is the primitive desire.

There are some observations made by alienists, which strongly tend to confirm Dr. Clevenger's theory.

It is well known that under pathological circumstances, relations obliterated in higher development and absent in health, return and simulate conditions found in lower and even in primitive forms.

An instance of this is the *pica* or morbid appetite of pregnant women, and hysterical girls for chalk, slate pencil and other articles of an earthy nature. To some extent, this has been claimed to constitute a sort of reversion to the oviparous ancestry, which like the birds of our day sought the calcareous material required for the shell structure in their food (?)

There are forms of mental perversion, properly classed under the head of the degenerative mental states, with which a close relation between the hunger appetite and sexual appetite becomes manifest.

Under the heading "Wollust.'—Mordlust-Anthropophagie" Krafft. Ebing describes a form of sexual perversion, where the suffered fails to find gratification unless he or she can bite, eat, murder or mutilate the mate. He refers to the old Hindoo myth of *Çiva* and *Dúrgá* as showing that such observations in the sexual sphere were not unknown to the ancient races. He gives an instance, where after the act, the ravisher butchered his victim, and would have eaten a piece of the viscera, another where the criminal drank the blood and ate the heart, still another where certain parts of the body were cooked and eaten.*

In reference to this question, Dr. Clevenger some time ago sent me the following interesting letter, which, anticipating much that I should otherwise say, may find a place here.

CHICAGO, *February* 17, 1881.

Dear Doctor:

The suggestions that you made, in a recent note to me, on the extension of the Hunger Theory to Man, are of too much value not to be published. Professor E. D. Cope kindly sent me the reprint of an article of his entitled "The Origin of the Will" which appeared in the *Penn Monthly*, for June, 1877, wherein the Professor takes the ground that Hunger is the primitive desire. "The movement of the Amœba in engulfing a Diatom in its jelly is as much designed, as the diplomacy of the statesman or the investigations of the student, and the motive may be the same in all three cases; viz.: hunger" (p. 438). "In the lowest animal the first movement was doubtless a mere discharge of force; but the first designed action, the appropriation of food, was due to a sense of want or hunger, which is a form of pain. This was followed by gratification, a pleasure, the memory of which constituted a motive for a more evidently designed act, viz.: pursuit "(p. 446). I am rather inclined to reverse the conception of the unconscious being derived from the conscious act and conclude that the pain of hunger is akin to the desire barium may have for sulphuric acid or any molecule may have for another.

Yours truly,

S. V. CLEVENGER.

I cannot see the necessity of considering "the movement of the Amœba, as designed as the diplomacy of the statesman etc." It is either a truism according to one reading, or utterly erroneous—according to another. If "as designed" in the above means—based on the same broad summation of registered impressions potent in intellectual activity, I must say that due regards have not been paid to very fundamental facts in framing the clause criticized.

E. C. SPITZKA.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

In reply to the remarks made by Mr. Morris about my communication to you (No. 43), I would like to say a few words.

In the first place I beg to enter a protest against the gentleman's suggestion with which he prefaces his reply, to wit:

"The main difficulty seems to be that I have gone counter to certain authors whom they are disposed to consider as authorities," meaning Prof. Dolbear and the writer. As to this objection, so often raised at the present moment, it seems to me that it is only applicable in case the authority is adduced in place of an argument, or in order to fortify it. As a rule, men of an independent turn of mind do not believe or accept theories because this or that authority has advanced them, but because they are plausible to them—perhaps only as long as they do not hear of any other in regard to the subject. But, if they should adopt another theory in place of one formerly held, it is certainly not on account of the fact that it has emanated from a certain authority, but because their mode of thinking and working out problems agrees with that which originated the theory, *i. e.* the authority's.

Since I have nowhere in my letter quoted any authority specifically, gathering my arguments from the works of those men whose writings are most congenial to my frame of mind, and from them weaving the net of my intellectual product with an occasional glimpse from my

* Ueber gewisse Anomalien des Geschlechts-triebes. Von Kraft-Ebing, Arch f. Psychiatrie VII.

own brains, I would seem to be justified in resenting this peculiar argumentation.

I might, in view of this unjust criticism, retort that perhaps it is altogether a better way to rely on an occasional authority, a good number of whom are towering up high above the sea of opinions as trustworthy beacons of light, than to steer along without looking up to them as guides now and then, and perhaps be wrecked on some unknown shore or unsuspected reef. The tendency to scoff at authorities because they are authorities, is just as pernicious as that to put faith in them for this same reason only.

As to my somewhat confused idea of heat, of which Mr. Morris takes the liberty to speak, I confess that I have supposed he understood the difference between radiant and conducted heat,* and he also was aware what was understood by universal consent with the expression "work." I should not have undertaken this discussion on physical subjects had I not been convinced that the terms to be used were agreed upon. However, Mr. Morris seems to be in a fair way to come down to the very last questions about the nature of motion and matter.

As to "*latent heat*," if Mr. Morris, Sir Wm. Thompson,* and many others persist in calling heat that which is not heat, they are at liberty do so; yet they are wrong.

This I have conclusively shown, and Mr. Morris has not even tried to argue on it. Nor has he thought necessary to argue in regard to my remarks on his erroneous conception of the action of gravity. He only reiterates his assertion that the energy with which a body weighing a million pounds would fall on a body weighing one pound is the same. In order to prove this he says "we must add," and add and add, and then *one* will develop just as much energy as a *million!*

It seems futile to argue longer on a proposition that is in direct conflict with Newton's first law. If Mr. Morris has no room for the latter in his Universe, I must respectfully decline to enter it, preferring to stay outside of it in company with Sir Isaac and various others equally sound and reliable.

If Mr. Morris says motion is motion and cannot possibly become anything else, he is certainly right; but he forgets that there are certain *forces* for which we have as yet not been able to *prove* conclusively that they *are* motions. Of course, Mr. Morris has told us how he conceives of this relation between gravity and molecular motion, so called. (And there is cohesion and magnetism yet to account for.) But his explanations are wide away from the mark, which lies in an entirely different direction.

The combined action of all the radiant energy emanating from an infinite number of celestial bodies is transmitted in every direction through the Universe, and by oscillations, vibrations, and undulations of the attenuated matter (*not ether*—there is no ether!) which fills the interstellar spaces. In striking the surface of the various orbs, great and small, it exerts a uniform pressure, gravity.

Respectfully,
GEO. W. RACHEL, M. D.

NEW YORK, *May* 30, 1881.

THE "*Astronomische Nachrichten.*"—It is announced that after the termination of the current volume, by authority of the Prussian Government, a new arrangement for the management of this journal will take effect. It will be edited by Prof. A. Krueger, the director of the Observatory at Kiel, in co-operation with the president of the "Astronomische Gesellschaft," of which association it will become a recognized organ.

* SCIENCE, Vol. I. p. 245. L. 24 fr. below.

** Admits that t is not heat, but favors the expression for convenience.

To the Editor of SCIENCE:

I can scarcely permit such curious statements as made by Prof. A. E. Dolbear, to pass unnoticed. In "SCIENCE" No. 43, he says:—"The decaying stump that shines by night, has a temperature not appreciably higher than surrounding objects." Can it be possible that he compares the state of matter in ancient wood, with the inconceivably rare gas whence Neptune was formed? Several cubic miles of it only weighed a grain, as has been proven by Helmholtz. It was in dissociation, no two atoms touched, therefore we assert with reason that it was absolutely cold and dark. The atoms in the stump had been in intimate association; indeed their organization was once so complex as to have been endowed with that most mysterious of all entities—LIFE!

When decaying, it was surrendering the force whose work organized it, and its faint luminosity was a portion thereof. The light was a result of preceding work, but in interstellar space, where atoms were yards apart, no previous work had been performed, and no force evolved whether heat, light, or any other save gravity and the slowest radial motion possible.

EDGAR L. LARKIN.

NEW WINDSOR OBS., Ill., *June* 13, 1881.

REPLY TO DR. J. J. MASON'S LETTER.

The writer of the review referred to, states that notwithstanding the construction which Dr. J. J. Mason now desires to see placed upon his words, the most careful reader would fail to draw any other conclusion from Dr. Mason's article, than that it was written in support of the theory that large cells are motor, and that sensory cells are small. It is true as Dr. Mason states that the sentence just preceding the one quoted in his letter refers specifically to the spinal cord of the turtle. But it is none the less true that the whole paragraph polemizes against a statement of Stieda's that the observations "have great weight against the conclusion that only the large nerve cells are connected with motor fibres," as not representing the ordinary view. In the earlier part of his article, Dr. Mason indeed goes so far as to question the statements of our best cerebral antomists that certain very large cells are connected with the auditory, *i. e.* a sensory nerve, and this in obedience to the same theoretical bias which is manifested a few lines further on in this wise. "I would suggest, however, to those who may feel disposed to regard these cells (large cells of auditory nucleus and oblongata) as connected with the sense of hearing, that such a view involves giving to this apparatus in its central portion, a structure almost identical with one universally admitted to be motor, like, for example, that concerned in raising the lower jaw; whereas in the central structures for vision and olfaction *the cells are all very small.*" (Italics are own.) What other than the size of the cells and their nuclei does Dr. Mason refer to when he speaks of a "structure universally admitted to be motor?" Especially when it is borne in mind that immediately after he claim that all sensory cells are very small. In view of all this Dr. Mason's statement that no such claims as the one imputed to him by the reviewer had ever been made by him "in any form by hint, inference or otherwise," must have been penned in strange forgetfulness of what he has laid down in his published article. The reviewer can only interpret the remonstrance as an abandonment by Dr. Mason of his previous position. Every statement in the quoted paragraphs is simply erroneous, and to bring Dr. Mason face to face with facts that he has questioned, the reviewer refers to Dr. Mason's statement that the cells connected with vision "are *very* small," and the reliable findings of Professor Packard, who happened to state that in the locust these cells are very large in relation to the other cells of the nervous system.

R. C. S

THE CONSERVATION OF ELECTRICITY.

The following is from the preface to "Elementary Lessons in Electricity and Magnetism," by SILVANUS P. THOMPSON, now in the press.

"The theory of electricity adopted throughout is that electricity, whatever its nature, is *one*, not *two*. That this electricity, whatever it may prove to be, is not *matter*, and *energy*; that it resembles both matter and energy in one respect, however, in that it can neither be created nor destroyed. The doctrine of the *Conservation of Matter*, established a century ago by Lavoisier, teaches us that we can never destroy nor create matter, though we can alter its distribution and its forms and combinations in innumerable ways. The doctrine of the *Conservation of Energy* which has been built up by Helmholtz, Thomson, Joule, and Mayer during the last half century teaches us that we can neither create nor destroy energy, though we may change it from one form to another, causing it to appear as the energy of moving bodies, as the energy of heat, or as the static energy of a body which has been lifted against gravity or some other attracting force into a position whence it can run down, and where it has the potentiality of doing work. So, also, the doctrine of the *Conservation of Electricity*, which now is growing into shape, but here first enunciated under this name, teaches us that we can neither create nor destroy electricity, though we may alter its distribution—may make *more* to appear at one place and *less* at another—may change it from the condition of rest to that of motion, or may cause it to spin round in whirlpools or vortices which themselves can attract or repel other vortices. According to this view all our electrical machines and batteries are merely instruments for altering the distribution of electricity by moving some of it from one place to another, or for causing electricity when heaped up in one place to do work in returning to its former level distribution. Throughout these lessons the attempt has been made to state the facts of the science in language consonant with this view; but rather to lead the young student to this as the result of his study than to insist upon it dogmatically at the outset.'

A WATER CARRYING TORTOISE.

At a meeting of the Academy of Sciences the other evening, a very fine specimen of the desert land tortoise, from Cajon Pass, San Bernardino county, California, was received. The specimen had been carefully prepared, and was as large as an ordinary bucket. The tortoise is a native of the arid region of California and Arizona, and Prof. E. T. Cox, who was present, related a curious circumstance connected with it.

He found on dissecting one of them that it carried on each side a membrane, attached to the inner portion of the shell, in which was about a pint of clear water, the whole amount being about a quart. He was of the opinion that this water was derived from the secretions of the giant barrel cactus, on which the tortoise feeds. This cactus contains a great deal of water.

The tortoise is found in sections of country where there is no water, and where there is no vegetation but the cactus. A traveler suffering from thirst could, in an emergency, supply himself with water by killing a tortoise. They are highly prized by Mexicans, who make from them a delicious soup. The foxes of the desert attack the tortoise and finally overcome them by dragging them at times for miles.

B. B. Redding said he would try to obtain a live one for the Academy in order that its habits and peculiarities may be carefully observed and noted. He instanced being on the Gallapagos islands in 1849 and assisting in the capture of 92 land tortoises, varying from 450 to 600 lbs. in weight, which the vessel brought to San Francisco and sold for more money than the whole cargo of lumber netted at that time. They were two months on board the vessel, yet ate nothing and those killed had in them considerable quantities of pure water. They live on the high lava rocks, which rise as mountains on the island, where there are no springs or streams, and the only dependence of animal life for water is necessarily upon the irregular and uncertain rain showers.

It may be mentioned that the tortoise are of different species, though they may have the same habit in respect of carrying water. The famous edible species of the coasts of the Pacific and Indies, of which the headquarters is at Gallapagos islands, is the *Testudo Indica*. They grow to five, six, and even seven hundred pounds or more. Those found in this State are smaller and are the *Agassii* species first described some years ago by Dr. J. G. Cooper, if we recollect aright. Those Mr. Redding describes from the Gallapagos were offered water while on the ship but refused it. Yet when killed they all contained water. The place they inhabit is a dry one, lacking water. It may be that they go to the high places and obtain it from the vegetation, the same as our species does.

DALTONISM.—A Belgian Commission is making investigations on Daltonism. Their method of procedure is as follows:—On a table exposed to the bright sunlight are placed skeins of wool varying in color. The subject under examination is given, for example, a green colored one and he is told to select another of the same color. If he does so without hesitation, he is not affected with Daltonism, but he is still subjected to other trials, as for instance, the observation of colored signals at a distance. This applies especially to those employed on railways.

NEW RECORDING APPARATUS OF MOVEMENTS.—About twenty years ago M. Marey proposed to inscribe the different movements of living animals by means of a lever, as light as possible, and protected from every cause tending to set it in vibration, Since this epoch a considerable number of operations have been accomplished by the aid of this instrument; thus, the phenomena of the circulation of the blood, of respiration and of the movements of the heart have, in the employment of this method, been conclusively solved. Nevertheless one objection has been raised against these instruments: in the sometimes excedingly complicated tracings of physiological acts, the proper movements of the lever have increased the real curve of the movement which is to be inscribed. M. Marey has therefore invented and presented to the Academy a new inscriptic apparatus, the lever being reduced, which gives microscopic inscriptions, and thus can inscribe rapid movements with the greatest precision. The tracings of this instrument, which may be produced by the vibrations of the voice, or by the breath, are afterwards enlarged by projection and reduced to the necessary size. The microscopic inscription given by the new apparatus extends to an almost indefinite degree phenomena susceptible of registration.

FLUORESCENT SUBSTANCES.

SUBSTANCE.	By transmitted light.	Fluoresces.
Magdala red.................	Red	Red
Induline (acid sol.)...........	Dirty green	Red
Nigrosine (acid sol.)..........	Dirty green	Red
Tri-sulpho-acid of induline....	Blue	Red
Resorcin (di-azo compound)*..	Yellow	Vermilion
Resorcin (di-azo compound)*..	Violet red	Vermilion
Resorcin (di-azo compound)...	Green	Dark red
Resorcin (phthalic acid compound)*	Yellow	Green
Resorcin (phthalic acid and bromine compound)........	Red	Green
Amido-phthalic acid..........	Colorless	Green
Murexide (di-azo compound)..	Yellow	Green
Beta-naphthole...............	Brown	Blue
Naphthalamine*....	Colorless	Violet-blue

The four marked * are the best for exhibition purposes, the last surpassing sulphate of quinine. The fluorescence of the di-azo resorcin compound in direct sunlight shows a fluorescence not inferior to vermilion paint in brilliancy.

SIDNEY JEWSBURY.

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THE NEW COMET.

The great comet which has so suddenly flashed into our Northern sky is one of the most brilliant comets that has appeared for many years. It has a large and very stellar like nucleus which is surrounded with envelopes, very much like those of the Donati comet of 1858, which was described so well by Professor George P. Bond of the Harvard College Observatory. The dense nuclei of such comets give one the idea of a mass and quantity of matter quite different from the ordinary telescopic comets, through which the faintest stars can be seen. The tail of the present comet is now about twelve or fifteen degrees in length, and altogether this comet presents a very beautiful spectacle at three o'clock in the northeastern morning sky. The motion of the comet is three or four degrees toward the north, and it will soon reach a position where it will be visible during the entire night in the greater part of the United States.

The first duty of the astronomers will be of course to get observations of its positions and to compute the orbit of the comet. Since for this purpose observations on three days are sufficient, we shall soon have a certain knowledge of its motion. The knowledge of the orbit will decide the question whether this is the large comet whose discovery was telegraphed to Europe from Buenos Ayres by Dr. B. A. Gould, on June. 1st, and also whether it is identical with the great comet of 1807. The observations of the comet of 1807 were discussed in a very complete manner by Bessel who found its periodic time to be between 1400 and 1900 years, and it will be a curious fact if the true period proves to be only seventy four years.

This great comet also presents a good opportunity for the spectroscopists to examine its chemical nature, and a rare occasion for the study of the physical constitution of comets. No doubt these questions will be well attended to by the astronomers and students of our country.

The question of the formation of a comet's tail, and how the particles of matter are driven out from the nucleus in the direction opposite the sun has not yet been answered in a satisfactory manner, and all the facts that can be gathered from observations of this comet will be extremely valuable. In his discussion of the physical constitution of Halley's comet in its appearance of 1835 Bessel found that a repulsive force from the sun was very decidedly shown by the observations of the tail. Similar results were reached by Professor Pierce of Harvard College, Professor Norton of Yale College and by Dr. Pope in their discussion of the Donati comet of 1858. This is an interesting question and it may have an intimate relation with the theory of a resisting medium in space which has been indicated by the motion of Encke's comet.

We learn that unfortunately the weather at Washington has been unfavorable for several days past; but from the numerous good telescopes scattered over the country, we doubt not that good observations of this interesting comet will be gathered.

THE ADDRESS OF THE PRESIDENT OF THE ROYAL MICROSCOPICAL SOCIETY.

LIONEL S. BEALE, F. R. S.

(Concluded from page 297.)

One may transport oneself in imagination into infinite space, amid the never-ceasing vibrations visible and invisible—"The lucid interspace of world and world, where never creeps a cloud, or moves a wind," and may perhaps all but see combined in one mental image, as they ever course through space, suns and worlds and systems. And although at first the mind is almost lost in the contemplation of the infinite physical vastness presented it, it is nevertheless able to seize in some degree a more than shadowy conception of the exactness and regularity of the eternal movements, and to recognize the never-ceasing operation in the material universe of inflexible, unchanging law.

But he who in imagination can succeed in mentally placing himself amid the atoms in the interatomic spaces of a living particle, will be in the very heart as it were of an infinity of a very different order—infinite movement and change affecting infinitely minute particles, so very near to one another that the matter of one may as it were run into that of the other, and the masses divide and subdivide again. Of all this movement and change of particles how very little of what occurs in a portion of matter not more than the one hundred-thousandth of an inch in diameter can be comprised in one mental image? But beyond all this there is the power of prospective change, acting through years it may be, which is somehow associated with the minute particles of living matter, as well as many complex phenomena of which the mind cannot take cognizance as a whole, but must consider, as it were, one by one in several successive pictures.

Could we peer into the very substance of the living particle itself as it was increasing in size and communicating to non-living matter its wonderful properties, what should we see? What is it that happens at the moment when a little complex organic matter dissolved

in water passes from the non-living to the living state? Should we see atoms being arranged and entering into new combinations according to some physical properties inherent in the very matter—atoms combining according to their chemical affinities; or should we see the complex chemical compounds of the pabulum being changed, their elements being somehow torn asunder from their combinations, or rather quietly separating from one another in obedience to some force or power of which we cannot form any accurate conception? The most extraordinary active atomic movements must be taking place, and in the quietest possible manner. Certainly the phenomena which accompany ordinary chemical decompositions in non-living matter do not occur. No two things in this world can be more dissimilar than man's chemical laboratory and nature's laboratory in this living matter. That the formation of the germ is to be accounted for by the operation of the ordinary forces of matter is one of the most absurd of absurd propositions; but that the idea of such an origin should still be entertained and taught by a physicist or chemist is unaccountable.

There are no actions in non-living matter with which the actions of living matter can with any degree of fairness or accuracy be compared. No argument in essential particulars can be pointed out which would justify the use of the word "analogy" without doing violence to truth and cheating the reason. To maintain the identity of the vital and inorganic forces on the ground of some fancied analogy between vital action and crystallization is most wrong and willfully misleading, for the fallacy has been many times exposed and exploded. Been a crystal and living matter there is not the slightest analogy, for the one can be destroyed and caused to re-form as many times as we like, while the living matter cannot be even dissolved. In the attempt to dissolve it, it dies, and cannot be reproduced.

It is obvious that before particles of living matter pass from the living into the formed state their component atoms, or groups of them, must somehow be made to take up a definite position with respect to one another. Such changes of place as must occur can only be brought about by some peculiar force, property, or power, the action of which is temporary. Seeing that the changes in question can take place only while matter is in the temporary living state—this matter having been detached from matter in the same living condition—the force or power in question must be of an exceptional nature, and of an order different from that to which the ordinary forces or powers of non-living matter belong. This wonderful living power which is postulated causes the atoms or the particles of the matter to take up certain positions favorable to their combination in a certain definite manner. Thus certain substances are formed which have a peculiar chemical composition, and in certain cases special properties and endowments not possessed by substances that can be formed in any other way. It seems to me it would be as unreasonable to maintain that the bricks, or rather the clay of which they are made, or the silica and alumina of the clay, or the properties of the elements entering into the composition of these substances, design, fashion and build the house, as to assert that the formation of living things is due to the physical properties of the materials of which their bodies are composed. Vital power impresses as it were its seal upon the matter—upon the structures of the living organism—and ought surely therefore to be considered as above and superior to the mere stuff that it moulds. Vitality, or vital power, forces, bends, arranges, and fashions just as man himself moulds and fashions the clay he works with, only silently, invisibly, more perfectly, and in a definite and prearranged manner, and without mind or will or ingenuity, or instruments or organs,

Judging from the facts, is it not indeed more probable that the ordinary properties, the attractions, the affinities of mere matter are in suspension rather than in action, while the matter continues to be in the living state? When these properties and affinities come into play, do we not get from the matter that was alive albuminous matters, fat and other things, of known properties and definite composition? But neither these nor any definite compounds existed when the matter was living. They came into being at the moment of its death. The idea of these substances existing in the living matter is inadmissible, for if they were there, some of them could be demonstrated. Such a substance as fatty matter cannot, of course, exist in the living state; fat cannot grow and form fat out of materials which contain the elements of the substance in different states of combination, any more than granite can. If it be conceded that during the living state the ordinary properties and affinities of the matter are suspended, it will be admitted that none of the ordinary properties of material particles can be reasonably credited with the ability to interfere with the exercise of affinities; and therefore it seems reasonable to conclude that some totally different power, *vitality* or *vital power* (which same, unlike the ordinary properties of the matter, is lost or ceases to act when living matter dies), is the true cause of the exceptional state in which the material particles are held while the matter remains living.

But thought may take us yet further. Gradually passing inwards towards the centre, through vast concentric layers of particles, we arrive at last in imagination near the centre of a particle far too minute to be visible, where the atoms of lifeless matter first live. As to the actual nature of this wonderful change which occurs, we are, and from a purely physical point of view must remain, in darkness. For it is certain that the new temporary living state is absolutely district from the non-living state in which the matter existed but an instant before. Before long this will, I doubt not, be generally admitted by those acquainted with the facts and not biassed by previous confessions or beliefs.

It is invariably in living matter devoid of structure and form, that all those wonderful actions of surpassing interest which result in the development of form the most striking and structure the most elaborate, are carried on. Forces or powers, but of a non-material order, transmitted through succeeding particles of the same kind, and continuously operating, it may be upon vast quantities of matter, through centuries or centuries of centuries (millions on millions of years"), are the activities by which the re-arrangement of the elements under certain fixed conditions which eventuate in definite and predetermined form, structure, and composition, is brought about. The changes, conversions, formations in question, at present invisible and undemonstrable, require considerable time for their completion. Compared with the visible phenomena which succeed them, and which may be watched, described and delineated by us, they are slow indeed. During days, weeks and months, in darkness and in silence, arrangements and re-arrangements of the most complex character incessantly and quietly proceed, as we say, in obedience to *laws* (though we do not *know*), ere the first visible traces of the new being can be discerned by the most careful investigation.

Remember that the changes in question affect a mere modicum of matter. A grain, nay, the hundredth, the thousandth part of a grain, and far less than this may at one time constitute the material substance from which springs a tree that in its maturity will comprise tons of matter, every grain of which will be stamped with individuality. Is it not, then, most strange that in these days which surpass all previous time in the passion exhibited by men to see into the nature of things, that attention should be so much absorbed in considerations relating to the mere matter of which a living thing is made, while the study of the forces and powers which have effected the forming and shaping of the material substance is not only almost wholly neglected, but positively discouraged?

And yet these forces or powers fashion the germ and cause it to be like its predecessors, or modify its character and cause it to give rise to forms perhaps not before attained. With what shall these forces of the living world, operating so marvellously upon infinitesimal particles of matter, be compared? The changes have been likened to those which take place in the formation of crystals, which can be dissolved and caused to re-form as often as we choose; to the aggregation of particles of lifeless matter which can be made to separate or aggregate as we will; to machines which are made by us in separate pieces and afterwards put together; and to many other things between which and living particles there is not the faintest resemblance or the slightest analogy. Uninquiring, unthinking persons have been, and are at this time, misled by crude and false comparisons, and deceived by forced and fancied analogies. The coarse materialism of our day ought long ago to have been dismissed with scorn as unworthy of the age in which we live, and of the acceptance of any one who would not disgrace himself by helping to force thought back again to the point it had reached more than two thousand years ago.

No one acquainted with the facts of vital change can doubt that phenomena of the same order as those in operation to-day attended the development of primeval forms of life. For not only do we meet with living matter producing the same structures as existed during early periods, but it is probable that some of the living things now growing and multiplying are identical with some that existed in the very dawn of life-history. Unbroken *continuity, descent, derivation,* in operation through the ages without change in power or property, or alteration in form or composition; *repetition* wi'hout gain or exaltation; *continuous descent* without degradation or improvement; monotonous succession without progression or advancing evolution. Nevertheless, we are expected to accept the dictum that amid these myriads of myriads of similar organisms, here and there one more fortunate or more gifted than the rest—we are not told why, when, or how—became endowed with the marvellous power of endless modification. We are asked to believe that rigid laws uniformly operating with the same consequences, for ages, suddenly changed, and that long-imposed uniformity gave place to capability of differentiation. But if we try to realize what, according to the terms of the hypothesis must have happened in the living matter, into what a sea of fantastic conjecture do we plunge! The new or modified powers must have originated in or emanated from particles in the very centre of minute living spherules. When we consider the minuteness and insignificance as far as the mere matter is concerned, of the living particles we are referring to, many will, I think, be inclined to admit that it is at least as probable that new forms of living matter of this infinitesimal minuteness originated anew, as that forces which had been in operation for ages, under inexorable unchanging laws, were entirely and suddenly changed or removed, and replaced or supplemented by additional and very different forces obeying very different laws.

Moreover, as no direct or positive evidence of a reliable character has yet been obtained in favor of the direct conversion of non-living matter of any kind into a living form, while there is nothing to indicate that the passage from the non-living to the living was effected by gradual change, as has been suggested by some, it is as reasonable to assume that several infinitesimal life-forms with very different powers of development sprang at once into life, though the ultimate form to be assumed was postponed for ages, as that one single living form only was so formed with the power both of endless monotonous repetition, as well as of infinite and never-ceasing capacity of variation and change, one or other of these opposite attributes being accidentally exercised or capriciously taken advantage of by such of the descendants as were assured that they were above all the most fitted to survive.

Doctrines of evolution are, no doubt, an advance upon the direct mechanical formation of fully formed organisms hypothesis; but although some evolutionists have so expressed themselves as to lead us to infer that an idea so absured as the above had been entertained, it need scarcely be said the inference is their own and totally unfounded, suggested by themselves for the satisfaction of ridiculing it and exposing its inferiority to their own hypothesis. No doctrine of evolution yet put forward seems to afford any help to those who are familiar with the characters of the living matter of different organisms, as far as these can be elucidated by any means at present known. Evolutionists generally do not take cognizance of the difficulties which are so patent to microscopical observers. Some of them have hardly condescended to notice the living matter, out of which and by which all the forms of life they profess to account for are developed. It is true that it has been suggested that there are structural differences in the apparently similar manner, which structural differences result in the production of such dissimilar beings; but speculations concerning hypothetical structure are as futile as those which deal with the hypothetical form and properties of the hypothetical inhabitants of Jupiter.

All living matter is, I repeat, structureless, and it is to the power rather than to the mere matter we must look for the explanation of the marvellous differences in the beings evolved by different kinds. The similarity of various embryos of different animals has often been alluded to, and it has been said, for example, that at a certain period of development the embryo of man could not be distinguished from that of a dog. That there is a general rough resemblance is perfectly true, but, on the other hand, any one who examined the minute structure of corresponding tissues and organs, would not find the likeness so great as is supposed, while he would be struck with a great number of points of difference. Not one structure could be found in any part of one embryo which did not exhibit peculiarities by which it could be distinguished. It would, therefore, scientifically be more correct to say that the embryos were *not like one another*, than that *they were like*. But any argument based upon the likeness, if it existed, would not help the evolutionist, inasmuch as the "likeness" is far greater at an earlier stage of existence, before any form or structure whatever has appeared. Every living form comes from an equally structureless material, and the forms near one another in the scale are not more like one another than they are like forms far above or far below them. If, for example, the evolutionist would examine embryonic living matter at a very early period of development, he would discover not only that man and dog were not to be distinguished, but that not one form of living matter could be distinguished from any other form in nature; nay, the living matter which might become dog or man could not be identified by any means at our disposal, or distinguished from that which belonged to amœba or plant, and yet it is put forward as a discovery of recent date that certain properties manifested by the tissues of animals also characterize some of those plants.

But after all, the assumed likeness is but a likeness in certain general points, and those who wish us to draw certain conclusions from their statements, ought to be asked to point out how it is that every cell, every issue of the embryos they regard as being alike or identical, exhibits peculiarities and individual characteristics of its own as regards elementary arrangement, rapidity of formation, rate of growth, duration of existence, and a number of other points. Again, the statements about the changes occurring during development in the lower animals being represented by identical changes occuring during the earlier periods of development in the higher, are correct only when taken in a very rough and general

way. Such phenomena, it is said, show unity of plan, and favor the hypothesis of the descent of jelly-fishes from sponges, and of man from apes. No doubt they do if the mind is already prepared to receive such ideas, Those, however, who really study the operations of nature in her inner recesses where and while she is at work, will certainly often find where identity is affirmed, diversity really exists. Rough general resemblances can no doubt be pointed out, and be made much of, by those who do not look too closely or intently; but those who examine minutely and patiently will find that in very many cases the general resemblances will be outnumbered and outweighed by specific irreconcilable differences and individual peculiarities.

If then we examine living matter in that early period of development ere any structural peculiarities whatever have been manifested, we shall be face to face with the problem of life. For it is at this time, when the matter is without form, that the dispositions of the material particles, which at length result in the development of form, are made. Preparation is made for the division of the mass of the living matter into several portions, and for the orderly disposition of these in respect to one another, as well as in respect of the new masses which at some future time are to be detached from them. Throughout the whole period of the life of many organisms, similar wonderful changes are continually taking place, at least as respects the living matter of certain parts and organs; but we have no means of distinguishing the living matter which continues monotonously repeating similar changes, from living matter which divides and subdivides into masses, which in turn gives rise to successive generations of living particles, which may differ from one another and from all that have gone before, in *power*.

As far as I am aware, no form of the doctrine of evolution yet enunciated takes into account the phenomena of the living matter in which and by which all the changes recognized and professed to be explained are carried on. And yet it is only by these actions in living matter that evolution can be made to appear a plausible hypothesis. Only by carrying out very careful investigations on this formless, structureless living matter can we reasonably hope to obtain anything approaching an accurate conception of the wonderful working of real living nature. It seems to me that the "nature" of the evolutionist is but a fanciful and highly colored picture in which ideas suggested by investigations in stockyards and shambles are depicted, with the addition of the horrible scenes assumed by a vivid imagination to be enacted in the supposed everlasting fight for existence and scramble for mastery, in which conquerors are always being conquered by creatures just a shade more fitted to survive than themselves. Here is creation by destruction in a never-ceasing scramble going on for millions on millions of years, in which the only thing certain seems to be that the greatest misery is assured to the greatest number; life succeeding life, without good or reason or joy or hope; peaceful nature a continual massacre of experimental forms to be massacred in their turn, and these by more; a constant struggle to survive, in which success is rewarded by extermination. The "nature" of evolutionists is a very strange nature indeed, in which oppression, destruction, and tyranny seem to be the chief agents in creation and formation, development and advancement.

But besides the evolution of living forms and of the different organs, we are to believe in an evolution of matter, an evolution of worlds, of suns, of systems. Religion, law, and justice, art, science, and even thought are all products of this universal, never-ending evolution. But what is evolution, and who has given to the term an accurate definition? We shall be told there is evolution and evolution. One man's evolution goes too far, another's not far enough, and there is no general agreement as to what is meant by evolution, and whether the use of the term should be restricted to the living world or extended to the universe—though it must be obvious to any one who considers the question that the evolution of a living form and the evolution of the matter of a stone are as far removed from one another as are the question of the nature and scope of Infinite Power and the nature and properties of a gas or a metal.

Herbert Spencer has defined his "evolution" to be a change from an indefinite incoherent homogeneity to a definite coherent heterogeneity, through differentiations and integrations. But is not every one of these polysyllabic words as elastic as the word the meaning of which they are to explain? Every assertion made is wanting in proof, and most of the words may be used in totally different and even in opposite senses.

Any one who ventures to express a doubt concerning the absolute correctness of the assemblage of vague and even contradictory conjectures comprised in any hypothesis of evolution, is in danger of being abused and called names. He may be denounced to the world as a contemptible person who has made a vile and abusive attack upon some infallible authority who affirms himself to be the real discoverer of all the secrets of all the molecular machinery of creation. We now live under the most ridiculous of all forms of despotism. It has been said that we must accept such and such views or be debarred from accepting anything! But is it possible for any unbiassed person to accept implicitly doubts, vague suggestions of what may be, or can be, or might be—speculations, hypotheses, conjectures concerning things that lived under conditions which are in great part only conjectural? Probably no living person accepts as it stands 'The Origin of Species,' and it is doubtful whether the first chapter, or even the first sentence of the first chapter, would hold its ground without considerable alteration and qualification if subject to searching critical examination.

The facts known to microscopical observers in connection with the act of living of the smallest particle of the simplest forms of living matter are no more to be accounted for by any of the extravagant crotchets lately advanced as explanations of the facts, than are the general broad phenomena of nature which are under the observation of all. Evolution is a wholly satisfactory explanation only to those whose minds have been trained to submission to evolutional authority, and who have brought themselves to regard things as they have been told they ought to regard them, instead of venturing to use their senses, and reasoningt on the facts presented to their observation—and indeed see for themselves with their own eyes instead of accepting, without ever seeing, what they are told has been seen by eyes which are supposed to be specially privileged to see.

As evidence of the nonsense often advanced in favor of some form of evolution, let me quote a few sentences from an article on "Butterfly Psychology," published in the *St. James's Gazette*. Like most advocates of evolution, the writer has the knack of telling his story in such a pleasant way as to make people imagine that he is explaining the nature and cause of things he describes, while in truth he is doing nothing of the kind. He explains nothing at all, but merely announces astounding assumptious based upon conjectures of his own, or of others.

"In early life the future butterfly emerges from the egg as a caterpillar. At once his many legs begin to move, and the caterpillar moves forward by their motion. But the mechanism which set them moving was the nervous system, with its ganglia working the separate legs of each segment. This movement is probably quite as automatic as the act of sucking in the new-born infant. The caterpillar walks, it knows not why, but simply because it has to walk. When it reaches a fit place for feeding, which differs according to the nature of the particular larva, it feeds automatically. Certain special external stimulants of sight, smell, or touch set up the appropri-

ate actions in the mandibles, just as contact of the lips with an external body sets up sucking in the infant. All these movements depend upon what we call instinct—that is to say, organic habits registered in the nervous system of the race. They have arisen by natural selection alone, because those insects which duly performed them survived, and those which did not duly perform them died out. After a considerable span of life spent in feeding and walking about in search of more food, the caterpillar one day found itself compelled by an inner monitor to alter its habits. Why, it knew not; but, just as a tired child sinks into a sleep, the gorged and full-fed caterpillar sank peacefully into a dormant state."

Of course all this may have been written in joke. The writer may possibly be laughing at evolutionists. The "inward monitor" of the "gorged and full-fed caterpillar" undoubtedly looks rather suspicious, but one hardly likes to hint at anything so serious. Evolutionists will, I dare say, repudiate such "evolution" as a mere travesty, but it is quite time that half-a-dozen evolutionists who agree on main points should clearly state their belief.

In conclusion, let me ask you as students of nature's processes, whether you have not seen enough to convince you that the revival of the assumption which has been abandoned and reintroduced many times during the last few centuries, that the lifeless is the sole origin of the living—that in fact the non-living and the living are one—is now unjustifiable, and cannot be reasonably entertained. This monstrous fallacy, though taught with the greatest confidence, is based on assumption, and is supported by arbitrarily selected facts, and by not a few misrepresentations and dogmatic assertions. Whenever any form of this false doctrine has been successfully forced into popularity, it has led to the adoption and propagation of the most grievous errors and grotesque conceits.

COMET OBSERVATIONS AT PRINCETON.

The weather has been so unfavorable at Princeton, that we have been unable to make any very satisfactory measures upon the spectrum of the comet. On Saturday evening the comet was visible fairly for an hour or so, before it descended into a bank of cloud. On Sunday evening it was beautifully seen for about half an hour, and then was obscured by a fog which still continues.

The spectrum of the nucleus is very bright. It is apparently continuous, though there may be a little special emphasis at the points where the usual carbon lines ought to appear. The spectrum of the coma and of the tail is precisely like that of most comets, showing three bands which coincide sensibly with those given by the flame of a Bunsen gas-burner, presumably due to a hydrocarbon of some sort.

On Saturday evening the nucleus looked much like a star-fish, having five projecting points formed by jets of light protruding from the central globe to a distance of from four to ten seconds of arc. These jets were not equal in length or brightness, and were not symmetrically disposed with reference to the axis of the comet's tail. Two of them were somewhat curved, they were all diffuse and blunt at the extremity, rather than pointed.

On Saturday, instead of jets, the nucleus had a nearly circular envelope surrounding it, sharply defined from the coma. Its diameter was perhaps 20″, but the fog came on before any measures could be made. This disc of light, surrounding the nucleus, was not uniformly bright:—it was more brilliant on the side next the Sun, and there was a curious dark opening in it of oval form, some 20° one side of the axes of the tail. We were preparing to study the spectrum of this envelope critically, when we were cut off by the mist.

Although the Comet is now receding from both Sun and Earth, it is rising so much higher in the Northern Sky each night, that if the weather becomes favorable, it may yet be possible to get something more satisfactory; but just at present the rain is pouring and the prospect is rather dreary. C. A. YOUNG.

PRINCETON, N. J., *June* 27, 1881.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

LOCUSTS AND SUN SPOTS.

To the Editor of "SCIENCE:"

SIR: Perhaps you will permit me to explain one inapposite word occurring in my communication on the above subject.

When I stated that European migrants come north and east, I should rather have said *north and west,* the set of the migrations, as far as known, is on European areas north and west; and in this direction, butterflies, sphinx moths and locusts, whose point of departure has been traced to Southern Asia or Northern Africa, travel periodically; the occurrence being made known to us by their vanguard, so to speak, sweeping over the eastern shore of Great Britain. That this track is not voluntarily chosen by instinct, but rather due to a prevailing south-easterly direction of the winds, rests now-a-days on a great amount of experience. A. H. SWINTON.

GUILDFORD, ENG., *June*, 1881.

THE BLUE COLOR OF THE SKY.

Prof. Cornu having established the fact that the atmosphere of the earth exercises an energetic absorption upon the ultra-violet rays of the spectrum, whose limit varies according to the statement of the atmosphere and the altitude of the sun. Prof. Hartley sought to attribute this limitation to the influence of ozone. His experiments have demonstrated.

1.—That the ozone is a normal constituent of the higher atmosphere, where it is more abundant than on the earth.

2.—That this quantity of atmospheric ozone suffices to limit the spectrum in the ultra-violet region, without considering the absorption caused by the great density of the oxygen and nitrogen.

3.—That the blue tint of the atmosphere is due to the presence of ozone.

In respect of this last point, Prof. Hartley remarks that, if the ozone exists in the high regions of the atmosphere, the light reflected by clouds at a great heighth has a blue appearance because it traverses a gas of this color. It is so likewise with the light illuminating the distant portions of a landscape. Experiments have shown that 25 milligrammes of ozone for every square centimeter of a layer of 80 kilogr. thick can produce this phenomenon.

WE learn that Prof. H, S. Prichett, Director of the Glasgow Observatory, has been appointed Professor of Mathematics in Washington University, St. Louis, Missouri.

DOLBEAR'S NEW TELEPHONE SYSTEM.

Among the exhibits at the forthcoming International Electrical Exhibition at Paris, the new telephone we are about to describe will command attention as an original and important invention.

It embodies the most recent discoveries of Professor A. E. Dolbear, of Tuft's College, Massachusetts, who, as one of our most esteemed contributors, needs no introduction to the readers of "SCIENCE."

The advantages claimed by Professor Dolbear may be summarized as follows:

1. It is a *new* and independent system which has important advantages over the Bell and other Telephonic methods.
2. Its capability of transmitting speech over longer lines of wires than has been hitherto employed, and its freedom from the troubles of induction.
3. It is a silent instrument, the words coming out clear without the sputtering aud confused noises of the old system.
4. It is an absolute departure from the Bell system, and its principles of operation entirely independent.

We are enabled to place before our readers a description of this original telephone prepared by Professor Dolbear himself, illustrated by some excellent cuts loaned to us by Mr. H. C. Buck, who is leaving for Paris to represent Professor Dolbear at the forthcoming Electrical Exhibition.

Before describing Professor Dolbear's Telephone in detail, we may state that in order to receive messages by the Bell system it is necessary to use between the ear and the line wire an electrical machine, consisting of a magnet, a magneto-coil to influence the magnet, which coil is connected with the line wire and with the ground. Take out this machine, and we take out the Bell telephone system—this is substantially what Professor Dolbear claims to do—for to receive a message, he takes out the machine, and puts the end of the telegraph wire directly to the ear. For convenience of ordinary use Professor Dolbear provides the receiving end of his telegraph wire with a small handle, in which he arranges a couple of thin diaphragms, one of them attached to the wire—a contrivance that improves the vocal delivery of the line wire.

Professor Dolbear thus describes his invention:

RECEIVER.

This consists, in its simplest form, of two metallic disks about two inches in diameter, so mounted as not to be in metallic contact, and this is effected by turning a flange in a hard rubber case so they may be kept apart by it (see Fig. 1). A cap is screwed down upon each plate; one of them having a small hole in the middle of it to listen at; the other is a larger one, having a knob turned upon it for conveniently holding it in the hand. Through the middle of the knob a screw is sunk which touches the back plate and serves to adjust it to the best position relative to the front or vibrating plate. The back plate is thus fastened at both edge and middle, which prevents it from vibrating, while the front plate is only fast at its edge, leaving the middle free to vibrate.

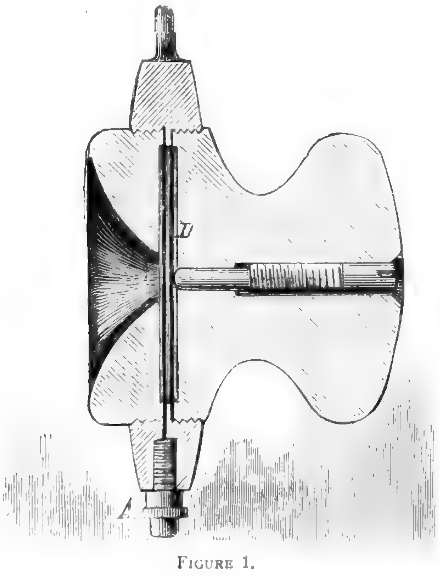

FIGURE 1.

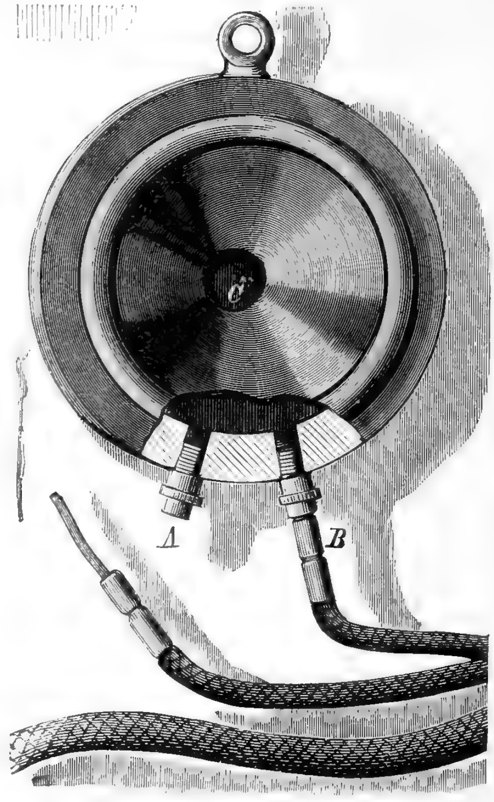

FIGURE 2.

Each of these plates, A B, Fig. 2, is in metallic connection with the induction coil so as to be its terminals. When thus connected and one makes and breaks connection in the primary circuit, a click may be heard by one holding the receiver near to the ear. If a Helmholtz interrupter be employed to make and break the primary circuit, the pitch of the fork can easily be heard, and with a *Reiss transmitter* or other suitable one in the same place, any kind of a sound will be reproduced.

The explanation of this is easily understood from the foregoing description of the conditions present. The electromotive force generated by induction in the coil changes the two terminals in the receiver, one positively, the other negatively; they therefore attract each other.

One of them is free to move, while the other is rigid. The middle of the freer plate consequently moves slightly toward the other whenever they are electrified, and *in so doing spends the energy of the electricity*, while its elasticity brings it back to its place. It is not essential, however, that both of these terminal plates should be connected to the induction coil, for if only one is connected, the recurring charges will cause the free plate to vibrate, for a charged body will attract any other body, so if the connection be to the back plate it will attract the front one and make it move, and if the connection be to the front plate it will attract the back plate and approach it. The effect will be increased by putting the finger upon

the terminal that is free ; not because it *makes a ground*, as it is termed in electrical science, or completes an electrical circuit, for if the individual listening be as perfectly insulated as glass or hard rubber can make him, the sound is as loud as if he stood on the ground ; but the individual becomes electrified by induction, it is the same as enlarging the terminal would be. Consequently receivers are

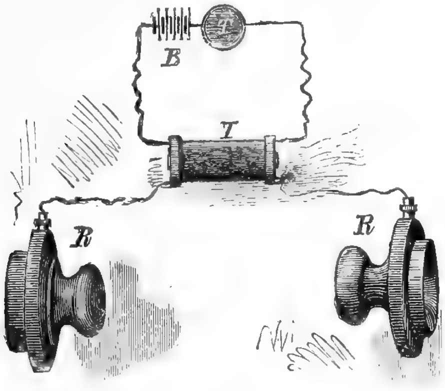

FIGURE 3.

made having only one wire terminal (see Fig. 3), the other plate being connected by a conductor to a metallic ring upon the knob, and this receiver is as efficient as the other.

Electricians will recognize in this structure what is technically known as the *air condenser*, and the mutual attraction of the two plates has been employed as a means of measuring electrical potential. In this case one of the plates is suspended from one arm of a balance, while the other is fixed underneath it at a short distance. The attraction of the plates when they are electrified requires an extra weight to keep them apart, and the weight needed is the measure of the attractive force. But the plates will attract each other when glass or mica or any other non-conducting substance is placed between them in the place of the air ; and one might expect that if such an air condenser would give sonorous results, other forms of condensers, would do so likewise, and this is so. Indeed, whoever has charged a Leyden jar has probably noticed the sounds coming from it when it is nearly saturated. In 1863 Sir Wm. Thompson had his attention directed to the sounds produced by discharge in an air condenser.*

When the two plates of Epinus's condenser are in metallic contact no sounds whatever can be produced by it, but if they are separated by a thin film of air they will

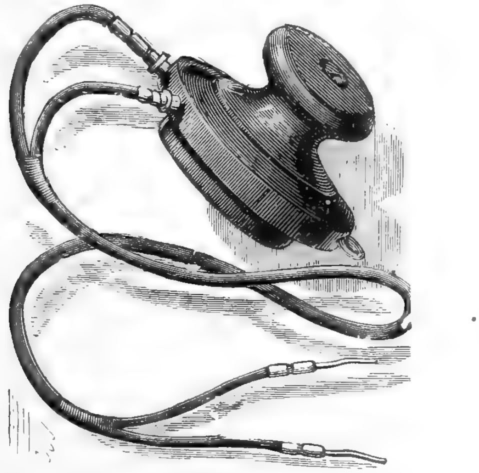

FIGURE 4.

reproduce speech (see Fig. 6, at E). In the first case the electricity passes from one plate to the other without doing work or changing its form ; while in the latter, its form is changed and work is done, and between the best conductors, such as silver and copper and the perfect non-conductor air there are all degrees of conductibility, and whenever electricity spends its energy upon an imperfect conductor it results in heating it ; that is, in molecular and atomic vibrations. Consequently an undulatory current from an ordinary transmitter, when sent through an imperfect conductor, will set up sound vibrations in it which may be appreciated by the ear. Let, then, any poor conductor, like a disk of carbon, a sheet of paper or of gelatin, or such chemical substances as ammonium chloride, be placed between the terminal plates, and an undulatory current sent through them will result in sound, and speech may be reproduced.

Now, the phenomena observed in Geissler's tubes and Crooke's tubes show that the residual gaseous molecules are violently impelled from the electrified terminals, not simply because they are electrified, but because they are heated, for the same phenomena are witnessed when the terminals are heated in other ways ; so it is probable that between the plates of the air condenser there is an actual impulsion of the air particles from one to the other, and that the phenomenon of attraction is not isolated from molecular impact. Receivers have been made in which a vacuum could be produced between the plates, but no great difference could be observed in their performance ; and when one reflects upon the immense number of

FIGURE 5.

molecules left in the best vacuum yet produced, it is not a matter for much surprise.

When a non-conductor, such as air, or vulcanite, or mica, separates the two plates, there is a complete transformation of the electricity at the limiting surfaces, and with small condensers the efficiency depends upon the electromotive force employed. For low electromotive forces, such as common batteries of a few cells can give, the effect is almost inappreciable, and for this reason such a receiver as this is quite free from the disturbance known as induction, and which is so troublesome in the magneto-telephone, such induced currents being generally of low electromotive force.

Among the earliest of my experiments, made while developing this method, was to attach one terminal wire from an induction coil to the outer coating of a Leyden jar, taking the other wire from the coil in one hand, and

* See papers on Electro-Statics and Magnetism, page 236.

applying one ear to the knob of the jar. Every word spoken at the transmitter was distinctly heard, but the prickly sensation due to the electricity was too disagreeable. Another receiver, not less curious than the Leyden jar, was found in the pair of insulating handles made for the medical application of electricity. When these were connected to the coil wires, and one held in each hand by the wooden part, while the metallic ends were placed at the ears, any kind of a sound at the transmitter was heard without any difficulty, but of course the same sensation was felt as with the jar. Many forms of condensers have been employed with capacities too small to measure up to two micro-farads, and these in all sorts of relations, charging the plates from batteries, from Holtz machines, charging the line as in cable works, etc., all of which give results that differ only in degree.

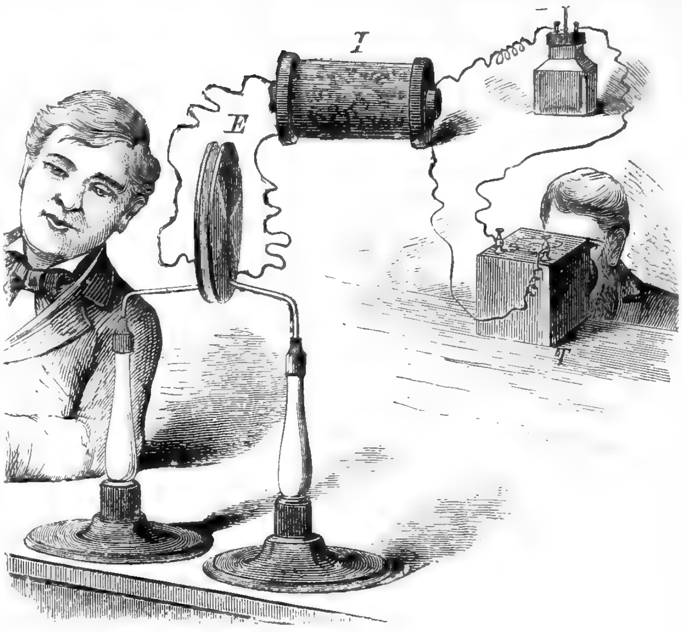

FIGURE 6.

THE TRANSMITTER.

As with other systems in common use, there is a transmitter as well as a receiver. One form of the transmitter is attached to the door of a box containing battery and coil. This transmitter is substantially the same as the one invented by Reiss in 1861. His consisted of a cubical box (see Fig. 6) about five inches on a side, having an opening on one side to talk into, and another one on top, across which the diaphragm was fastened. A pin of platinum was glued to the middle of the membrane, and connected by a wire to a binding screw. A V-shaped wire with platinum point touched upon the platinum of the membrane, and with its binding screw served to complete a galvanic circuit. This one (see Fig. 10) differs from this of Reiss only in making the chamber smaller, making the connecting wire on top T-shaped, and substituting carbon or other suitable substance for the platinum; but the platinum does very well. It is a matter of some surprise that the old transmitter is still spoken of as a *make and break* circuit, and that it can only transmit pitch, whereas, whether it breaks or not when a sound is made in it depends solely upon the intensity of that sound, just as with the Blake transmitter, if one talks gently to the original Reiss transmitter, it not only does not break, but it transmits speech with all its qualities.

Accompanying the transmitter an induction coil is shown at I, Fig. 6, and as the working of the receiver depends upon electromotive force and not upon current, it is necessary, if a coil be used to raise the electromotive force, to have one with many more turns than is needed with the magneto receiver, and the best results have been obtained with a coil having a resistance of four or five thousand ohms, but it is probable that this will be reduced.

On account of the high electromotive force a better insulation is needed than ordinary telegraph lines give, when the induction coil is at the further end of the line, but if it is at the receiving end, and a low electromotive force is employed in the primary, then ordinary insulation will answer. Again, the electromotive force being high, inserted resistances do not so markedly decrease the efficiency of the instrument, as in the case with the magneto-telphone. For instance, the articulation is perfect and loud enough with a resistance of fifty thousand ohms, a resistance equal to five thousand miles of common telegraph wire, and it may be heard through a resistance of a million ohms, practically an infinite resistance.

If one of the terminals of a receiver be charged in any way, the reaction between the two plates will be stronger than it will be without. Let, then, one terminal be attached to a knob of a Holtz machine that is kept charged by rota-

FIGURE 7.

tion. The sounds will be heard much louder, and any other source of electricity with high potential will answer the same purpose. Hence a battery of a large number of cells may be substituted for the Holtz machine, and one of the terminals of the battery may go to the ground, though this is not essential. This arrangement will keep the terminal plate charged to the potential due to the chemical relations and number of cells in the battery. If the battery be placed in the line wire it will keep both ends of the line charged. A Volta's pile may be substituted for the battery in either place, and so may a charged condenser of any capacity, the electrically

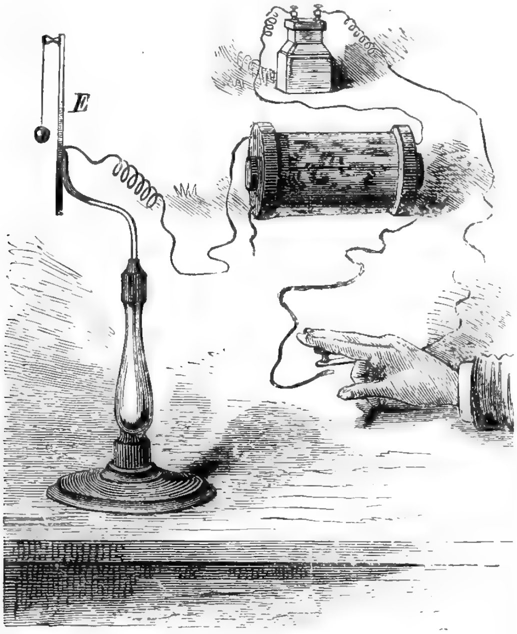

FIGURE 8.

charged terminals in this system acting in a way analogous to the permanent magnets in the magnetic system.

There are various other ways of employing condensers, and as one would infer from the preceding descriptions of the phenomena, these condensers will talk, that is, they will reproduce in sound the varying electrical conditions to which that may be subjected, as will also either a battery or a Volta's pile.

I have often heard them talk, and have made many experiments with such receivers.

By this system telephonic communication can be secured through ordinary medical electrodes.

In perfecting this new telephone Professor Dolbear has given long and constant study to the scientific problems involved, while the mechanical construction has been prosecuted by Mr. H. C. Buck, aided by skilled machinists and competent assistants. The above concise description in the inventor's own words will give our readers a clear understanding of the principles that underlie his interesting invention, and it only remains for us to describe in brief the several figures in our front page engraving.

Fig. 7 shows the telephone in actual use, the transmitter being secured to the wall, the battery and induction coil being placed in a box on the floor, or in a convenient closet. Fig. 4 is a perspective view of the new receiver; Fig. 2 a face view of the same, with a portion of the casing broken away to show the connection of the two binding posts, A B, with the diaphragms, C D, and the adjusting screws by which the distance between the diaphragms is regulated are shown in the sectional view Fig. 1.

Fig. 8 illustrates the principle of electrical attraction upon which the action of the new receiver is based; the electrostatic charge received by the plate, E, from the induction coil attracts the pith ball suspended in front of the plate.

Fig. 6 shows the two plates, E, of an Epinus condenser, placed near together and connected with the terminals of the secondary wire of the induction coil, I, and used as a telephone receiver.

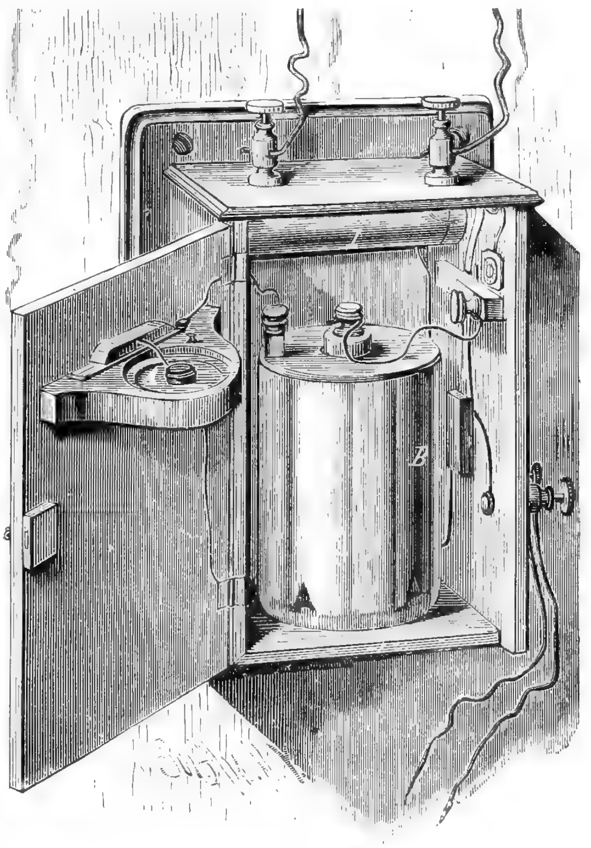

FIGURE 9.

Fig. 5 illustrates the essential features of the new telephonic system. I being the induction coil whose primary is in circuit with the battery, B, and transmitter, T, the receivers, R, are each connected with a single terminal of the secondary wire of the coil, I.

Fig. 9 shows Professor Dolbear's experimental telephone transmitter. In this instrument the diaphragm, A, is horizontal, and carries a carbon electrode, upon which rests a moveable carbon electrode connected by an arm with a delicately pivoted bar supported by the diaphragm cell. The local circuit is from the battery, B, through the carbon electrodes, and through the primary of the induction coil, I.

EXTRACTION OF SILVER.—To extract the silver from silvered objects, these should be plunged into a bath composed of a mixture of 100 grammes of finely pulverised saltpetre and 1000 grammes of sulphuric acid. If the acid is weak, the copper and the other metals except the silver will be attacked; if the acid is concentrated the silver alone will be dissolved.

UPON A MODIFICATION OF WHEATSTONE'S MICROPHONE, AND ITS ADAPTABILITY TO RADIOPHONIC RESEARCHES.*

By Alex. Graham Bell.

In August, 1880, I directed attention to the fact that thin discs or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm.[1] Shortly afterward, Lord Raleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating.[2] This explanation has recently been called in question by Mr. Preece,[3] who has expressed the opinion that, although vibrations may be produced in the discs by the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him the aerial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm—every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Raleigh on account of the failure of experiments undertaken to test the theory.

He was thus forced, by the supposed insufficiency of the explanation, to seek in some other direction the cause of the phenomenon observed, and, as a consequence, he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece, were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read

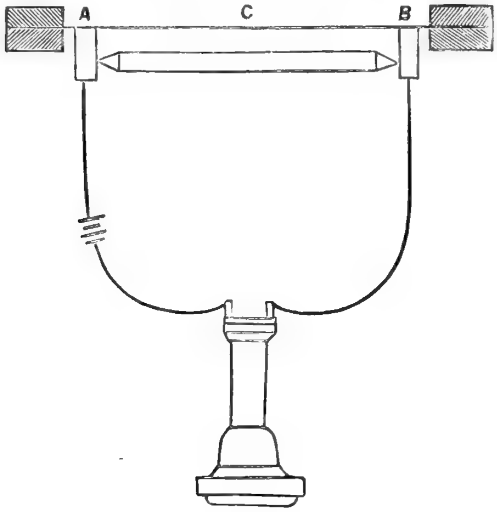

Fig. 1.

before the National Academy of Science[4] that audible sounds result from the expansion and contraction of the material exposed to the beam, and that a real to and fro vibration of the diaphragm occurs, capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations, that were so easily obseerved in our experiments, might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone shown in Fig. 1, and that the vibrating area was confined to the central portion of the disc. Under such circumstances it might easily happen that both the supports, *a b*, of the microphone might touch portions of the diaphragm which were practically at rest. It would, of course, be interesting to ascertain whether any such localization of the vibration as that supposed really occurred, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated.

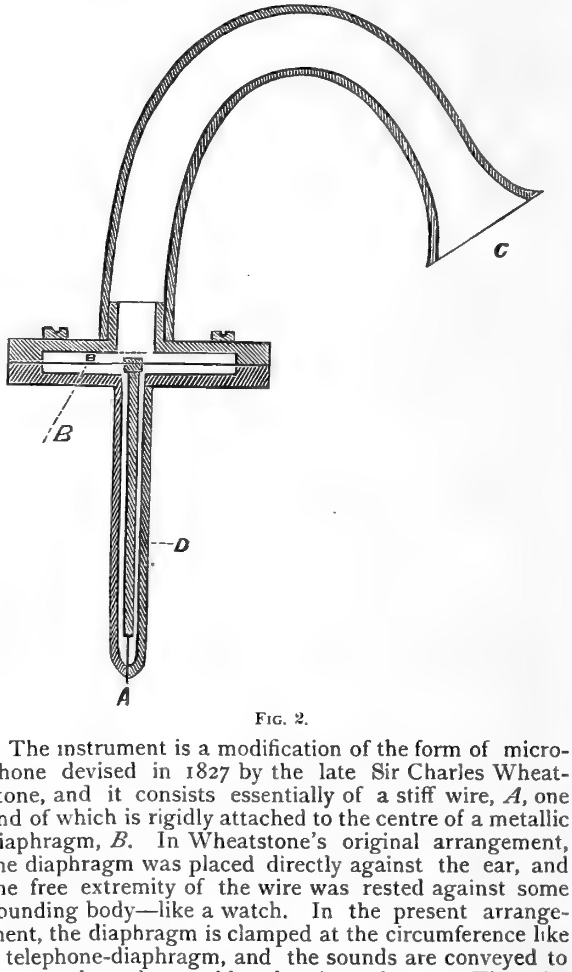

Fig. 2.

The instrument is a modification of the form of microphone devised in 1827 by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire, *A*, one end of which is rigidly attached to the centre of a metallic diaphragm, *B*. In Wheatstone's original arrangement, the diaphragm was placed directly against the ear, and the free extremity of the wire was rested against some sounding body—like a watch. In the present arrangement, the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are conveyed to the ear through a rubber hearing-tube, *c*. The wire passes through the perforated handle, *D*, and is exposed only at the extremity. When the point *A* was rested against the centre of a diaphragm upon which was focussed an intermittent beam of sunlight, a clear, musical tone was perceived by applying the ear to the hearing-tube *c*. The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area and in the corresponding area on the other side of the diaphragm. Outside of this area on both sides of the diaphragm, the sounds became weaker and weaker until, at a certain distance from the centre, they could no longer be perceived.

At the points where we would naturally place the supports of a Hughes' microphone (see Fig. 1) no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may, therefore, be reconciled with the positive results obtained in America by Mr. Tainter and myself, A still more curious demonstration of localization of

* A paper read before the Philosophical Society of Washington, D. C. June 11, 1881.
[1] American Association for Advancement of Science, August 27, 1880.
[2] Nature, vol. xxiii., p. 274.
[3] Royal Society, March 10, 1881.
[4] April 21, 1881

vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram), and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area and for a short distance outside, but not in other parts.

In this experiment, as in the case of the thin diaphragm, absolute contact between the point of the microphone and the surface explored was necessary in order to obtain audible effects. Now, I do not mean to deny that sound waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Raleigh actually occurs, and that it is sufficient to account for the audible effects observed.

ASTRONOMY.

On the 23rd ultimo, Mr. E. L. Larkin, a subscriber and contributor to this journal, telegraphed to Professor Swift, of Rochester, the discovery of a comet in the constellation of Auriga; but as others have since made the same claim, the priority of discovery awaits confirmation by those who dispense the pecuniary reward offered by Mr. Warner for all comets discovered during the present year.

We reserve until next week our report on this interesting celestial object, by which time our correspondents will have worked out the results of their observations, which have been delayed by atmospheric and other difficulties. The comet is now plainly visible, and American astronomers are on the alert to thoroughly examine it with all the appliances which modern science has placed at their command. At the date of our writing nothing reliable has been determined by actual observations, but some interesting facts, based on preliminary and partial observations, have been communicated, which, if accepted with reserve, pending final results, may be found useful to those directing their attention to the comet.

Professor Henry Draper is said to have made several successful photographs of the erratic stranger. Professor C. A. Young, of Princeton, has examined its spectrum, and reports that that of the nucleus was continuous, while that of the coma was sensibly coincident with the spectrum of the Bunsen burner flame. As seen directly in the 9½ inch equatorial, with eye-pieces of the lowest power, on the evening of the 26th, the nucleus was small and bright, with five bright jets of unequal length projecting from it a short distance. The tail showed three maxima of brightness, of which the brightest was near the axis, and was quite convex in the direction of increasing right ascension. On the 26th he states the spectrum was about the same, but the nucleus, instead of showing jets as before, was nearly surrounded by an envelope.

Professor Boss of the Dudley Observatory determines the diameter of the nucleus to be seven seconds or 1 500 miles, at an estimated distance of 45,000,000 of miles.

Professor Asaph Hall considers it most probable that the comet is identical with that discovered by Professor B. A. Gould at Buenos Ayres of the 1st of June. On the 26th ultimo an observation was made at the naval observatory, Washington, which indicated "the position of the comet at its lowest culmination, obtained with the transit circle, was at 11 h. 27. P. M., Right ascension 5 h., 48 m., 384-100 s., North declination, 57 deg., 40 m., 52 sec.

THE LUNAR ECLIPSE.—The eclipse of the moon on June 11 was seen under favorable conditions at the Naval Observatory, Washington. The only observations of importance were observations of occultation of B. A. C. 5862, and two faint stars during the eclipse.

THE OHM.

A British Association committee has been reappointed for the remeasurement of the Ohm, and of other units. It is not to their work, however, that we wish now to draw attention, but rather to a good stroke in the right direction, done in the Cavendish laboratory by Lord Rayleigh with the assistance of Dr. Schuster and others. The old British Association apparatus has been fitted up again, with such improvements as the criticism of nearly twenty years has suggested. It will be remembered that this is the only method in which the measurement of transient currents by ballistic galvanometers is not employed. A circular coil of insulated wire forming a closed circuit rotates about a vertical axis, and the electrical current induced in it by the earth's magnetism gives a steady deflection to a magnetic needle at its centre. The manifold precautions, calculations and corrections which have to be entered into by the experimenters are given by Professor Fleeming Jenkin and others. One important correction is that which is due to the self-induction of the coil which retards the current, and a most important fact has been brought to light by Lord Rayleigh, namely, that this self-induction is considerably greater than it was thought to be by the original committee. Professor Rowland, assuming that an unknown error existed proportional to the square of the speed of the rotation, has found that the original experiments of the committee lead to the result that the Ohm is 0.74 per cent. smaller than it was intended to be, and his own experiments lead to its being 0.89 smaller. Kohlrausch found it nearly 2 per cent. too great, and Weber thought it correct. The Cavendish laboratory experiments lead to its being 1.05 per cent. too small, and the elaborate paper to the Royal Society in which this result is given promises a redetermination with new apparatus on the same principle. In making the present determination a new method of suspension of the needle, a stroboscopic method of measurement of the speed—the old governor and the tinkling bell being discarded—and driving the coil by means of a water turbine instead of by hand, are some of the improvements which have been introduced.

It is to be remembered that no re-measurement of the Ohm can ever effect our use of it as a standard. It is no longer to be regarded as exactly equal to one thousand million C. G. S. units, but this is of no more consequence than the fact that one gramme is no longer regarded as being exactly equal to the mass of a cubic centimetre of water at 4° C.—*The Electrician.*

ALCOHOL IN WATER AND AIR.

An interesting discovery has been brought before the Academy of Sciences by M. Muntz, Chief of the laboratories belonging to the Agricultural Institute. He has found that alcohol is distributed throughout the universe, in the sun, air, water of the ocean and streams. It is a known fact that fermentation is a general phenomena in air, water and earth; This fermentation gives off CO^2, and as a necessary consequence, alcohol. This is what the experiments of M. Muntz have demonstrated; he has been able to prove the presence of alcohol in water, etc., by reducing the alcohol to an *iodoform* state by means of iodate and carbonate of soda. The precipitate which is obtained even in the presence of a millionth quantity of alcohol, affects the crystalline form of the snow examined under the microscope. The alcohol is produced in earth containing organic matter in decomposition, and hence it extends into the waters of streams, and into the atmosphere. Still, the portions are so infinitesimal that a water-drinker will never feel himself "alcoholized;" the dose of alcohol contained in a cubic metre of water (1000 litres), being at the most a gramme.—

ELASTIC RESTORATION OF CAOUTCHOUC.—Objects made of this substance easily lose their elasticity. Dr. Pol, however, avers that their elasticity may be restored by plunging them for an hour into a mixture composed of 2 parts of water and 1 part of ordinary ammoniac.

THE TELEPHONIC RECEIVER.

Mr. Preece has presented to the Royal Society the result of his investigations upon radiophony. They relate to the phenomena produced by the action of intermittent rays upon discs and vases of different substances. Confirming and pursuing the investigations of Mercadier in France, and of Tyndall, he has come to the conclusion that the sounds produced under these conditions are due to calorific effects, and not to light.

Caoutchouc and ebionite (hardened caoutchouc), are absolutely opaque, but they act as diathermics or tranparencies for calorific rays; the radiating heat can act through a screen of these materials.

It has been proved by delicate experiments, that six vibrations or more can be produced during a second, by the intermittent action of the heat, producing a dilitation of the disc's mass. The phenomena, therefore, produced by Bell and Tainter are not due to an absorption of heat, changing the volume of the affected substance.

Mr. Preece made use of a specially constructed chamber and is convinced that the sounds are produced by the contained air, and not by the discs or surfaces of the chamber. In it is placed a mechanism which recalls that by which the "moulinet" of Crook's radiometre is moved under the influence of the heat.

He has proved, finally, that the absorbing for the heat of the gas, contained in the chamber experimented on, influences the production of the sounds. These experiments have been repeated upon bottles blackened with camphor smoke, both on their exterior and interior surfaces.

Mr. Preece has thus been led to think that a wire of platinum, traversed by an intermittent current, can become a source capable of producing on suitable walls calorific rays, which have the power to cause sounds through the heating of the gas on contact with these walls.

The experiment was crowned with success; it was made at first by sending currents into a spiral of platinum by means of a stop-wheel turned with the hand, and when a good microphone was substituted a reproduction of the wood was effected.

Thus has been realized a receptive telephone founded upon an entirely new principle.—(*La Nature*).

ALTERATION OF MILK.

M. Fauvel, in the capacity of chemist to the municipal laboratory of Paris, has discovered that the milk employed for babes often undergoes an alteration which has hitherto been unsuspected. In his investigations he has noted the presence of cryptogamic vegetations. These are found in the tubes of glass and caoutchouc, which enter into the construction of the small feeding apparatus, especially so in the swelling of the rubber which the infant sucks. The new microphite can be easily cultivated in whey, and the author has thus observed the various stages of its development. This, however, is but the first of M. Fauvel's intended investigations. This discovery is important from a hygienic point of view. These observations were confirmed by the fact that twenty-eight out of thirty-one cases presented these symptoms. (*La Nature*.)

NOTES.

FIRE BALLS.—There are many persons who persist in their statement that fire-balls exist only in imagination; but here is the authentic statement of Henry O. Forbes, who, in a letter to *Nature* thus describes the phenomenon.

"I was standing in a window on the second floor of the Hôtel Braganza (in Lisbon), which stands close to and high above the Tagus, and had an unbroken view of the river. There occurred a flash followed by an instantaneous crash, but the tail of the flash, however, gave origin to two balls, which descended separately and not far apart, towards the river, and when quite close to, or in contact with the water, burst in rapid sequence, with explosions which might have been the crack of doom."

PHOSPHORESCENCE.—Mr. W. Crookes, after submitting the action of precipitated aluminum to the action of electric discharges in a Geissler tube, announces that a phosphorescence similar to that obtained from the ruby was developed. This is, evidently, the reproduction of the phenomena obtained, a long while ago, by M. Edw. Becquerel by means of the solar light. Mr. Crookes, indeed, adds that the aluminum, if sufficiently electrified, passes from an amorphous state into a crystalline structure, a fact quite credible, and that it assumes at the same time a rose shade kindred to that of the natural ruby, a tint very difficult to understand.

EFFECTS OF TEMPERATURE UPON MAGNETISM.—Mr. John Trowbridge has just completed the following experiment in the physical laboratory of Harvard University. He submitted a bar of iron to a great cold of 60° cent. below zero, obtained by evaporating CO^2. He proved that the decrease of magnetism, suspected by Wiedeman, if the bar be at a lower temperature than that allowing magnetic impregnation, is indeed a demonstrated physical fact. The bar, which had been magnetized at 20° C. below zero, had lost almost ⅔ of its magnetism after 47 minutes of exposure to this cold. He also observed that, by keeping a bar of steel for a certain time at a temperature of 20° cent., 50 per cent of its primitive magnetism was restored.

PREECE ON FAURE'S BATTERY.—Mr. Preece, the electrician, is not favorable to Mr. Faure's battery. He remarks that although it possessed considerable force its resistance was very feeble and it could therefore give a powerful current. He dwells especially upon "time" as a factor in electric experiments. A strong current of one minute duration can be readily obtained, but for purposes of lighting, something more durable is needed. It is a pretty thing, but for to-day it is not practical.

THE AURORA BOREALIS.—The idea that the Aurora Borealis gave forth a distinctly audible sound was hitherto regarded as absurd. Physicists, however, are beginning to acknowledge it as a fact. "*Nature*," of London has a few letters on the subject. There seems to be two opinions as regards the nature of the sound produced. One party pretends that the noise is analogous to the rustling of silk, the other party compares it to the sound of crackling flames. The question however will shortly be solved by means of baloon ascensions that are now being made.

ACTION of Light upon Phosphorescent Bodies.—M. Clémandot.—The author maintains that phosphorescence is a purely physical phenomenon, due to a vibratory action exercised chiefly by the blue ray of light. He connects these phenomena of vibration in phosphorescent bodies with those which light occasions in organized bodies.

NOTE.

I wish some one would begin with the start given by the paper on polarization of sound, in "SCIENCE" for May 14, and thoroughly go through the subject of Etherial Physics.

The mechanics and elementary laws of action of the Ether substance are needed.

The seemingly rotary or spiral course pursued by the particles conveying light and electricity, as shown in the polarization of light and in that of magnetism, are especial subjects of choatic conception. And there is more beyond! SAMUEL J. WALLACE.

NOTICE TO CORRESPONDENTS.

The writer of a paper "*On Ether*" received by us, will much oblige by forwarding his name and address.

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SATURDAY, JULY 9, 1881.

OBSERVATIONS UPON THE COMET AT PRINCETON.

The comet has been seen and observed every night since June 25, except on June 30th. Every night, however, except July 2d and 3d, the observations have been interfered with by clouds, so that very little continuous thoroughly satisfactory work has been possible.

The light has fallen off rapidly. On the 26th, the comet was for half an hour better seen than at any other time, and the nucleus was judged to be just about equal to Arcturus in brilliancy. On July 2d, it was compared pretty carefully with the Pole Star and with α Urs. Majoris by *squinting*, so that the blurred images of star and comet were brought close alongside. I judged it just equal to Polaris and about $\frac{1}{4}$ to $\frac{1}{3}$ of a magnitude fainter than Dubhe.

The nucleus and coma have presented a very interesting series of telescopic phenomena, in the main such as have been seen in all other large comets. It is noteworthy, however, that immediately behind the nucleus no strongly marked dark shadow-like stripe has been developed, nor, what is perhaps just as common on the contrary, any bright central streamer. On the whole, the central portion of the tail has been a little less brilliant than the edges, even close to the head, but the difference has been slight. On the 25th, the nucleus about 10 P. M. showed 5 projecting jets, much like the pseudopodia of some low animal organism—not well formed, nor distinct, nor symmetrical,—their length from two to six times the diameter of the nucleus, those on the front of the nucleus being the longer.

On the 26th, the nucleus was almost entirely surrounded with a nearly complete, well defined, circular envelope about 1′ in diameter. In this envelope was a curious oval vacuole, behind the nucleus, but on the preceding side of the axis of the tail.

On subsequent evenings no envelopes nearly so complete were noted—only jets of varying length and position, those on the side of the sun being apparently blown back, like flowing hair, by some solar repulsion.

On the 29th there was but one jet on the sunward side, and this was curiously curved toward the preceding side, making the whole look like a comma. (We use preceding rather than Western, because below the the pole where the comet was, the terms Eastern and Western might lead to misapprehension.)

On July 1st the head was curiously unsymmetrical. The coma was extended out in the South following direction like a great liberty cap, the axis of the principal jet which divided both ways, in front like hair parted in the middle, being inclined some 50° to the line of this extension.

With the spectroscope a number of observations have been made.

The nucleus has generally given a simply continuous spectrum, extending from below C well above G; but on June 25th and July 1st, it showed distinct banding at points where the bands of the spectrum of the coma crossed it.

This was seen by several observers on the 25th, and by both Mr. McNeill and myself on the 1st.

The spectra of some of the brighter jets had been caught and isolated several times. They were in all cases continuous, without detectable bands of any kind.

The spectrum of the tail was found to be continuous, with a faint superposed band-spectrum, the same as that of the coma. On July 1st and 2d this band-spectrum was distinctly traceable to at least 15′ distance from the head of the comet, the continuous spectrum perhaps 5′ or 10′ further.

The spectrum of the coma consisted of the usual three bands; but both the upper and lower bands, though pretty bright, were very ill defined; so much so, that I could obtain no satisfactory measurements of wave length, farther than to observe on June 25th and 26th, that the lower edges of the upper and lower bands of the so-called 'first' spectrum of Carbon, (λ, 5635 and 4740) given by a Bunsen burner, fell apparently near the lower limit of these two bands in the comet spectrum as seen with a one prism spectrum. But these comet-bands did not look at all like the flame bands, the difference of appearance being so great, as somewhat to shake my belief for the time being, in the identity of the two spectra.

The middle band, on the contrary, was perfectly defined at its lower edge, and with the one prism spectroscope distinctly showed three fine lines in the band, and these, so far as could be judged, coincided exactly

with the three lines in the middle band of the carbon spectrum.

This resolution into lines was seen by Professor Brackett, as well as by Mr. McNeill and myself, on June 29th; it was still evident on July 2d, but no longer on July 3d.

The coincidence with the middle band of the flame spectrum, has always appeared to be precise; but to obtain further evidence as to the exact position of the band a careful comparison was made on July 2 with the *b* lines of the magnesium spectrum, using the Grubb spectroscope with a dispersive power of *four* sixty-degree-dense flint prisms and a magnifying power of about 25. The slit was opened until the well defined upper edge of *b* just touched the lower edge of b_2. Then, the spark producing the magnesium spectrum being suppressed, the bright wire of the micrometer was set upon the lower edge of the comet band; finally, the spark being restored, the distance was measured from the edges of b_2. In this way twelve readings were obtained by Mr. McNeill and myself, all giving results ranging between 5160.0 and 5169.5, the mean being 5164.8 ±.0.7. I do not think the possible error can exceed 2 divisions, or three times the probable error. If so, the Comet spectrum cannot possibly be identified with the *second* Carbon spectrum, (the spark spectrum of a Geissler tube containing C O). Since the corresponding band of that spectrum has a wave-length of 5198.4. The wave-length of the band in the flame spectrum is 5165.3—both according to the figures of Dr. Marshall Watts, given in *Nature*, vol. 20, page 28.

As a further test, on July 3d, the suggestion of Dr Watts, made in the paper referred to, was followed out by confronting the comet-spectrum by means of an occulting bar, directly with Geissler tubes containing CO and CO_2, and with the Bunsen-burner flame. The bands on this night were more distinctly defined than on previous occasions, though the nucleus spectrum was less brilliant, and the result of the confrontal was very satisfactory and decisive. The upper and middle bands were found undoubtedly coincident, so far as the power used could show, with the flame spectrum, and *not* with the bands of the tube spectra. In the case of the lower band, the evidence was less conclusive, because the edge was ill defined and faint, making pointing difficult, and because bands of the flame and tube spectra are nearly coincident here. Still, even with this band, the evidence of about half a dozen pointings turned in the same direction.

On the whole, I consider it now absolutely certain, that the comet-spectrum is not the *second* spectrum of Carbon, whether it be the *first* or not. As to this latter point I do not feel quite sure, but the coincidences are certainly very remarkable and close, hough the peculiar appearance of the upper and lower bands when the comet was brightest requires explanation. C. A. YOUNG.

PRINCETON, *July* 4, 1881

PRIMORDIAL COSMIC RINGS.

III.

By EDGAR L. LARKIN.

The doctrine that a sphere of atoms, abandoned rings, or any other shaped masses can develop into planets is a physical error. It is impossible that the ball revolved. Face the south, hold the plane of the page of "SCIENCE" horizontally, call the paper the centre of the sphere, and it will be seen that to cause rotation force must be applied, if above the centre, from west to east; below, from east to west; to the right, from below, upward; and to the left, from above, downward. The gas was of excessive tenuity, and external force instead of causing rotary motion would displace the atoms in front of it. The mildest, or most violent force alike, would be unable to cause revolution in a globe of atoms of such extreme mobility. But there was no external force; energy is a property of matter, and the nearest matter was 20 trillions of miles away. If the sphere rotated the motion came from internal causes, none of which could have at that time existed. There were no vortices, currents, tides or whirlwinds in matter of such rarity; no force outside, and none within save the slowest possible radial descent. The sphere was at rest. No point in the experiment of M. Plateau had analogy to the generation of rings on the gaseous globe. He placed a globule of oil in a fluid having like specific gravity, passed a wire through it, and turned it as an axis until the sphere of oil partook of the rotation, flattened and detached a ring. The cosmical mass was of rare gas, and existed in a void, with no external power to turn it. If Plateau had suspended a ball of hydrogen in a vacuum, annihilated the attraction of the earth, and then made it revolve without applying force, the cases would be similar.

Neglecting the laws of Nature we will assume that the primitive sphere was in rotation. Admitting it, a demonstration will be made that if by unknown law it cast off a ring or any other form of mass, said portion could not have been abandoned anywhere in the vicinity of the orbit of Neptune.

First proposition:—If the sphere by rotary motion, or other mode of force cast off its equator, matter which condensing made Neptune, then that planet formed, and now moves on a line that coincided with the *Centre of Gravity* of the discarded mass, no matter what was its shape, size or density.

This statement we deem self-evident, incapable of argument, and an absolute truth.

Second proposition:—If the ring that contained the matter now existing in Neptune, was thrown off the equator of a sphere, a section of the ring perpendicular to its length would be either a circle, or a segment of a circle. That is, the ring would be either cylindrical, or flat inside and curved outside, the curvature being the arc of a great circle, a meridian bisecting the poles of the sphere. We can conceive of no form of mass capable of being detached from the circumference of a sphere. other than cylindric or segmental.

Third proposition:—If the Neptunian ring was not cast off when the mass was a sphere, it was abandoned after the ball had depressed at the poles, and elongated at the equator. And a perpendicular section of such detached protuberance would be some one of the Conic Sections.

Draw a chord of an arc from north to south any distance below the orbit of Neptune, so that it does not de-

scend farther than half way to Uranus, or 500,000,000 miles; then all the matter alluded to in this paper will be above the chord. So long as the mass remains spherical the chord will cut out a segment of a circle. Now let the cosmic sphere receive some unknown impulse that will accelerate its velocity of rotation, and the mass will change to a spheroidal form. The chord of the arc will shorten, matter at the equator will become elevated and sections of the protuberance will change curvature. Make both ends of the chord points of tangency, and produce tangents to the curve to infinite space. Then if rotation accelerates, the curve bounding the ascending equatorial protuberance must continually change form, and the tangents, direction; while sectional curves will pass all varieties of the hyperbola, parabola and ellipse. Thus let the mass become very oblate, pass a cutting plane down to the chord, and the curve cut out will be hyperbolic. Increase rotation; the equator will become higher, the chord shorter and the sections parabolic. Let the velocity be still accelerated, the equatorial matter will be lifted to greater altitudes, the chord will be shorter than ever, and the sections elliptical. To this reasoning the objection may be raised by some that no matter how flattened the mass might become, sections cut to the chord of the arc would in every case be elliptical. We do not insist that they would be hyperbolas or parabolas; but will prove if ellipses, that elliptical segments are more fatal to the theory of ring formation than are segments of any other form of curve. Two factors engaged in the evolution of cosmic rings—gravity and an opposing force generated by rotary motion. We attack the whole Nebular Hypothesis with the fact that if the revolving gaseous mass abandoned matter at present existing in Neptune, the planet is now in the position of the centre of gravity of the detached portion. This being true, Neptune never became a member of the solar family by displacement of its material from the original mass, because no mass could have been cast off whose centre of gravity coincided with the orbit of that world. Let us see if the Neptunian ring was abandoned when the cosmic mass was a sphere. If so, the ring was either cylindrical or a segment of a circle. But the centre of gravity of a section of a cylindric ring is in the centre of the section. Since Neptune now traverses a path once the centre of gravity of the ring, it follows that when detached the spere of gas was larger than a ball bounded by the Neptunian orbit, as there must have been as much matter above the centre of a section of the ring as below. The larger the sphere the slower the rotation, hence it did not rotate as rapidly as it would, had it been equal in size to a globe having the diameter of Neptune's track. But it had to revolve faster to detach a ring because Neptune now moves on an orbit with a velocity of 3.36 miles per second; yet displays no tendency to leave it on a tangent. And greater detaching force would have been required to cause a ring to leave the equator than would now be necessary to throw Neptune off its orbit, because the force had to overcome what little cohesion the dissociated atoms had. The sphere must have been far larger than the path of Neptune, because the ring, being abandoned at the equator, had to be hundreds of millions in thickness to secure gas enough to condense into the planet, and its rate of rotation proportionately less than its present velocity.

It is certain that the ring whence Neptune was formed was *not* cylindrical. The only other possible form of ring is segmental. The distance of centres of gravity of all circular segments from the centre of the circle can be calculated. The problem resolved itself into this:—given the distance of the centre of gravity of the segment of a circle from the centre, to find the dimensions of the segment, and radius of the circle. We know that Neptune is in the position of the centre of gravity of whatever shaped mass was detached. But it lies on the circumference of a circle whose radius is the distance to the sun. Therefore the circle must have been larger than its orbit to be able to afford a segment having sufficient size to have its centre of gravity coincide with the track of Neptune. In all these computations we take the distance of Neptune from the sun to be 2,780,000,000 miles.—Elements of 1850, Newcomb's Astronomy. The ring of whatever shape is supposed to be detached, severed, straightened, and cut into an infinite number of sections perpendicular to its length. In the case in question, sections are segments of a circle, and we are in search of the radius of the circle whence the segment was cut. We have found the length of the radius to be 3,000,000,000 miles, by means of the formula, $G = \frac{C^3}{12A}$, wherein

G=the distance of the centre of gravity of the segment from the centre of the circle.

C=the chord of the arc, or base of the segment.

A=the area of the segment.

That is—"Divide the cube of the chord of the segment by twelve times the area of the segment; the quotient will be the distance of the centre of gravity required from the centre of the circle."—Vogde's Mensuration p, 237. Making approximation with a circle whose radius was 2,900,000,000 miles, with chords at different distances within the Neptunian orbit, it was found in two trials that a circle of that radius was untenable. Using a circle having a radius of 3,000,000,000 miles, and chord descending 300,000,000 miles, it was soon found that the centre of gravity of that segment was in distance from the centre equal to the distance of Neptune from the sun. But the chord was 2,600,000,000 miles long! Does anybody believe that a break took place along a line of such length, and 300,000,000 miles below the equator of the sphere? Was detachment possible when the sphere rotated slower than the orbital velocity of Neptune now is, yet shows no signs of elevating to a tangent to its path, though moving with unimpeded force? The first world was not abandoned by the cosmical mass when a sphere.

Could it have been formed from the matter contained in the segment of any other curve known to geometers?

To find the centre of gravity of a parabolic area:—"The centre of gravity is on the axis, at a distance from the vertex equal to three-fifths the altitude of the segment," Peck's Calculus p. 175. Then Neptune, as it is the centre of gravity of the parabola must be two-fifths above the base or limiting plane of the curve, We have made calculation of the altitudes of several possible parabolas, by locating the base at different distances between the orbits of Uranus and Neptune. The following table shows the distances of the limiting planes below Neptune, the altitudes of the segments, above the base,—above Neptune,—and also gives the diameter of the mass on the hypothesis, that it could have been so elongated as to make it possible that parabolas could be cut out of the equator by perpendicular planes.

TABLE I. ALTITUDES OF PARABOLAS. DISTANCES IN MILES.

Distances of Base Below Neptune.	Altitudes Above Base.	Altitudes Above Neptune.	Diameters of Mass when so Expanded.
500,000,000	1,250,000,000	750,000,000	7,060,000,000
400,000,000	1,000,000,000	600,000,000	6,760,000,000
300,000,000	750,000,000	450,000,000	6,460,000,000
200,000,000	500,000,000	300,000,000	6,160,000,000
100,000,000	250,000,000	150,000,000	5,860 000,000
50,000,000	125,000,000	75,000,000	5,710,000,000
25,000,000	62,500,000	37,500,000	5,635,000,000

Should these figures be deemed unsatisfactory, because they relate to sections or surfaces, while actually considering a solid ring, a table of *paraboloids* is inserted. The ring was 17,467,000.000 miles long, cut it in an infinite number of parabolic sections; revolve each

about its axis considered motionless, and an infinite number of paraboloids will be generated, all interlacing throughout the length of the ring. The centre of gravity of a paraboloid is two-thirds the distance, from the vertex to the limiting plane.

TABLE II. ALTITUDES OF PARABOLOIDS IN MILES.

Distances of Base Below Neptune.	Altitudes Above Base.	Altitudes Above Neptune.	Diameters of Mass when so Elongated.
500,000,000	1,500,000,000	1,000,000,000	7,560,000,000
400,000,000	1,200,000,000	800,000,000	7,160,000,000
300,000,000	900,000,000	600,000,000	6,760 000,000
200,000,000	600,000,000	400,000,000	6,360,000,000
100,000,000	300,000,000	200,000,000	5,900,000,000
50,000,000	150,000,000	100,000,000	5,760,000,000
25,000,000	75,000,000	50,000,000	5,660,000,000

No tables of altitudes of hyperbolas or hyperboloids have been inserted, as the distances of their gravitation centres differ so little from parabolic segments, that it was not thought best to fill up the columns of "SCIENCE" with useless figures. For those who think the ring could not have been left when sections were parabolic or hyperbolic, we give a table of altitudes of ellipsoids, that is when sections cut to the chord as before, were ellipses. "The centre of gravity of a semi-prolate spheroid of revolution is on its axis of revolution and at a distance from the centre equal to three-sixteenths the major axis of the generating ellipse."—Peck's Calculus, p. 175.

Therefore Neptune is 3-16 above the conjugate axis, and 13-16 below the vertex of the ancient semi-ellipsoid, all the worse for the theory of ring detachment. Consider the ring cut by perpendicular planes descending to the chord, into an infinite number of semi-ellipses. The chord becomes the conjugate; revolve each curve about its semi-transverse axis regarded as stationary, then the ring will be made up of an infinite number of semi-prolate spheroids of revolution, each so nearly coincident with the next as to have the surfaces fail to coincide only by infinitesimal space. The table is computed by calling the conjugate diameter, the chord of the arc, and the semi-axis major, the line reaching from its centre up to the equator, Neptune being in the centre of gravity of the solids of revolution.

TABLE III. ALTITUDES OF SEMI-PROLATE SPHEROIDS.

Distances of Conjugate Axes Below Neptune.	Altitudes Above Base.	Elevations Above Neptune.	Diameters of Cosmic Sphere When so Elongated.
500,000,000	2,667,000,000	2,167,000,000	9,894,000,000
400,000,000	2,134,000.000	1,734,000,000	9,028,000.000
300,000,000	1,600 000,000	1,300,000,000	8,160,000,000
200,000,000	1,066,000,000	866,000,000	7,292,000,000
100,000,000	533,000,000	433,000,000	6,426,000,000
50,000,000	266,000,000	216,500,000	5,592,000,000
25,000,000	133,000,000	108,250,000	5,778,000,000

These tables of absurd figures are inserted to show the hypothesis irrational. No such extension of the mass was possible, and no protuberance could have arisen above the equator able to afford perpendicular sections, hyperbolic, parabolic or elliptic. Nor could the chord become the limiting plane of any parabola, hyperbola or conjugate axis of any ellipse. Yet, the tables are logical deductions from the doctrine of ring detachment, for if the mass depressed at the poles, and elongated at the equator, curvature of radial sections must have assumed all varieties of conics. Since the centres of gravity of all these curves, and solids generated by their revolution are known, the figures are correct if the theory is true. It will be shown in a paper on mass, volume and density, that most of these equatorial elevations could not have contained matter enough to form Neptune.

Is it credible that the primeval mass ever detached rings or any other shaped portions? From the altitudes of these conoids it is seen that to cast off the Neptunian material the rupture in every case took place at depths of hundreds of millions of miles, where cohesion was greatest and rotary velocity least! In all these computations the abandoned masses were considered as homogeneous, as difference in density in a gas of such excessive rarity cannot enter as a factor at depths of a few hundred million miles. It may be said that cohesion in a gas so rare, was not a factor. Granted, then *rotation* was not, since a ball of gas of such tenuity as to have no cohesion, could not possibly be set in revolution. The equatorial edge of the mass could not have become angular, for sections cut to the base would be triangles, whose centres of gravity are two-thirds the distance from the angle to the base, and nowhere near where Neptune exists. Neither could sections have been cissoidal, conchoidal, cycloidal or sectoral, nor of any other similar curvature known to geometry. The surface was not irregular; loose masses did not float above the periphery; the matter was all of the same specific gravity, hence *buoyancy* did not obtain on a mass of dissociated atoms. The mass existed in a void, else external matter by friction would have induced currents from east to west. No modes of energy save rotary force, existed to detach a ring, no internal repulsion, as that had vanished in dissociation. The dogma is beset on all sides with difficulties. When the mass was spherical, matter enough to form Neptune was unable to leave the equator; when elongated, segments of enormous depth had to be left by the shrinking mass, to afford material sufficient to condense into the oldest planet; and the break occurred where it was most difficult to be made, and where the power necessary to make it was the least.

Not only the most complex, but the simplest laws of nature dispute the Nebular Hypothesis. Even primary schools have text books wherein laws are laid down that subvert it! Primers of natural philosophy teach that if a revolving sphere diminishes in diameter, its velocity of rotation becomes accelerated, and the same primers teach that if the diameter increases the velocity diminishes. Therefore, if the primeval gaseous sphere ever revolved, said rotation caused the equatorial diameter to increase in length; but as soon as lengthened the velocity of rotation diminished and the mass again became a sphere, the oscillation always remaining within small limits. The diameter of the mass when spherical was 5,560,000,000 miles; can it be believed that rotation so far gained mastery over retardation as to allow the mass to attain diameters ranging between 6,000,000,000 and 7,000,000,000 miles to detach parabolic segments; and between 6,000,000,000 and 9,000,000,000 miles to abandon semi-prolate spheroidal sections to make up a ring? We are unable to conceive that valid argument can be made in favor of the detachment of matter in any form or volume from the mass. This theory, opposed by every known law of nature has actually been entertained by eminent physicists, geometers and astronomers, fully conversant with these same laws that destroy the doctrine; a thing long noted by psychologists, wherein delusions hold sway over fine minds with greater tenacity than ideas known to be true.

SEISMOLOGY IN JAPAN.—The labors of the Seismological Society of Japan have established the fact that there is a chronic center of disturbance within a radius of a few miles from Yokohama. We are glad we do not reside in the said Yokohama; at the same time, we congratulate the society on the success attending its researches.

THE USE OF WATER AS A FUEL.

By Dr. George W. Rachel.

The results of certain experiments, made with what has been called the Holland Hydrogen Locomotive, have lately been published in several city papers. They are not only of the highest practical importance, but of great scientific interest, so that it appears entirely proper to discuss them from that aspect in this journal.

The fuel used is naphtha and water; the manner in which combustion is accomplished by a peculiar unique apparatus may be shortly described thus:

The principal feature of this new invention is an iron retort having two compartments, one for naphtha and the other for water. The two fluids are conducted into the two chambers by induct-pipes at one end of the retort, while at the opposite side there are two escape-pipes, through which the vapors of the two substances escape from their respective chambers, where gasification has taken place. The two gases are being mixed by passing into a common receptacle, the manifold, and from there they are distributed through three main pipes to the 352 burners. Of these 44 are placed directly under the (four) retorts, while the balance is arranged under the boiler.

The astonishing results obtained by this ingenious apparatus have been the subject of many discussions in various scientific and industrial journals on both sides of the Atlantic. The attacks have usually been directed against the possibility of making an advantageous use of the hydrogen for the purpose of combustion. The explanation that in the Holland retort the principal source of the tremendous heat produced, is due to the combustion of hydrogen derived from the dissociation of the water vapor, has been supposed to be met by the following statement:

The dissociation of the steam must consume as much heat, as is afterward developed by the combustion of the hydrogen.

It was contended that the principle of the Holland method was entirely wrong, implying an error against the law of the conservation of energy which is the fundamental law of the Universe, and therefore this whole matter must be a delusion.

This objection, which looks plausible enough can be shown to be erroneous, as it is based on a misconception, or rather a misinterpretation of this great law of Nature. The error consists in the wrong application of the word heat; the sentence containing the objection to be correct, must read thus:

The dissociation of the steam must require as much *energy*, as is afterward developed by the combustion of the hydrogen thus obtained. Now, it is a fact, that the energy developed by the combustion of the hydrogen invariably takes the form of heat, but the principle of the correlation of forces which forms the basis of this very law, teaches us that it must not necessarily do so during the process of dissociation. In order to fully expose the misinterpretation of Nature's fundamental law contained in the objection above quoted, we may be allowed a few words on the subject of dissociation.

Prof. H. St. Clair Deville, who first succeeded in an ingeniously contrived apparatus to dissociate water vapor into its elements, hydrogen and oxygen, estimates the temperature required for the purpose at 6000°C, probably even somewhere near 8000°C. Prof. Schroeder van der Kolk even places it at a still higher figure, viz.: about 10,000°C. But these figures, it must be well understood, refer to the dissociation of water vapor in the absence of any other element. If, on the contrary, the dissociation is induced to take place in the presence of other elements —notably metals—the dissociation temperature is lowered considerably. Thus, for instance, the dissociation is effected in the presence of platinum, at 1700°C; iron filings, 1400°C; silver, 1000°C, instead of 8000°C.

The question is now: How are we account for this? Prof. Deville in a controversy now going on between Prof. Ad. Wurtz and his school, and Berthelot and himself, on this very subject of dissociation, replies to some objections of his adversaries, as follows:*

It is a well-established fact that the dissociation of water-vapor takes place at much lower temperatures in the presence of certain elements. . . . . These and other examples . . . . prove that the development of heat during the formation of a compound body, does not hold any known relation to its dissociation temperature. Evidently the error is very frequently committed in regard to these processes to confound actual and kinetic energy, actual and latent heat."

The relation referred to in this passage, must, however, in the light of the law of "Conservation of Energy" be one of absolute equivalency; the energy expended on one process—dissociation—must under all circumstances be equivalent to the energy developed during the other process—formation of the compound body, *i. e.* in our case water-vapor.

If, therefore, our experiments show that the temperature of dissociation is lowered in the presence of certain elements, we must look for some other form of energy which supplants the amount of heat saved. What force is it that steps in here and plays the role of a dissociating agent in place of the tremendous heat? The answer is obvious, it is *chemical affinity*, for chemical affinity is the only form of energy capable of such intensity of action. Furthermore, chemical affinity is to a certain extent not directly discernible and measurable, as Friedrich Mohr has shown.†

The irrefutable proof for our assertion lies in the fact that there is in the case under consideration, always formed an oxide of the metal employed. We find the molten silver and platinum covered with thin films of their respective oxides while the iron filings show an amount of oxidation which is—as it is in the two former cases—in direct proportion to the quantity of vapor dissociated. The chemical affinity of the glowing and molten metals to the oxygen of the water-vapor being greater than the chemical affinity of hydrogen to oxygen, they appropriate the oxygen of the steam, and, combining with it, form their respective oxides—thus liberating the hydrogen and accomplishing dissociation.

Deville's above-quoted statement, that there is no known relation betweenthe formation—and dissociation-temperatures of compound bodies must be modified, therefore, in the light of the foregoing observations. What he is pleased to call "Kinetic energy" and "latent heat" is actually nothing else than chemical affinity. The position of Wurtz and his followers, by the way, is untenable; they contend that the two temperatures should be equal,(in accordance with the law of the conservation of energy) and meet the often observed fact that these temperatures differ considerably with the assertion that, as theoretically they should not do so, the observations are wrong. Their interpretation of the great principle contains the same error which the objections to the Holland process are suffering from; they insist that the energy which produces dissociation must take the form of heat and heat only, because heat is the only form of energy met with during formation. They forget that such limited application of this great principle is entirely arbitrary and that the only requirement of the law is that of absolute equivalency, while there is no rule as to the kind of energy required.

I have been somewhat elaborate in my remarks on the subject of dissociation, because the conditions under which the dissociation of steam takes place in the Holland process are the exact counterpart to those which have just been dwelled upon. Instead of the metals, the

*Comptes rendus, 1879.

† Science Vol. I, pg. 244.

carbon of the Naptha-gas reduces the dissociation-temperature.

The fact that under certain conditions carbon has a dissociating action on steam, or—as some put it—carbon may be burned up with watery-vapor, has been known for a long time; the presence of free hydrogen in furnace and generator-gases is due to this circumstance. The difference between the dissociating action of carbon on water-vapor as compared with that of the metals abovementioned is only one of degree. *The temperature at which it takes place is much lower.*

After a prolonged and careful observation of the phenomena connected with the Naphtha and water-process under consideration, the writer was firmly convinced that the carbon in them plays the role of a dissociating agent, and that the temperature at which its dissociating property asserts itself must be a low one, comparatively speaking. For, in this way only was it possible to account for remarkable results of the HOLLAND heating method.

Unhappily, we were not then acquainted with the experiments presently to be discussed, although the fact privately communicated to us that MR. MOSES FARMER, of Hartford, the well-known philosopher, had found experimentally the temperature at which carbon will dissociate water-vapor to be not much above 900° C., seemed to confirm the position taken.

While our proposal to entrust some able chemist with this investigation was under consideration, we became aware of the fact that the desired experiments had already been made in another quarter of the globe more than a year ago. Thus, fortunately, a doubt of their genuineness, which otherwise might perhaps have been entertained by the opponents of the HOLLAND method, is out of the question.

PROFESSOR ACKERMAN, who is superintendent of the chemical labratory at the *Stockholm School of Mines*, requested one of his assistants, MR. C. G. DAHLERUS, to make some experiments with the view of determining the temperature and other circumstances required for the combustion of carbon with watery vapor. The real aim was to explain the occurrence of free hydrogen in both furnace and generator gases; this fact is, as we said before, well-known to mining engineers.

The apparatus used by DAHLERUS consisted of a tube filled with charcoal, which was heated in a combustion furnace, while steam, generated in a separate boiler, was allowed to pass through it. The temperature was determined by trays of steatite containing pieces of Mayerhofer alloys, with various melting points being introduced into the tube. The gases generated were collected, after having passed through a spiral gas tube in order to condense the steam they contained, and were then analyzed. Every experiment lasted at least two hours before a sample of the gas was taken, the pressure of the steam in the induct pipe being kept as uniform as possible.

The results of these experiments have confirmed the correctness of our position, and have shown that dissociation of watery vapor in the presence of carbon takes place at much lower temperatures than has hitherto been admitted.

MR. DAHLERUS, in giving a table of his results, sums up as follows:

"On examining this table it appears that watery vapor is decomposed at a temperature which is indicated by the alloys as from 450° to 500° C.; *but the temperature may, in fact, not have been higher than* 400° *C., because zinc in the interior of the tube was not fused in any of the first five experiments.*"

It is evident that in the Naphtha and water process the conditions, under which the dissociating action of carbon on water vapor takes place, are much more favorable to it than those obtained in the apparatus used by DALHERUS for his experiments.

In the first place the action of carbon in the latter gentleman's apparatus could not but be of a very slow nature, the surface only of the glowing charcoal in the tube being enabled to gasify and act chemically on the steam surrounding the pieces of it. In the process under consideration, however, the whole of the carbon of the naphtha is in gaseous condition and by diffusion the vapor is acted upon simultaneously at every point. Furthermore, this very gasification of the carbon requires a definite, not inconsiderable, amount of heat which in DAHLERUS' apparatus has to be supplied by the steam itself, this being the only substance admitted into the presence of the charcoal in the tube. In the new process, on the contrary, this gasification is effected before the carbon-compounds of the naphtha are mingled with the steam and no loss is therefore experienced in in this direction. But, aside from these details—for the combustion-furnace will probably furnish the wanting heat—the highly important fact is established by these experiments that chemical affinity does, in this dissociation process, supplant heat for the greater part. And, considering the great advantages, above detailed, of the HOLLAND process over these experiments, we are justified in assuming the lowest temperature, found sufficient by DAHLERUS in five of his experiments, as entirely sufficient in the HOLLAND process also. *Instead of* 8000° C., *therefore being required for the dissociation of water, it will here take place at* 400° C.

A gain therefore of, say for convenience's sake, nineteen twentieths is effected; for every particle of hydrogen thus dissociated and liberated, at 400° C., will develope its full 8000° C., on combustion with oxygen, *i. e.*, on being burned up by the draft air. And this saving is accomplished by the supplanting of heat with chemical affinity, the latter performing the greatest part of the work of dissociation.

Nor is this all!

It is necessary to state here that DAHLERUS in pursuing his work had in view also, the preparation of *water gas*, which has been introduced into Sweden by PROF. TORREL, who was one of the commissioners from that country to the Centennial Exhibition at Philadelphia. He therefore endeavored to find the most favorable condition for the production of *water gas*, a mixture of hydrogen and carbonic oxide which is known in this country under various names (*e.g.* STRONG, LOWE and others.)

This explains the following sentence in the conclusions he draws from his results:

"Further we see that the greater the excess of watery vapor the richer in carbonic acid are the gases; or, in other words, *that carbonic oxide is very easily burned to carbonic acid by means of watery vapor*, and that the content of carbonic oxide is increased both by a lessened excess of watery vapor and by the raising of the temperature. The best gas is thus obtained by raising the temperature as high as possible and by a moderate supply of steam."

What DAHLERUS refers to as *the best gas* must be understood to be *water gas* in the accepted sense of the word, viz.: A mixture of carbonic oxide with hydrogen. It is for this reason that he advises the use of a limited supply of steam only; for, if there is an unlimited supply of steam, the dissociation of the same continues and the carbon, instead of being confined to its first stage of oxidation (to carbonic oxide), completes this process and is burned up to carbonic acid. Although the result is by these means a gas much richer in hydrogen—in fact twice as rich—this is not what the manufacturer of water-gas wants. He wants a product that may be used for illumination as well as for heating purposes and, therefore, he does not want an almost pure hydrogen-flame—which is non-luminous, as is well known. But with the HOLLAND process this is quite different; here the manufacture of illuminating gas is effected in a separate automatic arrangement which does not concern us here now. In the process under consideration, therefore, the heating quality of the gases is the only consideration. This, the more so, since there is here no separate gener-

ator from which the gases therein manufactured are led away in pipes to the heating-place. The generator, *i. e.* the HOLLAND retort is at the heating-place, in the fire-box of the locomotive, and the full effect of the carbon combustion is therefore obtained in both cases, whether the dissociation of the steam takes place to furnish oxygen for the first stage of this combustion only or whether the dissociation is accomplished so as to burn up the carbon completely with oxygen derived from the dissociated water-vapor. But there is this great difference: If the carbon derives all the oxygen necessary for its complete conversion into carbonic acid from the dissociation of the steam, there will be twice as much hydrogen liberated as against its conversion into carbonic oxide only, as will be seen from the following statement of the two cases by DAHLERUS:

"When watery vapor burns carbon to carbonic oxide, there are formed from two volumes of watery vapor and one volume of carbon two volumes of carbonic oxide and two volumes of hydrogen; further, when carbon is burned by watery vapor to carbonic acid, there are formed from one volume of carbon and four volumes of watery vapor, two volumes of carbonic acid and four volumes of hydrogen. Consequently the volume of hydrogen in the gases is equal to the volume of carbonic oxide and double that of the volume of carbonic acid."

In connection with these important relations I must, in conclusion, refer to the results of numerous experiments, made with the HOLLAND process, which can only be fully and satisfactorily explained in the light of the previous discussion. They are certainly a most remarkable series of experiments, never before equalled or excelled; the results accomplished by the Naphtha and water process have startled all experts and scientists who have witnessed them, while those who have not seen their actual performance reluctantly admit their genuineness. Yet they are absolute facts, and the possibilities which they have in store are greater than anything that has as yet been reported.

In starting the fire under the boiler of this locomotive, it must be stated, there is first lighted a small tank filled with naphtha, which is placed under one of the retorts in the fire-box. As soon as this retort is thereby sufficiently heated to gasify the naphtha, naphtha-gas is burned under all the retorts, and water admitted into them to be converted into steam. When both naphtha and water are thus gasified, their gases are jointly admitted to all the burners under the whole length of the boiler, and the generation of steam now begins in earnest. As soon as feasible, steam from the boiler is introduced into the retorts instead of water, so that after this period the naphtha only has to be gasified in the retorts.

I now give one of Mr. CONANT'S tables in full, containing the results of an experiment he witnessed on April 29th:

LIGHTED AT 10:05 A. M. GAS STARTED AT 10:35.

Steam, Pounds.	Time, M.	Naptha, Gall.	Naptha, Per Lb. Gall.	Naptha, Per Min Gall.	Totals. Gall.	Totals. H. M.
10	69½	5.62	.56	.08	5.62	1 09½
20	15	3.83	.40	.27	9.45	1 24½
30	18½	2.7	.29	.15	12.35	1 43
40	9	2.41	.24	.27	14.76	1 52
50	8	2.14	.21	.27	16.9	2 00
60	7	2.14	.21	.30	19.04	2 07
70	5	1.61	.16	.32	20.65	2 12
80	3½	1.07	.10	.30	21.72	2 15½
90	4½	1.07	.10	.24	22.79	2 20
100	4	1.07	.10	.27	23.86	2 24
110	4	1.07	.10	.27	24.93	2 28
120	4	1.07	.10	.27	26.00	2 32
Engine started out—safety valve blowing—oil disturbed and no record.						
133	5	----	----	----	----	----

Pop valve blowing av. 33 sec., with 32 sec. intervals. No right of way and no run.

The puzzling fact that the higher the temperature and the steam-pressure rise, the less naphtha is burned, would be absolutely inexplicable if it was not for the relations alluded to in the foregoing observations. Up to 60 or 70 pounds of steam-pressure in the boiler the consumption of naphtha averages 2.14 gals. for every ten pounds of pressure added, while above these figures, it averages only 1.07 gals.—just one-half of the former quantity—for every additional 10 pounds. We know what that means. It means that there is an evident supplanting of the naphtha by some other much more powerful heating agent; the naphtha in this process unmistakably plays a subordinate role, as far as the heating is concerned. We know its task. It dissociates the water and thereby liberates its hydrogen; it is the latter that furnishes the bulk of the caloric energy developed. During the earlier stages, when the steam-pressure is yet comparatively low, the quantity of steam introduced into the retorts is limited and the carbon therefore is burned up to carbonic oxide only by dissociated oxygen; as soon, however, as the steam-pressure rises above a certain point the quantity of steam introduced is very soon sufficient to furnish all the oxygen necessary for the complete combustion of the carbon of the naphtha to carbonic acid. Thus, we are enabled by a correct interpretation of Nature's laws to explain fully and satisfactorily the paradoxical fact that the greater the heat, the less the consumption of oil. We know that instead of two volumes of hydrogen in the first, we must have four in the second case.

There is one other point which I may probably feel called upon to treat of, viz.: the utter invisibility of this tremendous fire. For the present the above will suffice.

DR. GÜNTHERS ICHTHYOLOGY.*

Less than a century ago the last edition of the Systema Naturæ of Linnæus, published in 1766, was taken as the basis and text of essentially a new compilation by Johann Friedrich Gmelin, and among the species admitted by Linnæus were intercalated those subsequently added by others to the system. There were very many duplications arising from the imperfect acquaintance of the compiler with his subject, but nevertheless, all told, only 826 species of fishes were named. There are now known, in round numbers, nearly ten thousand species. In the interval between the compilations of Gmelin and the present were published works of a like nature, by Walbaum, Lacépède, Bloch, Schneider, and Shaw. These were all finished before 1804, and were all of very little value. For considerably more than half a century no other descriptive general enumeration of fishes was completed. Meanwhile, from 1828 to 1849, Cuvier and Valenciennes gave to Ichthyology 22 volumes of a work designed to be a general natural history of fishes, but this was never finished. At last, in 1859, was commenced and in 1870 brought to an end, a work purporting to enumerate all the species of fishes known to the dates of publication, by Dr. Albert Günther, under the auspices of the British Museum. For this contribution the scientific world was laid under great obligations to the author as well as publisher. It was a compilation requiring considerable skill and acquaintance with the literature, and the work may be said to have been moderately well performed. Its author followed the outlines of classification proposed many years before by the illustrious Johannes Müller. On the whole this was the best course, perhaps, to be taken at the time. In 1861, however, he gave a systematic re-arrangement of the Acanthopterygian families, which was above all characterized by an excessive valuation placed on very trivial charac-

* An introduction to the study of fishes. By Albert C. L. G. Günther. Edinburgh: Adam and Charles Black. 1880.

ters, and was, in some respects, a step backwards, although of not very much moment.

Another and most radical modification—the next stage—may be fitly noticed in the author's own words. [1.] "The discovery (in the year 1871) of a living representative of a genus hitherto believed to be long extinct, *Ceratodus*, threw a new light on the affinities of fishes. [2.] The author who had the good fortune of examining this fish, was enabled to show that, on the one hand, [3] it was a form most closely allied to *Lepidosiren;* on the other, that it could not be separated from the Ganoid fishes, and therefore that also [4] *Lepidosiren* was a Ganoid: a relation pointed out already by Huxley in a previous paper on 'Devonian Fishes,' [5] This discovery led to further considerations of the relative characters of Müller's sub-classes, and to the system which followed in the present work" (pp. 25-26). In regard to this claim there are several noteworthy and characteristic features.

(1) In 1870, in Dr. Günther's Cat. Fishes Brit. Mus., vol. 8, p. 323, it is expressly admitted that "after [the 'sheet' descriptive of *Protopterus* and *Lepidosiren*] had passed through the press, Mr. Krefft informed me of the most interesting discovery that a living representative of *Ceratodus* had been found in Queensland. Nothing of this genus was hitherto known beyond teeth, as those described and figured by Agassiz in Poiss Foss. iii, p. 129, pls. 18-20." (2) Dr. Günther knew nothing whatever of *Ceratodus* till he received a communication respecting it from Mr. Krefft. (3) As indicated by Dr. Günther himself (Trans. Royal Soc., v. 161, for 1871), Mr. Krefft, in even the title of his paper, published April 28, 1870, and before Dr. Günther's "reply had time to reach Mr. Krefft," recognized the affinity of the genus to *Lepidosiren.* (4) As early as 1860, Gill (as Brandt, Peters, Lütken and others subsequently recognized) showed that "*Lepidosiren* was a Ganoid," and that *Polypterus* was a type intermediate between the ordinary Ganoids and the Dipnoi. (5) Consequently the only novelty in Dr. Günther's work was "the system which is followed in the present volume," which has been pronounced by an eminently competent judge to be "a triumph of systematic *gaucherie.*" Whatever is true in the statements examined had been appreciated before Dr. Günther labored and only what is untrue to nature and to science was original with him. The co-ordination of the facts enumerated was the necessary logical result of the successive steps.

But what is "the system which is followed in the present work?" Only the salient features may be noticed, and these will sufficiently appear from the enumeration of the sub-ordinal, ordinal and super-ordinal groups. These are:

I. SUB-CLASS—PALÆICHTHYES.
- I. Order—Chondropterygii.
  - I. Sub-order—Plagiostomata [Sharks and Rays].
  - II. Sub-order—Holocephala [Chimæroids].
- II. Order—Ganoidei.
  - I. Sub-order—Placodermi [Extinct].
  - II. " —Acanthodini [Extinct].
  - III. " —Dipnoi.
  - IV. " —Chondrostei.
  - V. " —Polypteroidei.
  - VI. " —Pycnodontoidei [Extinct].
  - VII. " —Lepidosteoidei.
  - VIII. " —Amioidei.

II. SUB-CLASS—TELEOSTEI.
- I. Order—Acanthopterygii.
- II. " —Acanthopterygii Pharyngognathi.
- III. " —Anacanthini.
- IV. " —Physostomi.
- V. " —Lophobranchii.
- VI. " —Plectognathi.

III. Sub-Class—Cyclostomata.

IV. Sub-Class—Leptocardii.

To those familiar with the facts and details of the anatomy of fishes and the inferior vertebrates, this enumeration will be its own best commentary. Suffice it for the present at least to affirm that it involves more contradictions and inconsistencies than have been manifested in any recent taxonomical exposition of any class of animals emanating from a respectable source.

Almost equally in disaccord with the cultivators of the other branches of Vertebrate Zoology is Dr. Günther in his treatment of GENERA.

The extreme of differentiation is practiced by ornithologists, (provided the differences are obvious and external), and a course is pursued in mammalogy which has received the sanction of the greatest number of students of that class, during at least the last quarter of a century. American ichthyologists have endeavored to comply with the principles on which genera in the latter class have been recognized as much as the differences of facts will permit, and although, of course, there are many disagreements as to detail, there is an essential congruity between them. The principles, if any, applied by Dr. Günther are undiscernable from his work. His methods indeed, seem to have varied with the whim of the moment and to have been modified for each case: the results then happening appear for him to have crystallized and not to have been subject to review or further consideration afterwards. Strange contrasts constantly occur in the extension or limitation of the groups. In the genus *Tetrodon*, for example, is discoverable a very considerable range of variation, not only in external features but still more markedly in the details of structure, and especially in the bones of the head. So great are these that there are three well defined major groups and a number of minor ones entitled to generic distinction, but, nevertheless, our author has refused to admit more than one "genus" for all the representatives of the type, whereas, in the related group of Diodontines, he has recognized a number of genera upon characters of very much less moment, such as the development of the spines, nostrils, &c. Under the genus *Gasterosteus* are confounded all the representatives of the family of Gasterosteids, and yet upon differences of the same kind as those which distinguish, for example, the "*Gasterosteus spinachia*" from the other species of *Gasterosteus*, are elsewhere constituted distinct *families.*

These examples might be extended indefinitely. Heterogeneous combinations of forms on one hand chance in strange contrast with isolated generic types on the other.

Comprehensiveness of genera *per se* is not a great evil, provided there is consistency in the treatment of the subject, and that all share as nearly alike as the nature of the case allows. It is to the assignment of inordinate value to a few superficial characters, and the subordination, to the manifestation of such, of other characters whose coincidence demonstrates them to be of greater importance, that we object. It is true that the acceptance of such comprehensive groups isolates in a measure the class in which they are recognized from others and tends to constantly mislead the inquirer who would compare the constituents of the several classes, *e.g.*, as to their geographical or geological relations. Even this, however, is of minor importance. It is the utter disregard of the gradations of structural differences exhibited by Dr. Günther in his constitution of genera that detracts so much from the value of his work. To enter into detail would necessitate space equal to the portion considered, and some instances must suffice.

Serranus (p. 381) is distinguished among its allies by the "small scales," presence of "very distinct canines in both jaws," and the absence of serratures from the lower margin of the preoperculum. Under the genus thus defined, there are not only species which disagree with the principal characters, but the *typical Serrani (S. cabrilla, S. scriba,* etc.) are more nearly related to the species of *Centropristis* than to the rest of their associates. A

natural arrangement—*i.e.* one based on their anatomical details—would require, first, the fusion of the Güntherian genera *Centropristis*, *Anthias*, *Callanthias*, *Serranus*, *Anyperodon*, *Prionodes*, *Plectropoma*, and *Trachypoma*; then the wide removal of certain forms, and finally the disintegration of the conglomeration on an entirely different basis from that accepted by Günther.

The instances wherein genera are referred to families with the diagnoses of which they diametrically disagree are numerous. Leaving out of consideration cases of conflict of genera or species with the characters assigned as *ordinal* to the including group (e. g., *Pogonias*, *Sciæna*, *Gerres*) the following are examples:

The genus *Dactyloscopus* is referred to the family Blenniidæ, in which the spinous portion of the dorsal fin is said to be "as much developed as the soft, or more." *Dactyloscopus* has in the most evident manner, notwithstanding the erroneous definition of Günther (Catalogue of the Fishes in the British Museum, Vol. III., p. 279), only the first ten to twelve dorsal rays spinous, all the others being articulated. In fact, *Dactyloscopus* has nothing whatever to do with the Blenniidæ, but is very closely related to *Leptoscopus*, and belongs unquestionably to the same group; in other words to an entirely different division of fishes in the Güntherian system. (See Trachinidæ p. 462.)

The genus *Zoarces*, (p. 497) also referred to the family of Blenniidæ, still more disagrees with the true representatives of that family in the structure of the dorsal fin and, as he himself admits, has "no other fin spines" than a few near the caudal; it shows, in fact, an organization similar to that manifested in the family Lycodidæ of Günther, (p. 537) placed by him in a different *order* of fishes—the Anacanthini.

Siphonognathus is a remarkable genus referred to the family of Labridæ. This family is defined as having, in addition to other characters, "the soft anal similar to the soft dorsal, ventral fins thoracic, with one spine and five soft rays," and "branchiostegals five or six." Nothing whatever is said respecting the anal, ventrals, or branchiostegals of *Siphonognathus* and as the necessary data are thus entirely suppressed, it would naturally be assumed that the genus would have the characters attributed to the family. In fact, however, *Siphonognathus* has not the "soft anal similar to the soft dorsal," there are *no* ventral fins, and there are only *four* branchiostegal rays. It will be thus apparent that it would be impossible to identify this fish from Dr. Günther's Introductiou, unless it were assumed that great blunders had been made. This is indeed the case, but it is not safe to assume that the author is an habitual blunderer, and to proceed on that basis, even in the case of Dr. Günther. We are somewhat prepared, however, for the idiosyncrasy exhibited by Dr. Günther, when he compares the relationship of *Siphonognathus* to *Odax* as being similar to that of *Babirussa* to *Sus* (see Catalogue of Fishes in B. M., v. 4, p. 243). Any one who can really entertain such views, and consider the differences between the mammalian genera to be of the same kind or degree as those between the fish genera is unfit to institute comparisons.

Numerous genera are adapted, which, although they may be good, consistency would require Dr. Günther to merge with others. Thus we have *Ptyonotus* (which he has unnecessarily substituted for *Triglopsis* of Girard) retained for a form in the family of Cottidæ (p. 480); this is, however, far more closely related to the "*Cottus quadricornis*" of Günther than are any of his other species of that heterogeneous group. *Pammelas* is still retained as the name of a distinct genus which is allied to *Trachynotus*, although it had been named before Dr. Günther applied his, and its affinities have been well-known for many years to be with *Centrolophus*: it is indeed to a species of that genus (the *C. ovalis*), that the *P. perciformis* is most closely related, and yet in spite of the concurrent testimony of previous ichthyologists we find it injected, in the "Introduction to the Study of Fishes," into a family remote from that to which *Centrolophus* has been referred. As examples of other forms unnaturally separated we may instance (1) *Chætopterus* (p. 390) and *Aprion* (p. 397); (2) *Grystes* (p. 392) and *Huro* (p. 393), and (3) *Auliscops* and *Aulorhynchus* (p. 508). The last type, it may be remarked, is more nearly related to the so-called *Gasterosteus spinachia* than to the Fistulariidæ and should be either referred to the same family or differentiated as a distinct one.

Changes of the names of established genera on trivial pretexts are also indulged in. The name of *Triglopsis* was abandoned for *Ptyonotus* because there was a *Triglops* previously established. Although they are unquestionably much alike, they are sufficiently different, and Steindachner has even lately named a genus *Atherinops*, knowing well that *Atherinopsis* had already been proposed for another genus of the same family. *Dactylopus* is discarded for *Vulsus* because, forsooth, the term DACTYLOPODA had previously been applied by Meyer to a group (not genus) of extinct reptiles. And yet our author himself retains both *Chondrosteus* and Chondrostei etc., without the slightest demur. *Xiphasia* is rejected with an exclamation mark (!) and the yet more objectionable name *Xiphogadus* proposed because the author was dissatisfied with the name, and—we strongly suspect—still more with the namer (Swainson). Why expect any better reason?

The idea is conveyed in the work—and that it has been extensively claimed elsewise by our author is no secret—that all the established genera are admitted in this volume. Without counting the scores of genera that Dr. Günther refuses to recognize, but which every one applying the canons observed by mammalogists and ornithologists would adopt, there are many which even that author could scarcely neglect unless through ignorance. Among those omitted, and which are especially interesting, on account of representing previously unknown types of high value (families or sub-families), or because they throw light on the relations of families in which they belong are: *Elassoma*, *Xenichthys*, *Hoplopagrus*, *Gnathanacanthus*, *Nematistius*, *Grammicolepis*, *Bathymaster*, *Cottunculus*, *Oxylebius*, *Anoplopoma*, *Dactylagnus*, *Myxodagnus*, *Anarrhichthys*, *Plagiotremus*, *Chænopsis*, *Nematocentris* and *Protistius*. If he had really known *Hoplopagrus* (referred to incidentally on page 279, but not otherwise noticed), he, *perhaps*, would not have so far separated his "*Percidæ*" (pp. 375-379) and "*Sparidæ*" (405-410), as he has done: if he had known *Cottunculus* he would, *perhaps*, have recognized the affinity of *Psychrolutes* to the *Cottidæ*, and not isolated it as the type of a remote family—at least no scientific ichthyologist would have failed to so profit by the knowledge. The work of Bleeker, Steindachner, Klunzinger, Lütken, Vaillant, Sauvage, Giglioli and Collett in Europe, and that of all American ichthyologists has, however, been almost of nought so far as Dr. Günther is concerned. It need be only remarked, in connection with the latter, that of the numerous genera of Etheostomine fishes only *Pileoma* (*Percina*) and *Boleosoma* (p. 379) are recognized. The reason therefore is no secret—they are too small, and as they have not been able to grow larger, they do not deserve to be considered. The interesting relations, physiological and morphological, that they present are not sufficient to outweigh this cogent objection. Among American fishes there is no group that has been so much written about and that is better known than the genus *Micropterus*, but notwithstanding Dr. Günther has not yet learned that he has distributed its well defined representatives under three genera, nor that *Huro* was based on a mistake and is not a valid genus, nor that there are two, and only two, well-determined species, and those two can not be generically distinguished. When it is further remarked that only three genera are recognized

for the Centrarchines and Lepomines, and that these are diagnosed by the least important and most fallacious characters, and that thereby the species are thrown into almost inexplicable confusion, some idea may be formed of the unreliability of the work.

The general anatomical portion of the work is, on the whole, really a tolerably good *résumé* of facts respecting the structure and organization of fishes, for the author has wisely followed Gegenbaur, Huxley and Parker without sufficient deviation to fall into much error. One great objection to it, however, is the undue prominence given to the peculiarities of the teleostean types and the exhibition of them in such a manner as to prevent the reader's conception of the range of variation in the forms treated of, and especially as to the taxonomic value of such variations. In this connection too, we may notice the reproduction of some rather strange views. Thus, it is said that "the numbers of the dorsal and anal rays give good specific, generic, or even family characters," except when greatly increased, while "the taxinomic [taxonomic] value of this character becomes uncertain. The numbers of the pectoral and caudal rays are rarely of any account" (p. 44). The last remark embodies a striking illustration of the length to which Dr. Günther's neglect carries him in contempt of the facts. Far from the number of the completely developed caudal rays being of no account, there are rarely deviations in the number in related forms, and when such prevail they generally accompany other decided modifications of structure and are available for major diagnostic purposes, as Bleeker has observed. Again, it is claimed of the pectoral limb that the structure of that of *Ceratodus* "*evidently*" represents one of its first and lowest conditions" (p. 74). So far is this from being "evident" that it is difficult to understand how any one familiar with the stucture and development of the limb in the Selachians and related types, and conversant with the logic of science could entertain for one moment such an opinion and, on the contrary, not look upon the Ceratodontoid limb as an extreme deviation from the primitive type. But the very climax of absurdity and unscientific comparison is exemplified in the case of *Ceratodus* by the homologisation of the basal segment of the axis of the pectoral *fin* (not that which supports it) with the basal cartilage of the Sturgeon, and which itself is the source of several other errors (pp. 74, 76). A comparison of the pectoral limbs of *Ceratodus* and *Polypterus* would be sufficient to prevent any scientific naturalist from making such a blunder. We need not dwell further on such defects but in connection with the systematic portion, we cannot omit to notice that Dr. Günther recognizes that in the Chondropterygians there are no bones representing the membrane bones of the skull of the Ganoid and higher fishes; that at the most there are simply "rudimentary maxillary elements" (p. 69); that the scapular arch "is formed by a single coracoid cartilage" (p. 69); that "the same type of branchial organs [as in the Cyclostomes] persists in *Chondropterygians*, which possess five, rarely six or seven, flattened pouches with transversely plaited walls," each pouch opening "outwards, and by an aperture into the pharynx, without intervening ducts" (p. 137); and that an "air bladder is absent but occurs in all Ganoids," etc. (p. 141), and that the generative organs are very peculiar (p. 166). Yet in spite of all these differences, in face of the recognized similarity between the teleosteoid Ganoids (*Amia*, &c.) and certain Physostomes, and in ignorance of the evanescence of the characters designed to differentiate the Teleosts, he adheres to the combination of the Ganoids with the Chondropterygians in one sub-class—the Palæichthyes. It is indeed a "singular concurrence" of characters (p. 312)—but not of important ones—that is employed to segregate this group, for not one is common to all the members included in it, and at the same time exclusive of other types. A knowledge of the anatomical labors of recent biologists would have instructed him as to this fact. The "Sub-class Palæichthyes" is indeed, as has been said by a recent well qualified judge, "a triumph of systematic *gaucherie*." The group in fact is the outcome of a confusion of ideas respecting generalized characters and extravagant valuation of certain facts entitled to consideration but by no means to anything like the extent admitted.

Quite as inscrutable as his Morphology is Dr. Günther's Physiology. As we turn the pages of the Introduction we come across strange assertions respecting the functions connected with structural peculiarities. Several of these may be taken as examples.

The power of ejecting from the mouth drops of water to some distance, and with such force as to dislodge insects and precipitate them into the water, has been attributed to more than one Javanese fish, but whether the real shooter was a *Chelmo*, a *Toxotes*, or an *Epibulus*, or each one, (or even whether any actually had such power), seems to have become doubtful. Skepticism as to any case might have been legitimate, but Dr. Günther unqualifiedly asserts that as to *Chelmo* "*this statement is erroneous*," and that the feat "*is practised* by another fish of this family (*Toxotes*). The long slender bill of *Chelmo* (which is a true salt-water fish) rather enables it to draw from holes and crevices animals which otherwise could not be reached by it" (p. 399). *Toxotes* has an unusually deeply cleft mouth, and one less fitted to perform such a feat as that in question could scarcely be found. The inaptness of the structure to the alleged function might well evoke skepticism in anyone, and this being once excited, the literature respecting the several fishes which have been named ejaculators will demonstrate that (1) there is no *observational* basis for the attribution of blowing drops of water to the *Toxotes*, and (2) there have been observations (by Hommel, Reinwardt and Mitchell), of a certain kind, of ejaculatory feats by *Chelmo*. In fact, if it is conceded that the feat is performed by a fish, in the sentences repeated from Dr. Günther, there are concentrated seven distinct errors: (1) denial in spite of evidence, (2) affirmation without sufficient basis, (3) denial in face of (comparative) adaptation of structure to function, (4) credulity in spite of inaptness of structure to function, (5) gratuitous assumption of a function—"to draw from holes and crevices animals which could not otherwise be reached by it," (6) the assumption, by implication, that the Archer was not a salt-water type, although the first observer (Hommel) especially stated that it was a sea-fish, and (7) erroneous taxonomy in the association of *Toxotes* in the same family with *Chelmo*. Almost all possible kinds of errors have thus culminated in this single case.

An instance of another gratuitous assumption respecting a function, refers to a Sciænoid fish.

The genus "*Collichthys* Günther" (previously named *Sciænoides* by Blyth) is distinguished by a "great development of the muciferous system on the head and the small eye," and this characteristic "leads one [and but one—Dr. Günther alone] to suppose that these fishes live in muddy water near the mouths of large rivers" (p. 430). What teleological relation there is between muciferous channels and small eyes and the muddy water of large (or any kind of) rivers, Dr. Günther has not vouchsafed to inform us. That such characteristics do not usually indicate the conditions suggested, is admitted by Dr. Günther himself, for he has recognized that "the muciferous system of many deep-sea fishes is developed in an extraordinary degree" (p. 300), and that a large portion of the deep-sea forms are characterized by small eyes (pp. 300-301). The fact is that instead of the inference in question being the outcome of a consideration of the structure indicated, it is the result of data concerning the habitat of one species of the genus and the desire to connect the structure with some function, however irrelevant. It is recorded in the "Catalogue of the Acanthop-

terygian Fishes in the British Museum" (v. 2, p. 316)—a work which has served as the basis of the "Introduction to the Study of Fishes"—that the "*Collichthys pama*" inhabits the "Bay of Bengal, entering rivers." The statement given as a deduction is therefore really a co-ordination—and an entirely sophistical one—of the ascertained structural peculiarity and the habitat of that species.

One other characteristic deduction, also relating to a Sciænoid type, may be noticed because of its interest to American students.

The "Drum" of the Atlantic (*Pogonias chromis*) is especially mentioned in connection with "the extraordinary sounds which are produced by it and other allied Sciænoids." "It is [says Dr. Günther] still a matter of uncertainty by what means the "Drum" produces the sounds. Some naturalists believe that it is caused by the clapping together of the pharyngeal teeth, which are very large molar teeth. However, if it be true [sage proviso!] that the sounds are accompanied by a tremulous motion of the vessel, it seems more probable that they are produced by the fishes beating their tails against the bottom of the vessel in order to get rid of the parasites with which that part of their body is infested." In this paragraph are several illegitimate assumptions and inferences which a slight knowledge of the literature respecting the subject would have prevented. (1) The sounds are entirely independent of "vessels." (2) There was no reason to suppose that the fish in question was more infested with parasites in the tail than any other. (3) The statement that "allied Sciænoids" (and this is especially true of the closely related fresh-water sheepshead, or *Haploidonotus*, referred by Günther to a genus with which it has not the slightest affinity!) produce similar sounds was for the moment forgotten. (4) The co-ordination of facts and phenomena rendered it unnecessary to look to such source for solution. (5) The source indicated was one of the most improbable that could be conceived. There is, indeed, ample cause for surprise that any educated ichthyologist could suppose that a fish would agitate its tail in the manner suggested to relieve a spasmodic pain, such as is postulated by the explanation given. Our author's credence in the allegation that the sounds produced are "accompanied by a tremulous motion of the vessel," was, as we have seen, sufficient to impel him to substitute a most improbable for at least a probable hypothesis.

A mistake of another kind is made respecting the Rays. It is said that "the majority are *oviparous*" (p. 336). As was long ago recognized by Müller and Henle, the Raiidæ are the only oviparous rays; Gunther includes them all in one family and four genera, and admits about 35 species. All the others recorded by him, so far as known, are viviparous; they number, in his opinion, five families, twenty genera and more than 100 species, consequently the majority are *viviparous!*

Whether a work so abounding in errors that we are only able to specify a few as examples and hint at some kinds of others is worth acquiring must be left to the reader to judge. As a curiosity in taxonomical literature it certainly *is*, but for such purposes as are most desirable —correct information and identification of genera—it as certainly *is not*. THEO. GILL.

DESCARTES AND THE BAROMETRIC THEORY.—At one of the late sittings of the Academy of Moral and Political Science, M. Nourisson made an extremely interesting communication relative to a letter of Descartes, in which the great philosopher clearly indicates the principal of atmospheric pressure, twelve years before Toricelli's experiments on the barometer. Toricelli constructed the fast barometric tube in 1643; in 1647 Pascal accomplishes his celebrated experiments of Puy-de-Dôme and of the "Tour Saint Jaques." It would appear that Descartes had suggested to the author of *Pensées* the idea of this mode of experiment.

CORRESPONDENCE.

To the Editor of SCIENCE:

ON ETHER.

There are two theories in regard to ether, one of which assumes that it is a discontinuous medium, that is, a medium composed of particles at enormous distances apart, as compared with their diameters.

In this theory ether is spoken of or defined as an "imponderable elastic medium." If we examine the above definition we find several inconsistencies. To begin with, an imponderable body is a body without weight. Now the weight of a body, is the result of the mutual attraction, exerted between it and some other body; in other words, weight is the effect of gravitation. Now as every particle attracts every other particle with a force, that is directly as the mass, and inversely as the square of the distance between them, an imponderable body must be one in which the mass is zero, or that is at such a distance from every other body that the reciprocal of the square of this distance is zero. The last supposition is of course absurd.

Now the mass of a body is equal to the product of its volume and density, or $M=d\ V$ and if M is equal to zero, either d or V must be zero and as it would be impossible to conceive of a body that occupies no space, we must think of d as equal to zero, or in other words an imponderable body is simply a portion of space. This same theory assumes that radiant energy is transmitted by means of the moving particles of ether, *i. e.*, one particle moving with a certain velocity, strikes another and imparts some of its energy to it and this flying off strikes another and so on. But the momentum of a body is expressed by $M\ V$ and its energy by $\frac{M V^2}{2}$ (V=Velocity), making M equal to zero, as we must if the particles are imponderable, we have $O\ V=M_0=o$ and $\frac{O\ V^2}{2}=E=o$, hence the transmission of radiant energy by an imponderable substance, composed of particles is an impossibility. If we assume that the particles are effected by gravitation, then at once it is evident that the ether could not be of equal density throughout the universe, for around each celestial body there would be an atmosphere of ether which would gradually decrease in density from the surface of the body outwards.

By elasticity in the above definition, is meant that property of matter, possessed by gases in the highest degree, of having its volume or density changed by some force and regaining its former state when the original condition are again imposed. When a gas is compressed, the mean free path of the molecules is shortened and the compressibility is dependent upon the length of the mean free path. When the pressure is removed, the gas expands, the expansion being due to a conversion of the energy of vibratory motion of the molecules or heat into energy of translation. If the ether is elastic, then of course with a change from less to greater density the particles must be moved nearer together, and the compressibility will be dependent upon the average distance between the particles. When a change from greater to less density takes place, the particles must be moved farther apart and the explanatory reason given for this expansion is that the energy of the moving particles causes the expansion.

From what has been said in regard to imponderability, it is evident that a discontinuous imponderable elastic substance is an impossibility according to the present ideas of dynamics. The transmission of radiant energy by a discontinuous ether, if the particles are ponderable, is possible in two ways, 1st, By an alternate rarefaction, and condensation of the ether, similar to the manner in which sound is transmitted through air. 2d, By the

movements of individual particles. If the first is true, the same conditions must apply to the transmission of radiant energy, as to the propagation of sound. Sound travels through air with a velocity of 330 miles per second, at 0° cent.

Taking for an example a sound produced by a body vibrating 20,000 times per second and dividing the velocity of sound by this number, we have as the wave length 16.5 mm. Clausius has shown that the mean free path of an oxygen molecule is 5000 times its diameter. Taking $\frac{1}{5 \times 10^7}$ mm. as the diameter, we have 10^4 mm. as the mean free path of an oxygen molecule, and dividing the length of the sound wave calculated above, by this number we have 1665×10^4, or the length of the wave of this extremely high note, is 1665×10^4 times longer than the mean free path of an oxygen molecule, hence it is evident that the propagation of sound is dependent primarily upon the movement of aggregates of molecules.

The elasticity of the ether is assured to be many times greater than that of the most perfect gas. Assuming that it is 1000 times more elastic than oxygen gas, the average distance between the surfaces of the particles must be 1000 times greater than the average distance between the surfaces of the oxygen molecules.

Taking as the mean free path of an oxygen molecule, 10^4 mm., the distance between the particles of ether would be .1 mm. Now the wave length of a certain ray of red light is .000,609 mm., hence the average distance between the particles is 164 times as great as this particular wave length. It follows from this, that the transmission of radiant energy, through such an elastic medium as the ether, cannot be in any way comparable with the propagation of sound through air. If the energy is not transmitted in this manner, then it must be transmitted in the second way, *i. e.*, by the movements of individual particles. But with an ether as elastic as generally assumed, this is impossible, since the average distance between the particles is 164 times as great as the length of a comparatively long undulation, and it would be absurd to say that a vibrating molecule could, by impact with a particle of ether, send the particle 5000 times the diameter of the molecule, and further, that the particle would return from this long journey in time for the next vibration. Even assuming that the particles of ether could move fast enough to accomplish this movement in each vibration, then if the molecules are circular, the particle would have to return in a line that was normal to the surface of the molecule at the point of contact, or it would fly off in another direction after impact with the molecule, and as the particles are so far apart, as compared with the diameters of the molecules, if one particle was driven off there would be no other to take its place. There would also be required a series of particles in a straight line between the body receiving and the body radiating energy.

But it is needless to enlarge upon this method of transmitting radiant energy, for the constant length of undulation and undulatory motion itself, would be impossible in a medium, in which the average distance between the particles was 164 times as long as an undulation.

The only discontinuous medium through which radiant energy *could* be transmitted, would be one in which the average distance between the particles was a small fraction of an undulation. But in a medium of this sort there would be hardly any chance for compression, much less than in oxygen gas, and to assume that ether is less elastic than a gas, is contrary to the theory of discontinuous ether. As a discontinuous ether will not answer the requirements, we must, if we assume *any* ether, assume a continuous one. By means of a continuous ether all the phenomena of light can be explained. One is inclined, however, to apply to a continuous ether the same reasoning as is applied to matter. But as ether is not matter, we cannot with justice attribute to it any of the properties of matter except extension and elasticity, and till we are much farther advanced in our knowledge of the universe, it will be impossible to say anything about ether, except to assume its existence and its continuity. B.

THE COMET.

The comet was seen from this Observatory at 14h. 30m., June 22, 1881. The latitude of the place is 41° 13′; longitude from Washington in time, 53m. 48s. This longitude is approximate, as we have no transit, and being without a correct astronomical clock, are continually annoyed for want of true time. The latitude is somewhat indeterminate, as the declination circle has no Vernier, reading seconds. The telescope (a fine 6 inch Alvan Clark & Sons), is not precisely in the meridian, and we are unable to place it there with accuracy, having no micrometer. With all these hindrances, the adjustment is such that catalogued stars are always in field with power of 60, but in many cases fail to come to the centre or line of collimation. When observation was first made the declination was 43° 10′, then make δ=the declination = 43° 10′, and λ = the latitude of New Windsor = 41° 13′, and take:

log. tan. δ = - - - -	9.972,188
log. tan. λ = - - - -	9.942,478
log. sin. 55° 15′= - -	9.914,666

which being converted into time = 3h. 41m.; and 6h. — 3h. 41m. = 2h. 19m., A. M., June 23, mean local time in New Windsor, or time of comet's rising, that is, of the nucleus. The tail being several degrees long and direected towards Polaris, was above the horizon some time before.

Had the horizon been water, the nucleus would have come in sight at 2.19, as it was, an interval of 11m. was required to bring it above undulations of the earth. We thought best not to telegraph before seeing the nucleus, but as soon as we positively knew the apparition to be a comet, haste was made to send dispatch. The village is on a branch railroad and telegraph offices are not open nights, so we had to send to the residence of the operator, arouse and engage him to go to the office and send telegram. This took time, and it was not until after 3 A. M. that message was sent. Meanwhile we endeavored to place telescope on nucleus but were unable to, as there was a tree in range, causing another delay until 3.30, when observation was made—the nucleus being an hour above horizon and in apparent

R. A. - - - - - -	5h. 34m,
δ - - - - - - -	43° 10′

a rough position, as no corrections were made for refraction or parallax.

The telegram read: "Vast comet in northeastern heavens." After mature consideration we regret using so many words, one only—"Comet,"—was all that was necessary, when the acute observer, Swift, would have been on the alert at once. Before sunrise we were favored with 30 minutes good definition, when two envelopes were seen, the nucleus extending a bridge to the external surface of the inner one. Since, the nucleus has changed form, is no longer round, but has prolonged into a beak-shaped mass, and looks like Comet III., 1862, August 29, as drawn by Challis (Chambers' Astronomy, p. 322). The cometary matter is of great tenuity, as it was seen to run over a sixth magnitude star at 10h. June 28, which passed about 15″ from nucleus, yet it was visible through the immense volume of gas.

The comet was seen from many points in the Western States twenty-four hours before noticed at this place, by steamboat hands, street-car drivers, railroad conductors, night-watchmen, policemen and many others whose business required them to be out all night.

NEW WINDSOR, ILL., *July 1st*, 1881. EDGAR L. LARKIN.

SCIENCE:

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JOHN MICHELS, Editor.

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We are indebted to Professor Edward S. Holden for a series of seven interesting drawings of the recently discovered Comet; they are now being engraved and will appear in "SCIENCE" next week.

These drawings were made by Professor Holden from observations made with the 15-inch equatorial of the Washburn Observatory.

We have received a copy of the instructions furnished to the officers in command of the expeditionary force to Lady Franklin Bay, which appear to have given general satisfaction, and probably suffice for all the purposes of the expedition. Still we regret to find that the services of a naturalist have not been considered requisite, and that no provision appears to have been made for collecting specimens and information respecting the Fauna and Flora of the Polar regions. A microscope is not even added to the list of apparatus provided for the use of the expedition.

Mr. Alfred Russell Wallace in his last work, "Island Life," observes that there is an enormous waste of labor and money with comparatively scanty and unimportant results to natural history, of most of the great scientific voyages of the various civilized governments during the present century. All these expeditions combined have done far less than private collectors in making known the products of remote lands and islands. They have brought home, he asserts, fragmentary collections, made in widely scattered localities and these have been usually described in huge folios, whose value is often in inverse proportion to their bulk and cost. The same species have been collected again and again, often described several times, and, not infrequently stated to be from places they never inhabited. The result of this wretched system, says Mr. Wallace, is, that the productions of some of the most frequently visited and most interesting islands on the globe are still very imperfectly known, while their native plants and animals are being yearly exterminated. The remedy suggested by Mr. Wallace, is that resident naturalists at a very small annual expense, should be appointed, who, he considers, would do more for the advancement of knowledge in this direction, than all the expensive expeditions which have again and again circumnavigated the globe.

We are of course aware that most of the many recent expeditions to the polar regions have been specially organized for the promotion of the physical sciences, but the value of an expert naturalist on such occasions should not be neglected, and wherever permanent stations are established the naturalist may be expected to do good work, and even occasionally interpret natural phenomena which are sometimes inexplicable to the physicist.

The comet has been observed here (with the exception of June 27) on every night since June 23, although clouds have often considerably hindered the work.

In addition to the measurements of position, the light of different parts of the comet has been photometrically determined. This work, very probably, has been undertaken only at this Observatory. The instrument employed for the purpose is one which has already been extensively used here for measuring the light of nebulæ. The results of these observations are expressed in stellar magnitudes on Pogson's logarithmic scale, regarding the light of a star of the given magnitude as diffused over a circle 1′ in diameter, the brightness of which would then be equal to that of the observed portion of the nebula or comet. On the first five nights of the present month, various parts of the coma and tail have thus been observed. The result, from a provisional reduction, is as follows:

Coma,	0.′5	south of nucleus,	magnitude	6.9
"	0.′5	north of "	"	7.8
Tail,	0.°5	" "	"	9.6
"	1.°0	" "	"	10.3
"	2.°0	" "	"	11.0
"	3.°0	" "	"	11.2
"	4.°0	" "	"	11.6

I add the corresponding results, also from provisional reductions, for some other comets and nebulæ:

Palisa's Comet, 1879 *d*.	magnitude	8
Comet, 1880 *d*.	"	7
Webb's Planetary Nebula, DM. + 41°4004	"	4.7
Brightest part of great nebula in Orion (20 points in which have been observed), ...	"	8.0
Nebula G. C. 4487, ...	"	11.2
" G. C. 4802, ...	"	11.3

On June 28th, and on July 1, 3, 5 and 6, the coordinates of a number of points in the border of the comet's tail were observed for the purpose of determining its form.

EDWARD C. PICKERING.

HARVARD COLLEGE OBSERVATORY,
Cambridge, U. S., *July 6th*, 1881.

THE UNITY OF NATURE.

By the Duke of Argyll.

IX.

THE ORIGIN OF RELIGION CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(*Continued.*)

The considerations set forth in the previous chapter indicate the fallacies which lie in our way when we endeavor to collect from the worship of savage nations any secure conclusions as to the origin of Religion. Upon these fallacies, and upon no more safe foundation, Comte built up his famous generalization of the four necessary stages in the history of Religion. First came Fetishism, then Polytheism, and then Monotheism, and last and latest, the heir of all ages, Comtism itself, or the Religion of Humanity which is to be the worship of the future.

Professor Max Müller has done admirable service in the analysis and in the exposure which he has given us of the origin and use of the word "Fetishism," and of the theory which represents it as a necessary stage in the development of Religion.[1] It turns out that the word itself and the fundamental idea it embodies, is a word and an idea derived from one of those popular superstitions which are so common in connection with Latin Christianity. The Portuguese sailors who first explored the West Coast of Africa were themselves accustomed to attach superstitious value to beads, or crosses, or images, or charms and amulets of their own. These were called "fetiços." They saw the negroes attaching some similar value to various objects of a similar kind, and these Portuguese sailors therefore described the negro worship as the worship of "fetiços." President de Brosses, a French philosopher of the Voltairean epoch in literature, then extended the term Fetish so as to include not only artificial articles, but also such great natural features as trees, mountains, rivers and animals. In this way he was enabled to classify together under one indiscriminate appellation many different kinds of worship and many different stages in the history of religious development or decay. This is an excellent example of the crude theories and false generalizations which have been prevalent on the subject of the origin of Religion. First, there is the assumption that whatever is lowest in savagery must have been primeval—an assumption which, as we have seen, is in all cases improbable, and in many cases must necessarily be false. Next there is great carelessness in ascertaining what is really true even of existing savages in respect to their religious beliefs. It has now been clearly ascertained, that those very African negroes whose superstitious worship of material articles supposed to have some mysterious powers or virtues, is most degraded, do nevertheless retain behind and above this worship certain beliefs as to the nature of the Godhead, which are almost as far above their own abject superstitions as the theology of a Fénélon is above the superstitions of an ignorant Roman Catholic peasant. It is found that some African tribes have retained their belief in one Supreme Being, the Creator of the world, and the circumstance that nevertheless no worship may be addressed to Him has received from Professor Max Müller an explanation which is ample. "It may arise from an excess of reverence quite as much as from negligence. Thus the Odjis or Cohantis call the Supreme Being by the same name as the sky; but they mean by it a Personal God, who, as they say, created all things and is the Giver of all good things. But though He is omnipresent and omniscient, knowing even the thoughts of men, and pitying them in their distress, the government of the world is, as they believe, deputed by Him to inferior spirits, and among these, again, it is the malevolent spirits only who require worship and sacrifice from man."[2] And this is by no means a solitary case. There are many others in which the investigations of missionaries respecting the religious conceptions of savage nations have revealed the fact that they have a much higher theology than is indicated in their worship.

The truth is, that nowhere is the evidence of development in a wrong direction so strong as in the many customs of savage and barbarous nations which are more or less directly connected with Religion. The idea has long been abandoned that the savage lives in a condition of freedom as compared with the complicated obligations imposed by civilization. Savages, on the contrary, are under the tyranny of innumerable customs which render their whole life a slavery from the cradle to the grave. And what is most remarkable is the irrational character of most of these customs, and the difficulty of even imagining how they can have become established. They bear all the marks of an origin far distant in time—of a connection with doctrines which have been forgotten, and of conceptions which have run, as it were, to seed. They bear, in short, all the marks of long attrition, like the remnants of a bed of rock which has been broken up at a distant epoch of geological time, and has left no other record of itself than a few worn and incoherent fragments in some far-off conglomerate. Just as these fragments are now held together by common materials which are universally distributed, such as sand or lime, so the worn and broken fragments of old religions are held together, in the shape of barbarous customs, by those common instincts and aspirations of the human mind which follow it in all its stages, whether of growth or of decay.

The rapidity of the processes of degradation in Religion, and the extent to which they may go, depends on a great variety of conditions. It has gone very far indeed, and has led to the evolution of customs and beliefs of the most destructive kind among races which, so far as we know, have never been exposed to external conditions necessarily degrading. The innate character of this tendency to corruption, arising out of causes inherent in the nature of Man, becomes indeed all all the more striking when we find that some of the most terrible practices connected with religious superstition, are practices which have become established among tribes which are by no means in the lowest physical condition, and which inhabit countries highly blest by Nature. Perhaps there is no example of this phenomenon more remarkable than the "customs" of Dahomey, a country naturally rich in products, and affording every facility for the pursuits of a settled and civilized life. Yet here we have those terrible beliefs which demand the constant, the almost daily sacrifice of human life, with no other aim or purpose than to satisfy some imaginary Being with the sight of clotted gore, and with the smell of putrefying human flesh. This is only an extreme and a peculiarly terrible example of a general law, the operation of which is more or less clearly seen in every one of the religions of the heathen world, whether of the past or of the present time. In the very earliest ages in which we become acquainted with the customs of their worship, we find these in many respects strange and unaccountable, except on the supposition that even then they had come from far, and had been subject to endless deviations and corruptions through ages of a long descent.

Of no Religion is this more true than of that which was associated with the oldest civilization known to us—the civilization of Egypt. So strange is the combination here of simple and grand conceptions with grotesque symbols and with degrading objects of immediate wor-

[1] Hibbert Lectures, 1878.

[2] Hibbert Lectures, pp. 107, 108.

ship, that it has been the inexhaustible theme of curious explanations. Why a Snake or why a Dung-beetle should have been taken to represent the Divine Being, and why in the holiest recess of some glorious temple we find enshrined as the object of adoration the image or the coffin of some beast, or bird, or reptile, is a question on which much learned ingenuity has been spent. It has been suggested, for example, that a conquering race, bringing with it a higher and a purer faith, suffered itself to adopt or to embody in its system the lower symbolism of a local worship. But this explanation only removes the difficulty—if it be one—a step further back. Why did such sufferance arise? why was such an adoption possible? It was possible simply because there is an universal tendency in the human mind to developments in the wrong direction, and especially in its spiritual conceptions to become more and more gross and carnal.

Nor is it difficult to follow some, at least, of the steps of consequence—that is to say, the associations of thought —by which worship may become degraded when once any serious error has been admitted. Animal worship, for example, may possibly have begun with very high and very profound conceptions. We are accustomed to regard it as a very grotesque and degraded worship, and so no doubt it was in its results. But if we once allow ourselves to identify the Divine Power in Nature with any of its operations, if we seek for the visible presence of the Creator in any one of His creations, I do not know that we could choose any in which that presence seems so immanent as in the wonderful instincts of the lower animals. In a previous chapter we have seen what knowledge and what foreknowledge there is involved in some of these. We have seen how it often seems like direct inspiration that creatures without the gift of reason should be able to do more than the highest human reason could enable us to do—how wonderful it is, for example, that their prevision and provision for the nurture and development of their young should cover the whole cycle of operations in the second work of creation which is involved in the metamorphoses of insects—all this, when we come to think of it, may well seem like the direct working of the Godhead. We have seen in a former chapter that men of the highest genius in philosophical speculation, like Descartes, and men of the highest skill in the popular exposition of scientific ideas, like Professor Huxley, have been led by these marvels of instinct to represent the lower animals as automata or machines. The whole force and meaning of this analogy lies in the conception that the work done by animals is like the work done by the mechanical contrivances of men. We look always upon such work as done not by the machine but by the contriving mind which is outside the machine, and from whom its adjustments are derived. Fundamentally, however little it may be confessed or acknowledged, this is the same conception which, in a less scientific age, would take another form. What is seen in the action of an automaton is not the mechanism but the result. That result is the work of mind, which seems as if it were indwelling in the machine. In like manner, what is seen in animals is the wonderful things they do; and what is not seen, and is indeed wholly incomprehensible, is the machinery by which they are made to do it. Moreover, it is a machinery having this essential distinction from all human machines, that it is endowed with life, which in itself also is the greatest mystery of all. It is, therefore, no superficial observation of animals, but, on the contrary, a deep pondering on the wonders of their economy, which may have first suggested them to religious men as at once the type and the abode of that Agency which is supreme in Nature. I do not affirm as an historical fact that this was really the origin of animal worship, because that origin is not historically known, and, like the origin of Religion itself, it must be more or less a matter of speculation. Some animals may have become objects of worship from having originally been the subjects of sacrifice. The victim may have been so associated with the god to whom it was devoted as to become his accepted symbol. The Ox and the Bull may well have been consecrated through this process of substitution. But no such explanation can be given in respect to many animals which have been worshipped as divine. Perhaps no further explanation need be sought than that which would be equally required to account for the choice of particular plants, or particular birds and fishes, as the badges of particular tribes and families of men. Such badges were almost universal in early times, and many of them are still perpetuated in armorial bearings. The selection of particular animals in connection with worship would be determined in different localities by a great variety of conditions. Circumstances purely accidental might determine it. The occurrence, for example, in some particular region of any animal with habits which are at once curious and conspicuous, would sufficiently account for the choice of it as the symbol of whatever idea these habits might most readily suggest or symbolize. It is remarkable, accordingly, that in some cases, at least, we can see the probable causes which have led to the choice of certain creatures. The Egyptian beetle, the Scarabæus, for example, represents one of those forms of insect life in which the marvels of instinct are at once very conspicuous and very curious. The characteristic habit of the Scarabæus beetle is one which involves all that mystery of prevision for the development of the species which is common among insects, coupled with a patient and laborious perseverance in the work required, which does not seem directly associated with any mere appetite or with any immediate source of pleasure. The instinct by which this beetle chooses the material which is the proper nidus for its egg, the skill with which it works that material into a form suitable for the purpose, and the industry with which it then rolls it along the ground till a suitable position is attained—all these are a striking combination of the wonders of animal instinct, and conspicuous indication of the Spirit of wisdom and of knowledge which may well be conceived to be present in their work.

But although it is in this way easy to imagine how some forms of animal-worship may have had their origin in the first perception of what is really wonderful, and in the first admiration of what is really admirable, it is also very easy to see how, when once established, it would tend to rapid degradation. Wonder and reverence are not the only emotions which impel to worship. Fear, and even horror, especially when accompanied with any mystery in the objects of alarm, are emotions suggesting, perhaps, more than any, that low kind of worship which consists essentially in the idea of deprecation. Some hideous and destructive animals, such as the crocodile, may have become sacred objects neither on account of anything admirable in their instincts, nor on account of their destructiveness; but, on the contrary, because of being identified with an agency which is beneficent. To those who live in Egypt the Nile is the perennial source of every blessing necessary to life. An animal so characteristic of that great river may well have been chosen simply as the symbol of all that it was, and of all that it gave to men. There is no mystery, therefore, in the crocodile being held sacred in the worship of the God of Inundation. But there are other animals which have been widely invested with a sacred character, in respect to which no such explanation can be given. The worship of serpents has been attributed to conceptions of a very abstract character—with the circle, for example, into which they coil themselves, considered as an emblem of Eternity. But this is a conception far too transcendental and far-fetched to account either for the origin of this worship or for its wide extension in the world. Serpents are not the only natural objects which present circular forms. Nor is this attitude of their repose,

curious and remarkable though it be, the most striking peculiarity they present. They have been chosen, beyond any reasonable doubt, because of the horror and terror they inspire. For this, above all other creatures, they are prominent in Nature. For their deceptive coloring, for their insidious approach, for their deadly virus, they have been taken as the type of spiritual poison in the Jewish narrative of the Fall. The power of inflicting almost immediate death, which is possessed by the most venomous snakes, and that not by violence but by the infliction of a wound which in itself may be hardly visible, is a power which is indeed full of mystery even to the most cultivated scientific mind, and may well have inspired among men in early ages a desire to pacify the powers of evil. The moment this becomes the great aim and end of worship, a principle is established which is fertile in the development of every foul imagination. Whenever it is the absorbing motive and desire of men to do that which may most gratify or pacify malevolence, then it ceases to be at all wonderful that men should be driven by their religion to sacrifices the most horrid, and to practices the most unnatural.

But if we wish to see an illustration and an example of the power of all conceptions of a religious nature in the rapid evolution of unexpected consequences, we have such an example in the case of one man who has lived in our own time, and who still lives in the school which he has founded. I refer to Auguste Comte. It is well known that he denied the existence, or at least denied that we can have any knowledge of the existence, of such a Being as other men mean by God. Mr. John Stuart Mill has insisted with much earnestness and with much force that, in spite of this denial, Auguste Comte had a religion. He says it was a religion without a God. But the truth is, that it was a religion having both a creed and an ideal object of worship. That ideal object of worship was an abstract conception of the mind so definitely invested with personality that Comte himself gave to it the title of The Great Being (*Grand Etre*). The abstract conception thus personified was the abstract conception of Humanity—Man considered in his past, his present, and his future. Clearly this is an intellectual Fetish. It is not the worship of a Being known or believed to have any real existence; it is the worship of an idea shaped and molded by the mind, and then artificially clothed with the attributes of personality. It is the worship of an article manufactured by the imagination, just as Fetishism, in its strictest meaning is the worship of an article manufactured by the hand. Nor is it difficult to assign to it a place in the classification of religions in which a loose signification has been assigned to the term Fetishism. The worship of Humanity is merely one form of animal-worship. Indeed, Comte himself specially included the whole animal creation. It is the worship of the creature Man as the consummation of all other creatures, with all the marvels and all the unexhausted possibilities of his moral and intellectual nature. The worship of this creature may certainly be in the nature of a religion, as much higher than other forms of animal worship as Man is higher than a beetle, or an ibis, or a crocodile, or a serpent. But so also, on the other hand, it may be a religion as much lower than the worship of other animals, in proportion as man can be wicked and vicious in a sense in which the beasts cannot. Obviously, therefore, such a worship would be liable to special causes of degradation. We have seen it to be one of the great peculiarities of Man, as distinguished from the lower animals, that whilst they always obey and fulfill the highest law of their being, there is no similar perfect obedience in the case of Man. On the contrary, he often uses his special powers with such perverted ingenuity that they reduce him to a condition more miserable and more degraded than the condition of any beast. It follows that the worship of Humanity must, as a religion, be liable to corresponding degradation. The philosopher, or the teacher, or the prophet who may first personify this abstract conception, and enshrine it as an object of worship, may have before him nothing but the highest aspects of human nature, and its highest aspirations. Mill has seen and has well expressed the limitations under which alone such a worship could have any good effect. "That the ennobling power of this grand conception may have its full efficacy, he should, with Comte, regard the *Grand Etre*, Humanity or Mankind, as composed in the past solely of those who, in every age and variety of position, have played their part worthily in life. It is only as thus restricted that the aggregate of our species becomes an object worthy our veneration."[3] This, no doubt, was Comte's own idea. But how are his disciples and followers to be kept up to the same high standard of conception? Comte seems to have been personally a very high-minded and a very pure-minded man. His morality was austere, almost ascetic, and his spirit of devotion found delight in the spirit of the Christian Mystics. Yet even in his hands the development of his conceptions led him to results eminently irrational, although it cannot be said that they were ever degrading or impure. But we have only to consider how comparatively rare are the examples of the the highest human excellence, and how common and prevailing are the vices and weakness of Humanity, to see how terrible would be the possibilities and the probabilities of corruption in a religion which had Man for the highest object of its worship. Nor is this all that is to be said on the inevitable tendency to degradation which must attend any worship of Humanity. Not only are the highest forms of human virtue rare, but even when they do occur, they are very apt to be rejected and despised of men. Power and strength, however vicious in its exercise, almost always receives the homage of the world. The human idols, therefore, who would be chosen as symbols in the worship of humanity, would often be those who set the very worst examples to their kind. Perhaps no better illustration of this could be found than the history of Napoleon Buonaparte. I think it is impossible to follow that history, as it is now known, without coming to the conclusion that in every sense of the word he was a bad man—unscrupulous, false, and mean. But his intellect was powerful, whilst his force and energy of character were tremendous. These qualities alone, exhibited in almost unexampled military success, were sufficient to make him the idol of many minds. And as mere success secured for him this place, so nothing but failure deprived him of it. Not a few of the chosen heroes of Humanity have been chosen for reasons but little better. Comte himself, seeing this danger, and with an exalted estimate and ideal of the character of womanhood, had laid it down that it would be best to select some woman as the symbol, if not the object, of private adoration in the worship of Humanity. The French Revolutionists selected a woman, too, and we know the kind of woman that they chose. It may be wise, perhaps, to set aside this famous episode in a fit of national insanity as nothing more than a profane joke; but the developments of anthropomorphism in the mythology of the Pagan world are a sufficient indication of the kind of worship which the worship of Humanity would certainly tend to be.

The result, then, of this analysis of that in which all Religion essentially consists, and of the objects which it selects, or imagines, or creates for worship, is to show that in Religion, above all other things, the processes of evolution are especially liable to work in the direction of degradation. That analysis shows how it is that in the domain of religious conceptions, even more than in any domain of thought, the work of development must be rapid, because, in the absence of revelation or the teachings of Authority, fancy and imagination have no guide and are under no restraint.

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[3] Mill's "Comte and Positivism," p. 136.

When, now, we pass from the phenomena which Religion presents in the present day to what we know of its phenomena in the earliest historic times, the conclusions we have reached receive abundant confirmation. Of the Origin of Religion, indeed, as we have already seen, history can tell us nothing, because, unless the Mosaic narrative be accepted, there is no history of the origin of Man. But the origin of particular systems of Religion does come within the domain of history, and the testimony it affords is always to the same effect. In regard to them we have the most positive evidence that they have been uniformly subject to degradation. All the great religions of the world which can be traced to the teaching or influence of individual men have steadily declined from the teaching of their founders. In India it has been one great business of Christian missionaries and of Christian governors, in their endeavors to put an end to cruel and barbarous customs, to prove to the corrupt disciples of an ancient creed that its first prophets or teachers had never held the doctrines from which such customs arise, or that these customs are a gross misconception and abuse of the doctrine which had been really taught. Whether we study what is now held by the disciples of Buddha, of Confucius, or of Zoroaster, it is the same result. Wherever we can arrive at the original teaching of the known founders of religious systems, we find that teaching uniformly higher, more spiritual than the teaching now. The same law has effected Christianity, with this difference only, that alone of all the historical religions of the world it has hitherto shown an unmistakable power of perennial revival and reform. But we know that the processes of corruption had begun their work even in the lifetime of the Apostles; and every church in Christendom will equally admit the general fact, although each of them will give a different illustration of it. Mohammedanism, which is the last and latest of the great historical religions of the world, shows a still more remarkable phenomena. The corruption in this case began not only in the lifetime but in the life of the prophet and founder of that religion. Mahomet was himself his own most corrupt disciple. In the earliest days of his mission he was best as a man and greatest as a teacher. His life was purer and his doctrine more spiritual when his voice was a solitary voice crying in the wilderness, than when it was joined in chorus by the voice of many millions. In his case the progress of development in a wrong direction was singularly distinct snd very rapid. Nor is the cause obscure. The spirit of Mahomet may well have been in close communion with the Spirit of all truth, when, like St. Paul at Athens, his heart was stirred within him as he saw his Arabian countrymen wholly given to idolatry. Such deep impressions on some everlasting truth—such overpowering convictions—are in the nature of inspiration. The intimations it gives and the impulses it communicates are true in thought and righteous in motive, in exact proportion as the reflecting surfaces of the human mind are accurately set to the lights which stream from Nature. This is the adjustment which gives all their truthfulness to the intimations of the senses; which gives all its wisdom and foresight to the wonderful work of instinct; which gives all their validity to the processes of reason, which is the real source of all the achievements of genius; and which, on the highest level of all, has made some men the inspired mouthpiece of the oracles of God. But it is the tenderest of all adjustments—the most delicate, the most easily disturbed. When this adjustment is, as it were, mechanical, as it is in the lower animals, then we have the limited, but, within its own sphere, the perfect wisdom of the beasts. But when this adjustment is liable to distortion by the action of a Will which is to some extent self-determined and is also to a large extent degraded, then the real inspiration is not from without, but from within—then the reflecting surfaces of mind are so longer set true to the light of Nature; and then "if the light within us be darkness, how great is that darkness!" Hence it is that one single mistake or misconception as to the nature and work of inspiration is, and must be a mistake of tremendous consequence. And this was Mahomet's mistake. He thought that the source of his inspiration was direct, immediate, and personal. He thought that even the very words in which his own impulses were embodied were dictated by the Angel Gabriel. He thought that the Supreme Authority which spoke through him when he proclaimed that "the Lord God Almighty was one God, the Merciful, the Compassionate," was the same which also spoke to him when he proclaimed that it was lawful for him to take his neighbor's wife. From such an abounding well-spring of delusion the most bitter waters were sure to come. How different this idea of the methods in which the Divine Spirit operates upon the minds of men from the idea held on the same subject by that great Apostle of our Lord whose work it was to spread among the Gentile world those religious conceptions which had so long been the special heritage of one peculiar people! How cautious St. Paul is when expressing an opinion not directly sanctioned by an authority higher than his own! "I think also that I have the Spirit of God." The injunction, "Try the spirits whether they be of God," is one which never seems to have occurred to Mahomet. The consequences were what might have been expected. The utterances of his inspiration when he was hiding in the caves of Mecca were better, purer, higher than those which he continued to pour forth when, after his flight to Medina, he became a great conqueror and a great ruler. From the very first indeed he breathed the spirit of personal anger and malediction on all who disbelieved his message. This root of bitterness was present from the beginning. But its developments were indeed prodigious. It was the animating spirit of precepts without number which, in the minds and in the hands of his ruthless followers, have inflicted untold miseries for twelve hundred years on some of the fairest regions of the globe.

Passing now from the evidence of the law of corruption and decline which is afforded by this last and latest of the great historical religions of the world, we find the same evidence in those of a much older date. In the first place, all the founders of those religions were themselves nothing but reformers. In the second place the reforms they instituted have themselves all more or less again yielded to new developments of decay. The great prophets of the world have been men of inspiration or of genius who were revolted by the corruptions of some pre-existing system, and who desired to restore some older and purer faith. The form which their reformation took was generally determined, as all strong revolts are sure to be, by violent reaction against some prominent conception or some system of practice which seemed, as it were, an embodiment of its corruption. In this way only can we account for the peculiar direction taken by the teaching of that one great historical Religion which is said to have more disciples than any other in the world. Buddhism was in its origin a reform of Brahminism. In that system the beliefs of a much older and simpler age had become hid under the rubbish-heaps of a most corrupt development. Nowhere perhaps in the world had the work of evolution been richer in the growth of briers and thorns. It had forged the iron bonds of caste, one of the very worst inventions of an evil imagination; and it had degraded worship into a complicated system of sacrifice and of ceremonial observances. There seems to be no doubt that the teaching of the reformer Sakya Muni (Buddha) was a revolt and a reform. It was a reassertion of the paramount value of a life of righteousness. But the intellectual conceptions which are associated wiih this great ethical and spiritual reform had within themselves the germs of another cycle of decay.

(To be continued.)

THE COMET.

The comet is daily becoming a fainter object, and astronomers are now employed in making investigations based on their observations.

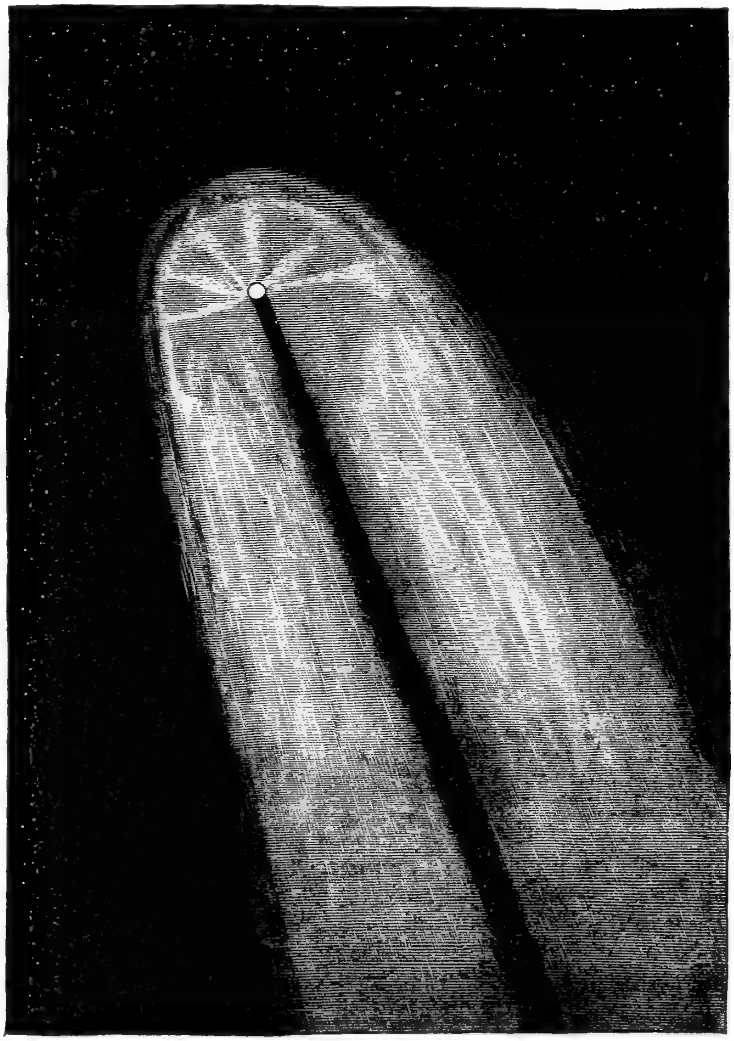
We understand that Professor O. Stone, of Cincinnati, has published a statement that he saw the nucleus of the comet divide into two parts. Professor Stone is not one likely to be mistaken in an observation of this nature, but we understand he has not been confirmed in this discovery, as observations since made with the large equatorial at Washington have failed to show any division. A disturbance, however, has been observed in the nucleus, which Professor Skinner considers might be mistaken for a division as described by Professor Stone.

On the 6th of July the comet was observed by Mr. Rock of the Naval Observatory, who thus describes what he saw:

"A bright tongue of light about one revolution long in direction of tail, with a slight node near end and curved."

In explanation of this Mr. Rock said: "I observed the comet at the time of its lower culmination about twenty minutes after midnight. The nucleus did not appear to be divided, but a bright band streamed out in the direction of the tail. This band was about fifteen seconds of the arc in length. Near the end of it was a bright spot, and that portion of the band extending beyond it was curved in the same general direction as the tail, but in a somewhat shorter arc. It is possible that the observer at Cincinnati was not able to distinguish the band of light which I saw uniting the nucleus and the node, and so concluded that he saw two nuclei. When I first observed the comet, on June 28, the coma was apparently homogeneous as it also was on July 2. On June 28, however, there were two spurs of light spreading away from the opposite sides of the head like angel's wings. On July 2, I did not observe these at all or they were very faint. On July 6 I observed the appearance that I have described. It may be that this was the same thing that I saw on June 28, observed from a different point of view. It is not improbable, however, that the nucleus has really divided. Comets appear to have a tendency to do that.

Head of Donati's Comet, After Bond.



Comet of 1881. After Prof. Henry Draper's Photograph.

To a correspondent of the N. Y. *Tribune*, Professor Harkness said:

"We think that the November meteors are the debris of a comet which first made its appearance about the year 900. This debris, to all appearance, continues to trail along the whole orbit of the comet of which it formed a part and which has disappeared. The August meteors are assigned a similar origin. Biela's comet reappeared once after its nucleus had separated into two parts; it has never been observed since. All comets appear to diminish in brightness, and it is probable that they become gradually disintegrated. I have undertaken spectroscopic investigation of this comet, sufficient to convince me that the spectrum is the same as that of all comets. I made observations on June 28 and July 1 and 2. On June 28, I found a bright continuous spectrum with three bands very hazy, the whole indistinct. Evidence of polarization was not trustworthy, and I concluded there was no polarization. On July 1 the spectrum of the nucleus was right, showing two bands; wave lengths approximately 550.29 and 611.5. On July 2, I found a bright, continuous spectrum extending from about wave length 577 to 428; the coma gave three bright bands; wave lengths

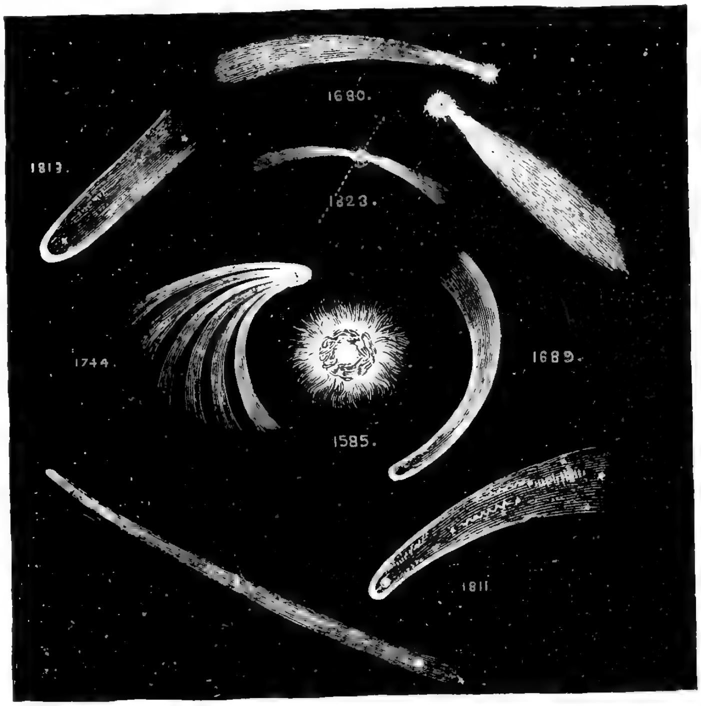
approximately 548.4, 513.3, 467.2. The tail gave no continuous spectrum. The mean of eighteen comets observed gives us wave lengths as follows: 556.4, 512.7 and 470.6; the mean of two nights' work on this comet gave me 549.3, 512.4 and 467.2. These two sets of figures agree as nearly as could be expected, considering that I used in my observations a single 60° prism, and there can be no doubt whatever that this is the usual comet spectrum."

Professor A. Hall also observed that he had received from Baron Struve, of the Imperial Observatory at Palkova, an ephemeris of Encke's comet extending from July 29 to November 14, and preparations are making at the Naval Observatory for careful observations of that body, which is considered of great scientific interest.

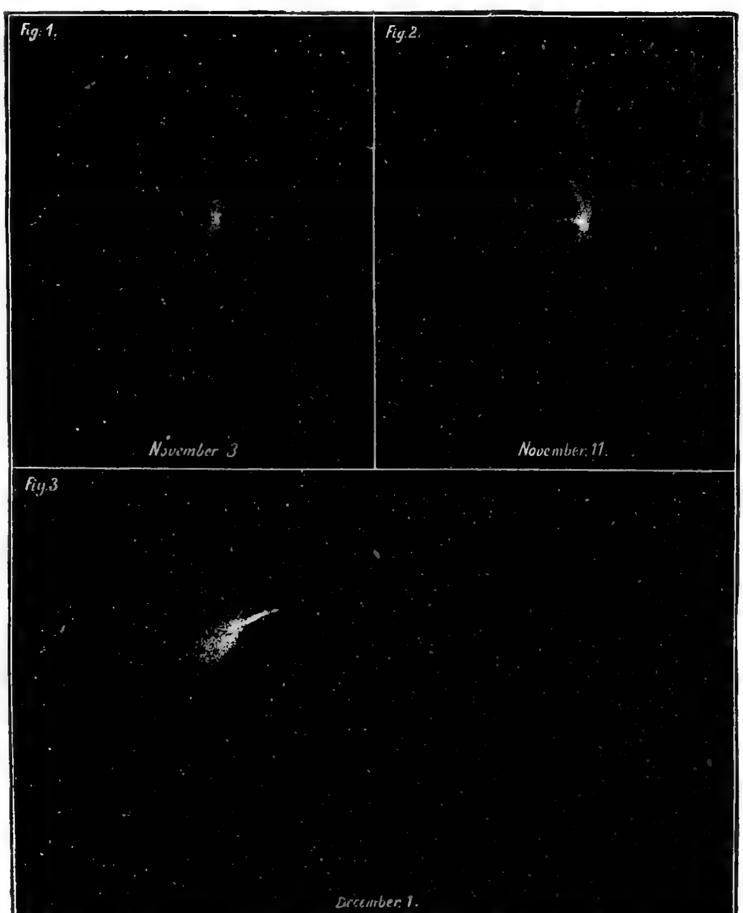
We present our readers with an illustration showing the appearance of the comet in one of Professor Henry Draper's photographs for which we are indebted to Messrs. Harper Brothers, and we hope to shortly publish Professor Draper's mature views based on his observations and photographs, both of the comet and its spectrum.

To a correspondent Prof. Draper gave the following particulars:

"In the spectrum of the comet there is one great band in the ultra-violet region beyond the line H. This morning I brought the spectroscope with me to the city, and have taken photographs of the spectrum of the electric arc with it. The electric arc



Group of Various Comets.



Views of Encke's Comet, 1871

contains carbon; not, in all probability, the pure element of carbon volatilized, but some compound of carbon—most likely a hydrocarbon. The spectrum of the electric or voltaic arc shows a strong band at the ultra-violet region, due to the presence of this carbon or carbon-compound. This spectrum I mean to compare with that of the comet, to see whether the bands in the ultra-violet region correspond in the two. If they do, the presence of some form of carbon in the comet will be demonstrated. My impression at present is that the ultra-violet spectrum of the comet does prove that it contains carbon, but I cannot speak with certainty until after I have made more careful measurements of the photographs. At all events, my experiments must settle the question.

We are indebted to Professor Pickering, of Harvard University, for some valuable observations which will be found in another column.

ON THE TAILS OF COMETS.

One of the most important articles in the June number of *Urania* is that by M. Th. Bredichin, on the Tails of Comets. After a series of investigations he arrives at the conclusion that the position, curvature and structure of a tail are explained by the repulsive force of the sun and by the effluvia of cometary matter from the nucleus towards the sun with a certain initial velocity or repulsion.

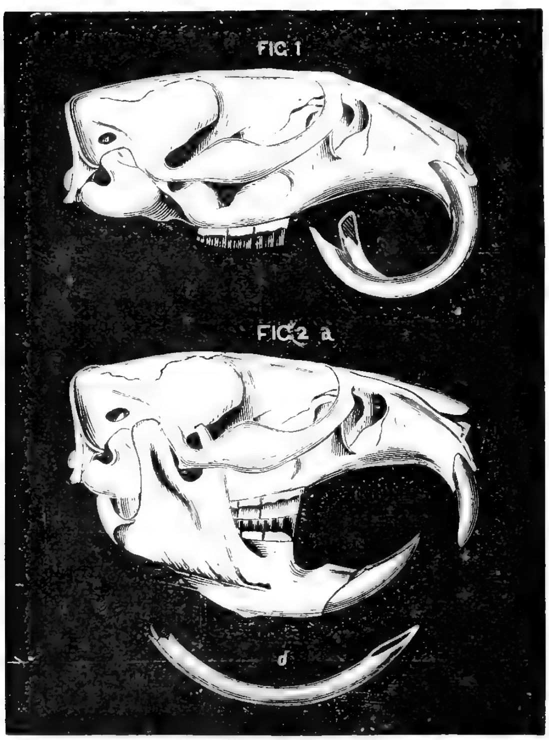
ON THE OVERGROWN TEETH OF FIBER WIBETHICUS.

By Herman L. Fairchild.

No group of animals is more clearly marked by a single feature than the *Rodentia* by their peculiar incisor teeth. Except in the Rabbits which have a supplementary pair in the upper jaw, the number is always four. The enamel is mostly, sometimes wholly, on the anterior surface; where also the dentine is harder. The constant abrasion consequently preserves a keen chisel edge which admirably adapts them for gnawing. This purpose requires them to be of a certain length. To keep that length, the loss from wear is compensated by continual outward growth from the base, the growth being supplied from permanent pulp. The outward growth and the terminal wear are nicely balanced.

It is evident that a loss of one incisor prevents abrasion of the opposing tooth, which, continuing to push outward, may become so long as to interfere with the proper use of the jaws and the remaining teeth. Such cases, while not unknown, are sufficiently rare to be of great interest to the naturalist, and of wonderment to the unscientific.

* Fig. 1, represents a striking example of such malformation in the case of a muskrat, *Fiber Wibethicus.* The figure is three-fourths the size of the specimen. This skull was found on the bank of Sacandaga River, town of Edinburgh, Saratoga Co., N. Y. Unfortunately no other portion of the skeleton was collected; but the most unobserving could not fail to notice such remarkable teeth. It was naturally supposed that some strange creature had been discovered. Falling into the hands of the writer its character was discovered and a normal specimen was procured for accurate comparison. The latter is shown in figure 2.



When removed from their sockets the overgrown incisors show a growth nearly to a complet circle, although the curvature is somewhat spiral. Their terminations exhibit the normal form produced by abrasion at a time when the teeth were of the proper length, and are naturally discolored with foreign matter on account of long disuse. The yellow color of the front surface of these incisors is fainter towards the ends, but still marked.

As the mandible is missing it is impossible to know what was the difficulty with the lower incisors. They might have been broken by severe usage or carried away by a gun-shot. But that the animal once possessed them is shown by the naturally abraded ends of the remaining ones. The trouble seems to have been of a character which prevented the after growth of the lower incisors. For if they had grown out again after an injury, they would have been forced to take a position in front of the lengthened upper incisors. This would have prevented the forward and backward motion necessary for mastication, and so prominent in Rodents, and moreover would undoubtedly have worn the anterior surfaces of the overgrown upper teeth. But the latter do not show the least unnatural abrasion, while the molars do show that they were used. Probably the breaking of the teeth near the bone would have so exposed the pulp as to destroy it and the implanted part of the injured teeth.

The fairly clean fresh surfaces of all the upper molars would indicate that the lower molars were quite intact and that the greatly lengthened teeth did not interfere with mastication, however much they interfered with prehension. The accumulation of foreign matter upon the sides of the molars is greater than on those of the normal skull. Perhaps this is due to less discrimination in choice of food, and possibly to somewhat greater age.

That the animal lived some considerable time after its misfortune, is proven by the great length of the teeth. The time required for this growth is unknown. It is, however, a very interesting point and should be determined. The rate of growth of the incisors may vary, possibly in the same individual, according to the kind of food and consequent wear; at least it would not be right to assume that the rate of growth is always the same. Observation upon a Rabbit or other rodent would be valuable but not conclusive, as the rabbit is entirely vegetarian in diet while the Muskrat is quite omnivorous. To answer the question before us, the observation should be made upon a Muskrat having the lower incisors removed or rendered useless, in order to repeat as nearly as possible the conditions under which we imagine our specimen existed.

As the ends of the overgrown incisors had long passed the point of greatest interference they did not prevent the taking of food with the mouth; and the creature probably did not die from starvation.

If the readers of "SCIENCE" can give any facts bearing on this matter from their personal knowledge and observation they will confer a favor by sending them to these columns.

In *Forest and Stream* of April 4, 1878, there is a sketch of a Woodchuck's skull showing an abnormal lengthening of both pairs of incisors, which, according to the description, did not prevent the animal from procuring sufficient food to keep it in good condition. And Owen's "Odontography" briefly describes (page 411, old ed.) the abnormal elongation of the incisors of rodents; and notices the skull of a beaver, of which a lower incisor formed a complete circle. Plate 104, Fig. 7, of the above work, also shows the abnormal upper incisors of a rabbit.

* This illustration was used, without comment, in an article by the writer in the *Popular Science Monthly* for June, 1880.

Absorbing Power of the Atmosphere.—M. Laight had long ago shown that the radiating light of the sun is largely absorbed by the layer of atmosphere. But penetrating more deeply into the question, he has successively and separately studied the absorption undergone by each ray of the spectrum. He concludes that these diverse rays are from being equally absorbed, and that the radiation is modified according to the degree of absorption. One of the results of this interesting fact is that the color of the sun is different from that which we attribute to him.

BOOKS RECEIVED.

ANTHROPOLOGY: An Introduction to the Study of Man and Civilization, by EDWARD B. TYLOR, D. C. L., F. R. S. With Illustrations. D. Appleton & Co., New York, 1881.

The present volume is one which will be very acceptable to a large class of scientific readers, for it places before them within the compass of a book of three hundred pages, the principles on which the science of Anthropology is based, and a synopsis of the mass of facts collected and arranged by Anthropologists, which are scattered in some fifty standard works and hundreds of independent papers on the subject.

As an introduction to the science of Anthropology, Dr. Tylor's work is a great success, and if carefully studied will save a vast amount of desultory reading on the part of the student, and as strictly technical details are carefully avoided, the author has succeeded in bringing the subject within reach of readers who have received or are receiving the ordinary higher English education.

The work opens with a brief but sufficiently comprehensive survey of the varieties of men, their language, their civilization and their ancient relics, thus showing by vestiges of man's early existence, what proofs we have of his first appearance and ultimate development.

The most common observer cannot fail to notice the broad distinction among races of men, but it is only within modern times that these distinctions have been worked out by scientific methods. One of the first questions which arise in tracing the history of mankind, is, did man originate from one stock in some primitive centre, and afterwards spread far and wide, or are the Negroes, Mongolians, Whites and other races distinct species, each sprung from a separate origin.

Dr. Tylor favors the views propounded by modern zoologists, which is against the several origins of mankind, for two principal reasons. First, that all tribes of men, from the blackest to the whitest, the most savage to the most cultured, have such general likeness in the structure of their bodies and the working of their minds,—as is easiest and best accounted for by their being descended from a common ancestry, however distant,—and secondly, that all the human races, notwithstanding their form and color, appear capable of freely intermarrying and forming cross races of every combination, which appears to point to a common ancestry. The author therefore advises the acceptance of this theory of the unity of mankind as best agreeing with ordinary experience and scientific research.

Any decision on this subject, however, must be considered provisional only, as our means of judging what man's progenitors were like, both in mind and body, before the forefathers of the present negroes and Tartars and Australians were separated into distinct stocks, is at the best most imperfect. Nor is it yet clear by what causes these stocks or races passed into their different types of skull and limbs, of complexion and hair.

We find no aid from the study of ancient inscriptions and figures, as to the condition of races at the beginning of historic times.

Figures of Egyptians drawn more than 4000 years ago, describe features very similar to those found in Egypt at the present day. The celebrated inscription of Prince Una, dating back 2000 years B. C., makes mention of the *Nahsi* or Negroes who were levied and drilled by ten thousands for the Egyptian army; and on the tomb of Knumkept of the 12th dynasty there is represented a procession of *Amu*, who are seen by their features to be of the race to which Syrians and Hebrews belonged. In fact all the evidences derived from ancient monuments, geography and history, prove that the great race-divisions of mankind are of no recent growth, but were already settled before the beginning of the historical period. We must then look to the prehistoric period as the time when the chief work was done of forming and spreading over the world the races of mankind.

We might expect that "language" would tell of man's age on the earth, but the reader of this work will find that although there is evidence that all recent language was derived from one primitive language, the most patient research shows that all trace of that primitive language is lost.

The first chapter of Dr. Tylor's work includes a history of the civilization of man and his gradual development in the appreciation of Art. The first traces of man in the stone age is described, dating back from twenty to a hundred thousand years, presenting evidence that, even at that remote period, man possessed all the attributes of humanity in a savage and rude condition.

In the second chapter man is compared with the brute creation. To show how man may have advanced from savagery to civilization is a reasonable task and is worked out to some extent by the author. But the evidence is wanting for crossing that mental gulf that divides the lowest savage from the highest ape.

The general conclusion advanced by the author in this branch of the subject is expressed by Dr. Tylor as follows: "On the whole the safest conclusion warranted by facts is that the mental machinery of the lower animals is roughly similar to our own, up to a limit. Beyond this limit the human mind opens out into a wide range of thought and feeling which the beast mind shows no sign of approaching. If we consider man's course of life from birth to death, we see that it is, so to speak, founded on functions which he has in common with lower beings. Man, endowed with instinct and capable of learning by experience, drawn by pleasure and driven by pain, must like the beast, maintain his life by food and sleep, must save himself by flight, or fight it out with his foes, must propagate his species and care for the next generation. Upon this lower framework of animal life is raised the wondrous edifice of human language, science, art and order."

To the many who have yet to master the principles of this, the latest of sciences, "*Anthropology*," we commend this book as one which will be read with much satisfaction and profit, for the study of man and civilization is not a matter of scientific interest only, but at once passes into the practical business of life. We have in it the means of understanding our own lives and our place in the world, vaguely and imperfectly, it is true, but at any rate more clearly than any former generation.

The knowledge of man's course of life from the remotest past to the present, will not only help us to forecast the future, but, says the author, guide us in our duty of leaving the world better than we found it.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

FIRE BALLS.

To the Editor of "SCIENCE":—

The interesting instance, narrated in a recent number of "SCIENCE," of the descent of fire balls as observed by Henry O. Forbes, calls to mind two occurrences which I have witnessed under circumstances favorable for accurate observation.

One sultry summer day, at sea, I was lying on the deck of a small schooner, watching in the sky the gathering clouds of a sudden and violent thunder shower. I was looking over the main mast, whose top was in the centre of my field of view. As the first scattered drops of rain began to fall, and in advance of any lightning or thunder, there appeared upon the top of the mast a brush of fire

remarkably like that which is often produced on a small scale in electrical experiments. This brush shone very distinctly against the heavily overcast and darkened sky; and it looked about as large as the hand of a half-grown child, with the fingers spread moderately apart. After one or two seconds it seemed to change into a ball of fire, of smaller size but greater intensity, and distinctly round in outline, which glided smoothly down the surface of the mast and across the wooden deck, until it passed over the stern of the boat and entered the water with an explosion not unlike the report of a large pistol. There was no lightning-rod upon the vessel, but the wood of the mast and of the deck was quite wet by the time the ball passed over it. The electrical disturbance did not approach the mast with a visible flash, and the sound of the explosion, the only sound noticed, was decidedly from the direction where the ball entered the water. The ball left the mast in a line at right angles to a tangent at the point of departure, while the nearest course to the sea would have been in the direction of the tangent; but having once commenced to cross the deck it took a perfectly straight course. The wood over which it passed was slightly discolored in several places, but not at all charred.

On another occasion I was standing, with several companions, in a carriage-house, in the country, having taken refuge there from a sudden shower. Through an open door we were gazing intently upon a large barn near by, discussing the safety of occupying that more commodious retreat, when a flash of lightning, in the usual zigzag form, passed obliquely from the clouds to the barn, striking the ridge at the very summit of the roof. Thence it passed, as a distinct ball of fire, over the wet shingles down the surface of the roof to the eaves and there entered the barn. We thought there was a report as the ball entered the barn, which had been recently filled with freshly gathered hay, but were not certain, owing to the nearly, if not practically, simultaneous arrival of the thunder sound. The nearest door of the barn was opened within a very few seconds, and the interior was found filled with fire and smoke; although the roof over which the ball had passed remained unaffected until destroyed by fire breaking out from within the building. Although I had been the only one in the party to insist in taking refuge in the carriage-house instead of the barn, there seemed to arise on the part of the majority a considerable unwillingness to further dwell upon the reasons for preferring the latter place of safety.

R. H. WARD, M. D.

To the Editor of "SCIENCE."

Allow me in reply to R. C. S. again to tell him, most emphatically, that I have never entertained, for a moment, the idea of "reviving" or advocating the theory that motor cells can be distinguished from sensory ones by their size. In order to "revive" a theory it must be restated in some form. In the "transactions" of the American Neurological Association, published in the *Journal for Nervous and Mental Disease*, July, 1880, p. 476, I am correctly reported as stating that "so far as sensation went, it had nothing to do with the subject of the paper." My theory relates exclusively to the nuclie in so-called motor cells. They are called motor not by any means on account of their size, but from their evident connection with motor filaments. In spite of my denial, R. C. S. still asserts the wrong thing, and shows none of the customary regret at having possibly misunderstood me.

Prof. Stieda is referred to by me not to "polemize" against him, but to show that, while he had measured their cells and their nuclie in the spinal cord of turtles, he had not anticipated me in attributing difference in size to difference in energy. Stieda's expression is; Physiologische Dignität, which I translate physiological importance. As neither sensation nor sensory cells are here mentioned by him, it seemed plain that he, like myself, referred solely to cells of the spinal cord which, by their close relation to motor filaments, are supposed to have a motor function.

The careful reader for whom R. C. S. so dogmatically responds, is respectfully requested to bear in mind that the three brief articles which I have published, relate throughout to Reptiles and Batrachians, and not to mammals. With this reminder he will have, I think, no difficulty in reading, in some places, between the lines.

As to the auditory nerve centres, it remains for me to state that the paragraph which R. C. S. quotes was offered as a mere suggestion to one who seemed also to think that the large cells in the vicinity of the roots of the auditory nerve, in the iguana, bore some relation to my theory. As his communication was stated to be preliminary in character, and had nothing to do with my subject, I decided to make no personal reference, suggesting that these cells (as claimed fourteen years ago by Deiters) were of doubtful function, and that the cells related to vision and olfaction were (in reptiles, etc.,) all very small. This, I believe, is true, but it revives no theory.

I leave my unknown critic to the contemplation of this clause which appears in his last publication: "Notwithstanding the construction which Dr. J. J. Mason now desires to see placed upon his words," doing him the justice to suppose that he knows what he insinuates, and that being mortal, he will hasten to admit that he may have misunderstood me.

JOHN J. MASON, M. D.

NEWPORT, R. I., July 2, 1881.

DECOMPOSITION OF WATER.—In decomposing water by discharging Leyden jars through platinum electrodes, Dr. Streintz finds that, with very small electrodes giving passage to a series of discharge currents in one direction, and then left to themselves, a remarkable reversal of E.M.F. occurs, but only when the discharges do not exceed a certain number. Dr. Streintz made use of a quadrant-electrometer in his experiments.

SIMPLE METHOD OF DETERMINING THE TEMPORARY HARDNESS OF WATER.—In order to ascertain the alkalinity of springs on the spot, with samples not exceeding 10 c.c., and with a single reagent, the author makes use of a tube of 30 to 40 c.m. long, closed at the bottom, and with a mark showing the capacity of 10 c.c. From this mark upwards the tube is graduated into 0.1 c.c. To determine the temporary hardness the tube is filled to the lowest mark with the water in question, and a little piece of filter-paper, which has been previously steeped in extract of logwood and dried, is thrown in, thus giving the water a violet color. Centinormal hydrochloric acid is then added from a dropping bottle, till the color of the liquid inclines to an orange. The tube is then closed with the thumb and well shaken. The greater part of the carbonic acid escapes, and the liquid becomes red again. Acid is again added, and the shaking repeated until the next drop of the acid turns the liquid to a pure lemon-yellow, a point which a little practice is easily reached. The amount of acid used is read off on the tube itself. The author proposes to express the alkalinity of a water by the number of c.c. of centinormal acid needed to neutralise 10 c.c. He thinks that this method will be found useful both for sanitary and geological purposes.—V. WARTHA.

CHEMISTRY OF THE PLATINUM METALS.—Contrary to the prevalent view, all the platinum metals, if precipitated by zinc in a state of very fine division, are soluble to a considerable extent in nitric acid, whether weak or strong, so that palladium cannot be separated from such a mixture by means of nitric acid. The solubility appears to depend on the relative proportion of one or other of the metals in the

mixture (mass action.) Pure palladium, even in thin leaves, is not easily soluble in nitric acid, whilst all the other platinum metals are perfectly insoluble if in a moderately compact condition. Palladium cannot be isolated by agitation with mercury from a solution which, along with the platinum metals contain base metals, such as copper, lead, &c., since the mercury precipitates, not merely the palladium, but all the other platinum metals, forming probably amalgams. From the platinum metals thus precipitated by mercury, metal free from mercury cannot be obtained by distillation and subsequent ignition, since a part of the mercury forms a stable compound with the platinoids. —THEODOR WILM.

GLYCERIN.—Notwithstanding the low price which prevails for almost every description of raw produce and manufactured goods, there are a few articles which form notable exceptions. Perhaps one of the most remarkable of these is refined glycerin, which, within the last two years, has advanced from about £30 to £130 per ton avoirdupois for 30° B. This enormous advance is due partly to increased consumption, diminished production and the influence of speculation working on a market devoid of stocks. In view of the present position of the article and the prospect of a continuance of high prices for a considerable time to come, the attention of soapmakers is now being turned to the utilization of their waste "leys," and various new processes for recovering the glycerin contained in these liquors have lately been tried with more or less successful results. Apart from minor impurities, waste soap "leys" are generally found to contain glycerin, carbonate of soda or caustic soda, chloride of sodium, gelatin and albumen. One of the processes for recovering the glycerin which promises to be the most economical and the most successful begins with concentrating the liquor until the salts contained therein begin to crystallize. The liquid is then cooled and filtered to rid it of gelatin and albumen. It is afterwards made to absorb carbonic acid, which precipitates bi-carbonate of soda, and which is separated from the liquor in the usual way. After undergoing this process the liquor is then made to absorb gaseous hydrochloric acid until what remains of carbonate of soda has been converted into chloride, and further, until all, or almost all, the chloride of sodium has been precipitated and separated from the liquor in the usual manner. Arrived at this stage, the liquor contains water, glycerin and hydrochloric acid. The acid is then evaporated entirely and absorbed in water for using afresh. The dilute glycerin remaining can be purified by filtering it through animal charcoal or by concentrating and distilling it in the usual way.

AN INDUSTRIAL AND TECHNOLOGICAL MUSEUM.—An Industrial and Technological Museum of a very comprehensive character is in course of organization at Sydney. It is to include animal, vegetable and mineral produce in the crude and in the manufactured states: waste products, of whatsoever origin, foods with their constituents, and that necessary shadow side of the picture, their adulterations; educational appliances; sanitary apparatus and systems, models, plans, machinery, etc., for mining; agricultural machinery and manures; models, drawings, and descriptions of patents; a department of economic entomology; ethnological specimens, etc. One remark in the prospectus may call up a smile. The museum is intended to occupy a similar position to the South Kensington Museum. This might be construed to mean that it is to occupy a site as far out of the way of merchants, manufacturers, patentees, etc., as possible. We need scarcely say that the project has our best wishes.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING JULY 9, 1881.

Latitude 40° 45′ 58″; Longitude 73° 57′ 58″; height from ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

JULY.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 3	29.974	30.100	0 a. m.	29.898	12 p. m.	77.3	67.3	87	5 p. m.	71	6 p. m.	65	5 a. m.	60	5 a. m.	139.
Monday, 4	29.861	29.898	0 a. m.	29.800	5 p. m.	71.6	67.6	85	1 p. m.	72	1 p. m.	70	5 a. m.	66	5 a. m.	136.
Tuesday, 5	29.850	29.906	9 a. m.	29.790	7 p. m.	77.3	70.7	85	5 p. m.	75	5 p. m.	68	3 a. m.	66	3 a. m.	138.
Wednesday, 6	29.828	29.902	12 p. m.	29.750	4 a. m.	82.7	75.0	88	4 p. m.	79	7 p. m.	74	5 a. m.	70	5 a. m.	147.
Thursday, 7	29.983	30.002	9 a. m.	29.892	12 p. m.	76.0	69.3	83	3 p. m.	71	3 p. m.	70	5 a. m.	68	5 a. m.	143.
Friday, 8	29.927	29.998	12 p. m.	29.836	6 a. m.	67.0	65.0	71	7 a. m.	69	7 a. m.	64	2 p. m.	63	2 p. m.	85.
Saturday, 9	30.059	30.090	12 p. m.	29.998	0 a. m.	70.3	66.6	80	4 p. m.	72	4 p. m.	63	6 a. m.	62	6 a. m.	140.

Mean for the week.. 29.926 inches.
Maximum for the week at 0 am., July 3rd.................. 30.100 "
Minimum " at 4 " 6th.................. 29.750 "
Range.. .350 "

	Dry.	Wet.
Mean for the week	74.6 degrees	68.8 degrees.
Maximum for the week,	at 4 pm. 6th 88. "	at 7 pm 6th, 79. "
Minimum " "	6 am. 9th 63. "	at 5 am 3rd, 60. "
Range " "	25. "	19. "

JULY.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in Lbs. per Sqr. Feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of Rain and Snow in Inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 3	w. s. w.	w. n. w.	w. s. w.	180	2½	1.20 pm	.416	.558	.585	69	49	55	2 cir.	2 cir. cu.	2 cu. s.	-----	-----	-----	--	4
Monday, 4	n. n. w.	e. n. e.	w.	104	4	6.30 pm	.581	.644	.654	72	85	85	3 cir. s.	9 cir. cu.	9 cu.	1.30 pm	7.15 pm	5.45	.80	0
Tuesday, 5	n. e.	s. s. e.	s. w.	94	2½	8.30 pm	.622	.650	.717	85	59	70	9 cu.	3 cir. cu.	5 cir. cu.	-----	-----	-----	--	2
Wednesday, 6	w.	n. n. e.	n. n. w.	141	2	2.40 pm	.690	.836	.773	70	65	71	0	4 cir. cu.	2 cir. cu.	5.00 pm	5.15 pm	0.15	.04	1
Thursday, 7	n. e.	s. e.	s. e.	180	3	2.00 pm	.641	.610	.631	76	56	80	2 cir.	3 cir. cu.	10	-----	-----	-----	--	0
Friday, 8	s. e.	e. n. e.	n. n. e.	202	6¾	11.40 am	.682	.562	.536	90	94	84	10	10	10	9.00 am	2.00 pm	5.00	.06	4
Saturday, 9	e. n. e.	s. s. e.	s. s. e.	102	1	4.00 pm	.529	.648	.622	89	73	85	9 cu.	7 cir. cu.	10	-----	-----	-----	--	0

Distance traveled during the weeek........................ 1,003 miles.
Maximum force.. 6¾ lbs.

Total amount of water for the week.............................90 inch.
Duration of rain...11 hours.

DANIEL DRAPER, PH. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

WAR DEPARTMENT REPORTS.

METEORS OBSRRVED DURING MAY, 1881.

Almota, Wash. Ty., 18th. Boise City, 3d. Ft. Stevenson, 2d, very brilliant, observed about 8 p. m. in the southern heavens; first appeared about 5° above the horizon and inclined south and downward; its flight lasted about 5 seconds, leaving a faint train; in size, it was apparently nearly equal to that of the full moon. Fredericksburg, Tex., 22d, 10.10 p. m., altitude 45°, direction from northwest to southeast; as it passed along it gave a light much brighter than the stars, leaving a trail of dark blue color and exploded apparently into three balls of fire. Davenport, 22d. Washington, D. C., 9th, 9.45 p. m., appeared 45° above south-eastern horizon, pursued a northwesterly course through zenith; extent of its visible path was about 20°. Wood's Holl, 3d, 2.10 a.m., from east to west. Woodstock, Md., 17th, 9 p. m.; 23d, 9 p. m., from southeast to northwest; 24th, 8 35 p. m., from west to east. Williamstown and Fall River, Mass., 13th. Thornville, Mich., 27th. Fayette, Miss., 1st. Atco, N. J., 22d, 24th, North Volney, N. Y., 23d. Waterburg, N. Y., 2d. Little Mountain, Ohio, 7th. Mission House, Wis., 2d.

Table of Maximum and Minimum Temperatures for May, 1881.

STATE OR TERRITORY.	SIGNAL SERVICE.			U. S. ARMY POST SURGEONS OR VOLUNTARY OBSERVERS.		
	Station.	Max.	Min.	Station.	Max.	Min.
Alabama	Montgomery	90°	58°	Auburn and		
"	---	---	---	Green Springs	---	56°
Arizona	Tucson	106°	---	Texas Hill	108°	---
"	Yuma	---	31°	---	---	---
Arkansas	Little Rock	87°	61°	Mt. Ida	88°	---
"	---	---	---	Fayetteville	---	48°
California	Visalia	95°	---	Turlock	109°	---
"	Campo	---	31°	Summit	---	30°
Colorado	Denver	84°	---	Hermosa	92°	---
"	Pike's Peak	---	5°	---	---	---
Connecticut	New London	89°	---	---	---	---
"	New Haven	---	36°	---	---	---
Dakota	Ft. Bennett	92°	28°	Ft. Buford	---	22°
"	Bismarck	---	28°	---	---	---
Delaware	Breakwater	82°	45°	Dover	90°	---
Dist. Columbia	Washington	95°	44°	---	---	---
Florida	Jacksonville	96°	63°	Ft. Barrancas	---	---
"	---	---	---	St. Augustine		
"	---	---	---	and Houston	---	60°
Georgia	Augusta	98°	---	Forsyth	99°	---
"	Atlanta	---	52°	McPherson Bks	---	50°
Iowa	Dubuque	90°	---	Clinton	95°	35°
"	Davenport	---	38°	---	---	---
Idaho	Ft. Lapwai	88°	27°	---	---	---
"	Lewiston and					
"	Boise City	88°	---	---	---	---
Illinois	Springfield	88°	37°	Pecria and		
"	Chicago	88°	---	Elmira	93°	---
Indiana	Indianapolis	89°	44°	Logansport	99°	---
"	---	---	---	Spiceland	---	41°
Indian Territory	Fort Gibson	92°	---	---	---	---
" "	Fort Sill	---	56°	---	---	---
Kansas	Leavenworth	90°	43°	Independence	93°	---
"	---	---	---	Clay Centre	93°	40°
Kentucky	Louisville	93°	51°	---	---	---
Louisiana	Shreveport	92°	---	Pt. Pleasant	93°	---
"	New Orleans	---	60°	---	---	---
Maine	Portland	86°	---	Gardiner	---	27°
"	Eastport	---	32°	---	---	---
Maryland	Baltimore	95°	46°	Woodstock	---	38°
Massachusetts	Boston	91°	36°	Williamstown	---	25°
Michigan	Marquette	88°	25°	Hudson and		
"	Port Huron	88°	---	Litchfield	94°	---
"	---	---	---	Ft. Brady	---	24°

STATE OR TERRITORY.	SIGNAL SERVICE.			U. S. ARMY POST SURGEON OR VOLUNTARY OBSERVERS.		
	Station.	Max.	Min.	Station.	Max.	Min.
Minnesota	Moorhead	88°	---	New Ulm	89°	---
"	St. Vincent	---	28°	---	---	---
Mississippi	Vicksburg	94°	62°	---	---	---
Missouri	St. Louis	91°	43°	---	---	---
Montana	Fort Keogh	98°	---	---	---	---
"	Ft. Assinniboine					
"	& Rock Creek	---	20°	---	---	---
Nebraska	North Platte	88°	37°	Ft. Niobrara	96°	---
Nevada	Winnemucca	86°	28°	Carson City	94°	---
"	---	---	---	Palisade	---	25°
N. Hampshire	Mt. Washington	61°	8°	Contoocookville	88°	---
New Jersey	Sandy Hook	91°	---	South Amboy	97°	---
"	Barnegat	---	37°	Atco	---	32°
New Mexico	La Mesilla	101°	---	Ft. Union	---	28°
"	Sante Fe	---	33°	---	---	---
New York	New York City	93°	---	West Point	98°	---
"	Buffalo	---	32°	Madison B'ks	---	21°
North Carolina	Charlotte	94°	---	Weldon	96°	---
" "	Fort Macon	---	50°	Murphy	---	43°
Ohio	Columbus	92°	---	Cincinnati	---	---
"	Cleveland	---	36°	Jacksonburg and		
"	---	---	---	Ruggles	96°	---
Oregon	Roseburg	85°	36°	Ft. Klamath	---	17°
Pennsylvania	Pittsburg	95°	38°	Milton	100°	---
"	---	---	---	Dyberry	---	21°
Rhode Island	Newport	85°	39°	Ft. Adams	---	35°
South Carolina	Charleston	91°	56°	Aiken	97°	53°
Tennessee	Knoxville	93°	49°	---	---	---
Texas	Rio Grande	102°	---	Ft. Ringgold	106°	---
"	Fort Davis	---	41°	Ft. Brown	---	36°
Utah	Salt Lake City	86°	40°	Promontory	91°	---
"	---	---	---	Kelton	---	30°
Vermont	Burlington	85°	30°	Charlotte	90°	---
"	---	---	---	Wookstock	---	23°
Virginia	Lynchburg	96°	---	Accotink	97°	---
"	Fort Myer	---	43°	Ft. Monroe	---	42°
Washington Ty	Almota	87°	---	---	---	---
"	Colfax	---	25°	---	---	---
West Virginia	Morgantown	85°	39°	Flemington	90°	---
Wisconsin	Madison	88°	35°	Beloit	91°	31°
"	Milwaukee	88°	---	---	---	---
Wyoming	Cheyenne	79°	32°	Ft. Fetterman	85°	16°
"	---	---	---	Ft. Bridger	---	---

SUN SPOTS.

The following record of observations made by Mr. D. P. Todd, Assistant, has been forwarded by Prof. S. Newcomb, U. S. Navy, Superintendent Nautical Almanac Office, Washington, D. C., to W. B. HAZEN, Brig. and Bv't Maj. Gen., Chief Signal Officer, U.S.A.

DATE, MAY, 1881.	No. of New		Disappeared by Solar Rotation.		Reappeared by Solar Rotation.		Total Number Visible.		Remarks.
	Groups.	Spots.	Groups.	Spots.	Groups.	Spots.	Groups.	Spots.	
1, 9 a. m.	0	0	1	1	0	2	1	6	
5, 7 a. m.	4	9	0	0	-----	-----	5	15	
7, 9 a. m.	0	0	1	2	0	0	4	13	
8, 9 a. m.	0	5	0	0	0	0	4	18	
5 p. m.	0	7	0	0	0	0	4	25	Many of the spots small.
9, 8 a. m.	0	0	0	0	0	0	4	18	
10, 9 a. m.	1	3	0	3	1	3	5	18	
11, 9 a. m.	0	7	1	4	0	7	3	17	
12, 9 a. m.	1	10	1	2	1	10	2	20†	
13, 8 a. m.	1	2	0	0	1	2	3	22†	
14, 8 a. m.	0	0	0	0	0	0	3	13†	
15, 9 a. m.	0	0	0	0	0	0	2	8	
16, 12 m.	2	5	0	0	1	2	4	8	
2[illegible], 10 a. m.	0	0	0	0	0	0	3	9	
23, 8 a. m.	1	5	0	0	0	0	4	16	
24, 6 a. m.	0	10	0	0	0	0	4	2[illegible]†	
25, 8 a. m.	0	10	2	6	0	0	2	30†	
26, 6 a. m.	1	7	0	0	1	7	3	37†	
27, 8 a. m.	1	7	0	0	0	6	4	44†	
28, 8 a. m.	0	12	0	0	0	12	4	56†	Many of the spots small.
29, 9 a. m.	0	0	0	8	0	0	4	48†	
30, 8 a. m.	2	15†	0	7	1	1	6	60†	
31, 7 a. m.	0	10	0	5	0	4	5	65†	Faculæ were seen at the time of every observation.

† Approximated.

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JOHN MICHELS, Editor.

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SATURDAY, JULY 23, 1881.

Professor Lewis Swift informs us that he has been receiving letters claiming the Warner Prize, at the rate of seventy per day for some time past; it may be convenient, therefore, if we state the conditions on which Mr. Warner offers the reward for the discovery of comets during the year 1881.

In the first place the comet must be telescopic, which is a bar to all naked eye observers, and the comet must be unexpected. An exception is made to this condition in favor of the comet of 1812, the reappearance of which is expected.

The first discovery of the comet must be made in the United States or Canada. To secure the prize immediate notification must be made by telegraph to Professor Lewis Swift, of Rochester, Director of the Warner Observatory. This telegram must give the time of the discovery, the position, direction and daily rate of motion with sufficient exactness to enable at least one astronomer to find it.

A study of these conditions will prevent useless applications and many disappointments. The first condition, however, which appears to limit claimants to the class who possess telescopes, should, in our opinion, be construed to object to naked eye observations only. A good opera or field binocular glass could be used with good effect in a search for comets. Caroline Herschell used a very simple instrument, and, in the course of her life, discovered no less than eight comets. With a tube with two glasses, such as was commonly used "as a finder," she used to "sweep" for comets, writing down and describing all remarkable appearances.

We direct attention to a series of interesting drawings of comet B, 1881, made by Professor Edward S. Holden at the Washburn Observatory, with the 15-inch telescope, constructed for the late Professor Watson, which will be found on pages 346 and 347 of this issue.

Professor Holden has attempted to delineate the appearance of the comet on six consecutive nights, commencing on the 24th of June, and also on the 8th and 11th of July.

Messrs. S. E. Cassino & Co., of 299 Washington street, Boston, are about to publish an international directory of the names and addresses of all those who are engaged in any of the departments of Science. Such a work can only be arranged in a satisfactory manner with the co-operation of scientific men. We therefore cordially respond to a request from Messrs. Cassino to make known their intentions in this direction, and we call upon all scientists at once to forward their names and addresses to the publisher.

This notice is not only intended for professional scientists, but for the large class of amateurs, who may be collecting, or giving their attention to any scientific specialty.

As the directory is partly prepared, prompt attention is essential to those who would have their names included.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

We remind our readers that the annual meeting of the American Association for the advancement of Science will be held this year at Cincinnati, commencing on the 17th of August next. The executive committee announce that the sessions of the Association will be held in the Music Hall and Exposition Buildings, on Elm street. All the meetings, general and sectional, will be under one roof. Each section will have a room regularly assigned to it, and every necessary facility in the way of tables, blackboards, etc,, will be provided. The offices of the Permanent and Local Secretaries, Reporters' Room, Post Office and Reception Rooms will all be on the first floor. Between the morning and afternoon sessions a daily lunch will be served in the wing of the Exposition Buildings known as Horticultural Hall.

On the first day of the meeting, besides the general session for organization, some of the official addresses will be delivered. In the evening there will be a citizens' reception.

On the following days the usual routine business will be transacted, papers will be read, and so on. A variety of social entertainments will be provided, and an afternoon is to be devoted to visiting the Zoological Garden.

Members of the Sub-Section of Anthropology, and others who are interested, will have an opportunity to examine the excavations at Madisonville, and to visit other localities of antiquarian interest near Cincinnati. After the adjournment of the Association, excursions will be organized on the Cincinnati Southern Railroad, and also, it is hoped, to the Mammoth Cave.

Beginning on the evening of August 16, and continuing through the meetings of the Association, there will be an exhibition of scientific apparatus, appliances, and collections. This exhibition is to be in charge of the Department of Science and Arts of the Ohio Mechanics' Institute, and a large amount of valuable material will be shown. Some of the leading dealers in chemicals, ap-

paratus, microscopes, minerals and zoological specimens have already notified the Special Committee of their intention to exhibit. The goods here displayed are to be kept over for the Ninth Cincinnati Industrial Exposition, opening September 7, the Managers of which have offered special premiums for this class of exhibits.

The local executive committee comprises the following names: A. T. Goshorn, Chairman; F. W. Clarke, Ormond Stone, Secretaries; Julius Dexter, Treasurer; J. D. Cox, William McAlpin, Herbert Jenney, George W. Jones, Archer Brown, C. W. Wendte, Robert Brown, Jr.

CONTRIBUTIONS TO COMPARATIVE PSYCHOLOGY.

By S. V. Clevenger, M. D.

II. LANGUAGE.

Excepting in Kussmaul's (1) elaborate essay, speech has had but little consideration anatomically and physiologically. The philologists and ethnologists have been trying to interpret phenomena while ignoring the mechanism directly concerned therein. As readily might the operations of a locomotive be explained by a person who had never seen one. Herbert Spencer, on the origin of language, is discursive and inconclusive. Darwin passes hastily over the subject in his "Descent of Man," but later (2) lays the foundation for a proper study. Bastian may be taken as the representative of the majority expressing opinions on language (3). He says: "Language was started by some hidden and unknown process of natural development or as a still more occult God-sent gift to man." If inquiries are to terminate in such assumptions, why not extend our conceptions of occult God-sent gifts, to the explanation of the Universe? Bastian's words mean, "I cannot fathom it, therefore, no one should try to do so."

Mivart (4) adopts the usually accepted divisions of language:

I. Sounds which are neither articulate nor rational, such as cries of pain, or the murmur of a mother to her infant.

II. Sounds which are articulate, but not rational, such as the talk of parrots, or of certain idiots, who will repeat, without comprehending, every phrase they hear.

III. Sounds which are rational, but not articulate, such as the inarticulate ejaculations by which we sometimes express assent or dissent from given propositions.

IV. Sounds which are both rational and articulate, constituting true speech.

V. Gestures which do not answer to rational conceptions, but are merely the manifestations of emotions and feelings.

VI. Gestures which do answer to rational conceptions and are, therefore, external, but not oral manifestations of the *mental* word. Such are many of the gestures of deaf mutes, who, being incapable of articulating words, have invented or acquired a language of gesture.

Analyzing these divisions, we find therein the prevailing idea to be that:

I. Language consists of speech and gesture (This essay will be directed toward proving that speech is also gesture; hence *Language is gesture accompanied, or not accompanied with sounds*).*

II. Language is voluntary or involuntary.

An impassable gulf exists between the voluntary and the involuntary in the minds of those who are disposed to reverence authority more than logic. The history of human thought proves Agnosticism to be a far better friend to man than Vaticanism or its disguises. Huxley (5) concludes that "We are conscious automata endowed with free will in the only intelligible sense of that much-abused term—inasmuch as in many respects we are able to do as we like—but none the less parts of the great series of causes and effects, which in unbroken continuity, composes that which is, and has been, and shall be—the sum of existence. As to the logical consequences of this conviction of mine, I may be permitted to remark that logical consequences are the scarecrows of fools, and the beacons of wise men. The only question which any wise man can ask himself, and which any honest man will ask himself, is whether a doctrine is true or false?" Kussmaul (6) feels justified in claiming that "each act of the will is always also the realization of a movement image previously sketched out in the recollection, or an entire chain of such movement images." * * * "*What we call the will is not only a motor, but always a sensory process.*" That which is involuntary in our actions appears, neurologically speaking, to be most evidently reflex, and those who know most about the mechanism of the will, know also that it is none the less reflex for being complex, or for having evaded the analysis of dualists and those ecclesiastically biased. It is from this automatical basis that I seek an explanation for the hitherto inexplicable. Brown-Sequard insists that speech is a reflex phenomenon (21). We find certain muscles, tendons, bones and cartilages concerned in mastication, and deglutition of food common to many vertebrates. Many of these same parts, separately or conjointly, prove useful to these animals in noise production: A woodpecker (7) finds by drumming rapidly upon a sonorous piece of wood, that he excites the admiration of his kind, and attracts attention to himself. When he repeats the operation for the distinct purpose of exciting admiration and attracting attention, he uses as much and precisely the same kind of reason, as the serenader, who pours out his rhyme to the jingle of a guitar. Wilder (8) speaks of the inharmonious feline nocturnes, and *Lieder ohne Worte*, but cats to whom that sort of music is addressed, find it quite as rational and expressive as the seranaded biped, and the greater part of both sorts of caterwauling, may be interpreted to mean the same thing, inharmonious only to those not interested.

Thus the brays, snorts, shrieks, grunts, etc., of the myriad kinds of animals are only methods for expressing their satisfaction or displeasure. Many such sounds being made use of after their accidental origination. The North American Indian uses the hoggish grunt in affirmation, and a perusal of Darwin's "Expression of the Emotions in Man and Animals" would be profitable to philologists who are not too strongly permeated by a metaphysical bias. At the outset any animal having observed that its noises, of whatever origin, attract attention of other animals would be led to the use of such noises as are serviceable. All that follows is simply an improvement upon these conceptions, and the animal that uses one noise or gesture, or a thousand, to bring itself into relation with other animals, expresses, in so doing, an idea, conveys a thought and hence speaks.

But this matter of reason and language possessed by animals has been ably worked out by observers and thinkers (9).

When water in an engine boiler is low and the alarm whistles through a simple float device; or when portions of machinery jar and scrape, the necessity for more water or oil is conveyed to the engineer's mind, and by a means comparable to the mechanism of crying. Just so the colony of beavers dive out of sight when they hear the warning slap of the sentinel's tail.

* No attempt at a perfect definition is made here. In fact the impossibility of absolute definiteness, in a world where everything is relative, seems, in this instance, not to have occurred to the metaphysicians. Language, owing to its blending of voluntary and involuntary, and consisting of gestures, used thoughtlessly, as well as those for expressing thought, is inseparable from other animal activities. One definition of Life is that it consists of Motion, but everything moves, hence everything lives, and there is no such thing as Death. Even the mathematical definition of a point is absurd and unthinkable. Who can define Health or Disease satisfactorily?

Professor Whitney, of Yale (10), thinks that "there needs to be, perhaps, a radical stirring up of the subject, a ventilation of a somewhat breezy, even gusty order, which shall make words fly high and noisily against one another before agreement shall be reached. If so, the sooner it is brought, in whatever way, the better; and they are no true promoters of the progress of Science who strive to smooth things over on the surface and act as if all were serene and accordant below." The gentleman just quoted might have made short work of his opponents had he approached the controversy physiologically.

M. Renan (11) says: "Languages have sprung forth completely formed from the very mould of the human spirit like Minerva from the head of Jupiter." Schleicher, Steinthal and Müller are guilty of similar puerilities. The latter claims that "animals cannot talk because they have no general ideas; they evidently have no general ideas because they do not talk." This sort of reasoning might be pardonable in scholars of metaphysical tendencies, but when we find Carl Vogt refusing to deal with the question, and Haeckel (12) saying, "Our ape-like progenitor very probably did not possess an articulate language of ideas," the appearance of this essay does not seem to require an apology. To deny, as Mivart (13) does, that "the cat, or any other beast or bird" has the gift of speech, and to base this denial upon man having a peculiar language of sounds and gestures to express his thoughts, is quite as sensible a proceeding as for the woodpecker to taunt man with his inability to drum in its peculiar way. "Psychology," says Mivart, "denotes the study of all the activities, both simultaneous and successive, which any living creature may exhibit." Mivart, therefore, is the grossest kind of materialist, without knowing it, for "Psyche," after this definition, consists of motion alone, and this pre-supposes a material origin. Kruse (14) mentions a deaf and dumb lad who, after having acquired a gesture language, told of years of abuse to which he had been subjected by an inhuman father and narrated other details of his previous life. Kussmaul cites this as an evidence of the speech faculty, upon its creation, finding everything prepared for it in the way of ideas to convey. The phylogenesis of speech should be studied by proper consideration of such facts. The dog only needs human speech to tell in words what he thinks and expresses in every other way beside when his master takes a gun to start on a hunt for game.

We may set aside all consideration of sound in language by remembering that persons entirely deaf may converse in the regular way, "judging of what was said by the movements of the lips and tongue, which they had learned to connect with particular syllables; and regulating their own voices in reply by their voluntary power, guided in its exercise by their muscular sensations" (15).

Speech therefore is the same as any other muscular act under the control of the will. The jaw is a limb, the parts accessory to which and concerned in its movements are as susceptible of cultivation as is the arm, and in the matter of speech acquisition, and the gradually better and better subjection to the mind of all bodily parts concerned in its expression. Herbert Spencer's words are applicable though the passages here given had no reference to the point under consideration:

"Functions, like structures, arise by progressive differentiations just as an organ is first an indefinite rudiment having nothing but some most general characteristic in common with the form it is ultimately to take; so a function begins as a kind of action, that is like the kind of action it will eventually become, only in a very vague way." (16) Thus a "lecture" by the Rev. Joseph Cook was predetermined by the bark of the primordial dog. (Vogt says "let them bark, it is their nature.")

"In animals, however, besides analogously structural changes wrought during the period of growth by subjection to circumstances unlike the ordinary circumstances, there are structural changes similarly wrought after maturity has been reached. Organs that have arrived at their full size possess a certain modifiability." (17) (This I would apply to the structural changes in the brain inevitable upon language learning as well as to those occurring through training or drilling in any art or trade involving manual dexterity or proficiency.)

"The growth of muscles exercised to an unusual degree is a matter of common observation. In the often cited blacksmith's arm, the dancer's legs, and the jockey's crural adductors we have marked examples of modifiability which almost every one has to some extent experienced. It is needless to multiply proofs. The occurrence of changes in the structure of the skin when exposed to a stress of function is also familiar. That thickening of the epidermis on a laborer's palm results from continuous pressure and friction is certain." * * * "An orchestral conductor gains by continual practice an unusually great ability to discriminate differences of sound, and in the finger reading of the blind we have evidence that the sense of touch may be brought by exercise to a far higher capability than is ordinary. The increase of power which habitual exertion gives to mental faculties needs no illustration, every person of education has personal experience of it." (18)

Language, therefore, may be regarded as pure gesticulation and its perfectibility as dependent upon the gradual evolution of the reasoning powers of animals. This being the case, it requires but a glance at the construction of the jargons of to-day (by courtesy called languages) to convince us of the very low plane to which man with his much vaunted intellect has arrived. From the teleological standpoint, certainly German with its nonsensical genders, French with its slaughter of letters for euphony sake, and English with its multitude of barbarisms, must have had more of a malign than divine origin. (But then the tower of Babel story accounts for it all.) Maudsley (19) mentions the inability of the Bosjesmen to talk in the dark, owing to their depending more upon signs than vocables for intercommunication. The North American Indians can thus converse without uttering a single sound. Laura Bridgeman may also be mentioned as expressing her thoughts, and even "muttering" in her dreams by finger motions. The necessity for such considerations as the foregoing appears in the philological bias which has crept into our physiological literature through the one-sided studies of such men as von Schlegel, and through their claims that the perfectly regular and complex construction of languages of many barbarous nations is a proof of the divine origin of language. By placing language upon an equal footing with all other voluntary gestures we see at once that speech is entitled to no more regard than any other set of complex motions performed by any animal to subserve rational purposes. We cannot deny the possession of rational language to animals when we see them conveying their thoughts and desires with and without sounds, by menaces, contortions, glarings, and a multitude of other movements. I have known mules and oxen on the arid plains of the West to acquaint a thirsty herd half a mile away that water has been discovered. All of us know of the hen's ability to talk to its chickens. The most perfect rhetoric and oratory of man can be said, therefore, to differ from these animal expressions only *in degree*, and often the most pretentious discourse conveys fewer ideas than the cluck of a hen or the growl of a dog. A pure linguist, hence, can claim but little more in an intellectual way than a pure gymnast. Different groups of muscles, nerves, bones, etc., are exercised and cultivated by each. Man can claim no more for developing adroitness in the use of his jaws, lips, tongue and larynx than any animal which, finding itself in possession of certain other limbs and groups of muscles turns them to the utmost possible advantage. The great function of the jaw was masticatory, its use

in enunciation of words was subsequently developed. The hands of our progenitors were adapted to climbing trees and by subsequent training are made dexterous in us in the use of tools. The point I desire to bring prominently into view is, that the speech faculty has for its basis nothing more important than prehensile abilities. The mechanic is entitled to the same amount of respect as the linguist; in fact, the mechanic is more apt to have acquired a respectable amount of skill in the use of his tools, as generally his labor is directed to some useful and definite end; not necessarily so with the linguist, his acquisition of a few jargons frequently causes him to be mistakenly regarded as intellectual. It is not the ability to use tools or to speak that elevates man above his fellow animals, for man is not the only animal that speaks or uses tools. The intellectual differences between men consist in the greater power of co-ordination and correlation of faculties.

Dr. M. Dax, in 1836, designated the left anterior lobe of the brain as the seat of language, because loss of speech often coexisted with disease of this part, though the labors of Bouilland previously had paved the way for this definiteness. Aubertin and Broca finally assigned this faculty as centralized in the "operculum," and Dr. Wm. A. Hammond (20), in reviewing the subject, concludes that: "The integrity of the posterior part of the third frontal convolution, and perhaps of the second, is indispensable to the normal operation of the function of speech." Hughlings Jackson, and Ferrier agree with Broca in restricting the location to the operculum, but Dr. Hammond (20) claims:

1. "That the organ of language is situated in both hemispheres, and in that part which is nourished by the middle cerebral artery.

2. "That while the more frequent occurrence of right hemiplegia, in connection with aphasia, is in great part the result of the anatomical arrangements of the arteries which favors embolism on that side, there is strong evidence to show that the left side of the brain is more intimately connected with the faculty of speech than the right."

I would like to suggest to the advocates of opercular and insular localization an idea which has probably not been previously advanced, to wit: The sinistral nature of central cerebral speech innervation has, doubtless, some relation to the azygous tendencies of the parts concerned therein; for example, the tongue, uvula, maxillæ, vocal cords, etc., though not strictly fused or impaired, present peculiarities of structure and synchronism of motion of the bi-laterally placed parts widely different from those of the extremities, which could easily influence innervation to centralize upon one side of the brain, particularly when favored by the better blood supply afforded by the left middle cerebral artery. Were the two hands of man joined so as to restrict motion mainly to a perpendicular plane, as in the case of the lower jaw, then we might expect the summit of the ascending frontal convolution on the left side to develop over the corresponding part on the right side as a centre for arm motions. But this matter of localization has not been firmly established. Dr. E. C. Spitzka, before the Medical Society of the County of New York in 1877, reviewed "The Localization of Cerebral Diseases in the Light of recent Anatomical Discoveries" (22). Spitzka acknowledged that "the fibres which ultimately abut in the hypoglossal and facial nerve nuclei can be traced into the operculum and island, giving us an anatomical basis for the aphasic symptom," but insisted that "our faculty of speech is certainly more complex than is generally supposed, and the terms amnesic and ataxic aphasia, by no means exhaust the possible pathological interferences with its delicate mechanism. The first step in the acquirement of speech is its phonetic element. We hear a word or sound, and as far as it is a mere sound impression it is registered in a sensory area of the cortex. We then experiment, as it were, with our motor appartus, until we find the combination requisite to repeat said word or sound. This motor innervation has its conscious seat in Meynert's region, while the sensory perception is located in a distant area (probably, though not certainly) the occipital lobe. Now in order that the sensory perception may control the "correctness" of the motor expression the two must be associated. It will then be indifferent, whether the sensory center, the motor center, or the associating band be destroyed, we will have aphasia in either case. And there are still more intimate relations which may be equally interfered with, causing either aphasia, agraphia, alexia, or a combination of any two of these, or all." * * * "Any intricate intellectual processes *must* involve the greater part, or the whole, of one hemisphere." This was *a posteriori* completely, and "localizers" should not fail to read the proceedings of that meeting carefully. These views are consistent with the theory I recently presented to the American Neurological Society concerning the histogenetic function of nerve cells in opposition to their being "force producers." Spitzka has shown that the Island of Reil has nothing whatever to do with the development of the speech faculty. In some aberrant forms he found this lobe largely developed. It would seem that primarily this region has, if it have any connection at all with speech innervation, only a certain convenience of situation, an accidental contiguity to certain fasciculi which was taken advantage of as the speech faculty developed.

(1). Ziemssen's Cyclopædia.
(2). "Expression of the Emotions in Man and Animals."
(3). "The Brain as an organ of the Mind."
(4). "The Cat.'
(5). "On the Hypothesis that Animals are Automata and its history."
(6). Op. Cit.
(7). The Duke of Argyll, in *Nature*. See "SCIENCE," Vol. I, p. 24.
(8). "Anatomical uses of the Cat."
(9). Houzeau. "*Etude sur les facultés mentales des animaux comparées à celles de l'homme*, Mons. 1872. Bechstein "*Naturgeschichte der Hof und Stubenvogel.*, C. G. Leroy, Intelligence and Perfectibility of Animals."
(10). "Are languages Institutions? *Contemporary Review.*
(11). "*Origine du Langage*," Chap. III.
(12). *Naturliche Schöpfungsgeschichte*."
13. Op. Cit.
(14). "*Ueber die Taubstummen*" *u. s. w. Schleswig*, 1853, *S.* 54.
(15). Carpenter's Physiology, p. 727.
(16). Principles of Biology, Vol. I., p. 157.
(17). Ibid, Chap. V., p. 184.
(18). Loc. Cit., p. 187.
(19). "Physiology of the Mind.
(20). "Diseases of the Nervous System," Seventh Edition, 1881, p. 182, *et seq.*
(21). E. C. Seguin, *Quarterly Journal of Psychological Medicine*, Jan. 1868.
(22). Journal Nervous and Mental Disease, Vol. IV, pp. 724-734.

ÆTHER.

By Pliny Earle Chase.

Professor of Philosophy in Haverford College, Pa.

The laws of æthereal action and re-action are laws of action and re-action in an elastic atmosphere.

The following well known laws have an important bearing upon photodynamics and other æthereal researches:

1. Cyclical activities may often be accurately represented by formulas which introduce mean or average ve-

locities and mean *vis viva*. This is the foundation of Maxwell's theory of the equality of mean *vis viva* in the molecular movements of different gases at equal temperatures, and of Pfaundler's discovery that in estimating the heat of dissociation, the mean should be taken between the temperatures of incipient and of complete dissociation.

2. The projectile force, which produces flight or cyclical motion against any central acceleration or retardation, is equivalent to the mean acceleration or retardation multiplied by one-half the time of flight or cyclical motion.

3. The velocities of wave motion in elastic fluids, and of cosmical and molecular orbital motion, can all be expressed by the common formula $v = \sqrt{2\,gh}$.

4. Every periodic vibrating or orbital motion can be regarded as the sum of a certain number of pendulum vibrations. (*Fourier's theorem.*)

5. The distance of the centre of oscillation from the centre of relative stability is at two thirds of the length of a linear pendulum, or at the square root of four tenths of radius in a rotating sphere.

6. The acceleration of any force, which is uniformly diffused from or towards a given centre, varies inversely as the square of the distance from the centre.

7. Times of revolution under the action of such forces, vary as the three halves power of the distance; distances vary as the two thirds power of the time.

8. Centres of inertia, or nodes, in a vibrating elastic medium, tend to produce harmonic nodes.

9. The mutual inter-actions of cosmical, molecular or atomic bodies are proportioned to the respective masses; actions which are considered with reference to a single active centre vary directly as the mass and inversely as the square of the distance.

10. In elastic atmospheres the densities decrease in geometrical progression, as the height above the surface increases in the arithmetical progression.

11. Living force, or *vis viva*, is proportional to the product of mass by the square of the velocity.

12. The distance of projection against uniform resistance is proportioned to the living force.

13. In condensing nebulæ, the velocity of circular orbital revolution is acquired by subsidence, from a state of rest, through one-half of radius.

The following additional propositions may be readily deduced from the foregoing.

14. *Mean vis viva* may be represented by the *vis viva* of centres of oscillation.

15. The force of planetary projection should be referred to perihelion; the force of incipient subsidence, to aphelion.

16. In synchronous orbits, the mean velocity of rectilinear oscillation is to the velocity of circular orbital oscillation as twice the diameter is to the circumference.

17. The acceleration or retardation of a centripetal force varies as the fourth power of the velocity of orbital revolution.

18. In cyclical motions, the resultant of all internal forces must be in equilibrium with the resultant of all external forces, at the expiration of each half cycle.

19. The modulus of cyclical motion is equal to the product of mean acceleration by the square of the time of a half cycle.

20. The sum of all external forces may, therefore, be represented by a velocity which is equivalent to the mean or resultant internal force acting for one-half of the cyclical time.

21. The influence of a central force which acts at the extremity of a linear pendulum is nine times as great upon the centre of oscillation, as its influence upon the centre of suspension.

22. The limiting *vis viva* of wave propagation is five-ninths of the mean *vis viva* of the oscillating particles.

23. In condensing nebulæ, rupturing forces which are due to central subsidence may be represented by fractions in which the denominator is one greater than the numerator.

24. In synchronous rotation and revolution, the nucleal radius varies as the three-fourths power of the limiting atmospheric radius.

25. The variation in mean *vis viva* of gaseous volume is to the variation in *vis viva* of uniform velocity as 1 is to 1.4232.

26. The mean thermal and mechanical influences of the sun must be in equilibrium.

27. The collisions of particles, in subsiding towards a centre of force, tend to form belts at the centre of linear oscillation.

28. The limiting velocity between tendencies to aggregation and tendencies to dissociation is to the velocity in a circular orbit as the ratio of the circumference of a circle to its diameter is to the square root of two.

29. In explosive, as well as in cyclical motions, equilibrium must be established between internal and external forces.

30. Apsidal and mean planetary positions must also be controlled by like tendencies to equilibrium.

31. Undulations in an elastic medium maintain the primitive velocity which is due to their place of origination.

32. When two or more cyclical motions are combined, they must all be modified by the tendency to conservation of areas.

33. In expanding or condensing nebulæ, the conservation of areas maintains a constant value for the modulus of rotation.

34. Instantaneous action between different masses or particles, by mere material intervention, is impossible.

35. In synchronous motions about different centres, the mean distances from the centres of motion vary as the cube root of the masses or other controlling forces.

36. Constant velocities, in a homogeneous elastic medium, represent constant living forces.

37. The time of acquiring orbital velocity, at Laplace's limit of possible atmosphere, is to the time of acquiring "nascent" or dissociative velocity at the nucleal limit, as the diameter of a circle is to its circumference.

These laws are applicable in all branches of radiodynamics, viz.: photodynamics, thermodynamics, electrodynamics, cosmodynamics, chemical physics, hydrodynamics and pneumatics.

COMET C, 1881.

At 3 A. M., of the 14th instant, a comet was observed at Ann Arbor by Mr. J. M. Schaeberle, an amateur astronomer, who has the privilege of the University Observatory.

Mr. Henry M. Parkhurst, of Brooklyn, whose recent calculations on comet B, 1881, proved to be very accurate, has published in the New York *Herald* the following observations on Mr. Schaeberle's comet:

"The position of the new comet on the 20th instant at 2h. 46m., Washington mean time, was:—Right ascension, 5h. 54m. 58s.; North declination, 40 degrees, 40 minutes. This shows a motion of 29 minutes per day—an increase of 7 minutes—showing that the comet is not so distant as I had hoped. I have not succeeded in reconciling my two positions with that telegraphed for the time of discovery. To satisfy the right ascension given the comet must have already passed its perihelion and be moving in such an orbit that it will pass between the earth and sun within a fortnight, and be no more seen in this hemisphere. The increased brightness this morning tends to support this idea. Yet it may not have reached its perihelion; in which case it may be visible for a month longer. I shall be compelled to wait for a third accurate observation before I can determine the orbit more exactly. In any event the comet is coming directly toward the earth, and it will become much brighter than at present, so that it will probably be visible to the naked eye as soon as the moonlight ceases to interfere. It is now about 12 degrees southeast of Capella, the bright star in the northeastern sky at 3 o'clock in the morning."

COMET B, 1881.

With the drawings of the above comet we received from Professor Edward S. Holden the following letter:

WASHBURN OBSERVATORY,
MADISON, WISCONSIN,
July 9, 1881.

To the Editor of "SCIENCE."

MY DEAR SIR—As you request, I send you with this, the drawings of the head of the bright comet which have been made here. The 15½-inch equatorial, with the zone eyepiece (field 25'.5, power 145), has been used. The drawings have all been made by me, and in them *the darker the shading, the brighter the corresponding part* of the comet.

Very sincerely yours,
EDWARD S. HOLDEN.

DESCRIPTION OF ILLUSTRATIONS.

Figure 1. June 24, 1881, 14h., m. t.

" 2. " 25, " 10h., m. t. Hazy and outlines of comet not well seen. The drawing shows only the structure of the head. The nucleus is not round, and is eccentric in the envelopes.

Figure 3. June 26, 1881., 11h., 22m., m. t. Hazy and clouds. The dark semi-circular line in upper part of nucleus represents a dark part.

Figure 4. June 27, 1881, 13h., m. t.

" 5. " 28, " 10h., m. t.

" 6. " 29, " 9h., 30m., m. t.

" 7. July 8, " 10h., 35 m.—Moonlight. The nucleus is not double. There is a dark, narrow channel between the following side of the nucleus and the envelopes, as in the figure.

Figure 8. July 11, 1881, 9h., 30m., m. t.—Strong moonlight and twilight.—In this figure, which is engraved differently to the others, the white part represents light, and the shading darker portions.

FIGURE 4.

Advices from Europe state that this comet was observed by Dr. Elkin, of the Royal Observatory, Cape of Good Hope, who states that after a week of overcast sky the comet was found there on May 31. Mr. L. A. Eddie, F.R.A.S., of Graham's Town, saw it on May 27, and others claim to have seen it two days earlier. On June 4 the tail was 6° long, coma 20 minutes, and nucleus 20 seconds in diameter; the comet was as bright as α Columbæ.

Mr. William Huggins states that "On Friday night, (June 24) I obtained, with one hour's exposure, a photograph on a gelatin plate of the more refrangible part of the spectrum of the comet which is now visible. This photograph shows a pair of bright lines a little way beyond H in the ultra-violet region, which appear to belong to the spectrum of carbon (in some form) which I observed in the visible region of the spectra of telescopic comets in 1866 and 1868. There is also in the photograph a continuous spectrum in which the Fraunhofer lines can be seen. These show that this part of the comet's light was reflected solar light.

FIGURE 3.

This photographic evidence supports the results I obtained in 1868, showing that comets shine partly by reflected solar light, and partly by their own light, the spectrum of which indicates the presence in the comet of carbon, possibly in combination with hydrogen."

The following spectroscopic notes, by W. H. M. Christie, of the Royal Observatory, Greenwich, will be read with interest:

With the Sheepshanks equatorial (6¾ inches aperture) the head showed the want of symmetry that has been remarked in some other comets. On June 24 the preceding side was much the brighter, there being a strong brush or

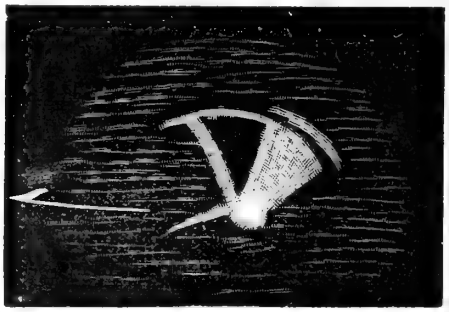

FIGURE 8.

arc of light on that side, with a bright fan close to the nucleus and a much smaller arc on the following side, the two arcs appearing to spring from the nucleus on opposite sides, and higher up to interlace. A very remarkable feature was a straight wisp of light extending from the nucleus nearly along the axis of the tail. On June 25 this had become much less striking, and the appearance of the head had entirely changed. The following side was then much the brighter, and the general appearance was that of a parabolic envelope, with a much brighter unsymmetrical parabola placed within it, the latter having its focus on the following side of the nucleus, and its axis turned round in the direction *n p s f* from that of the tail.

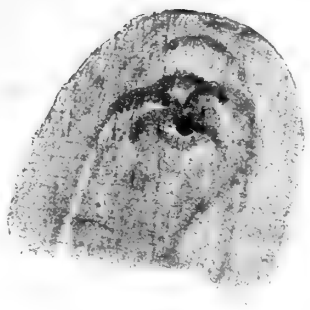

Fig. 5.

Fig. 2.

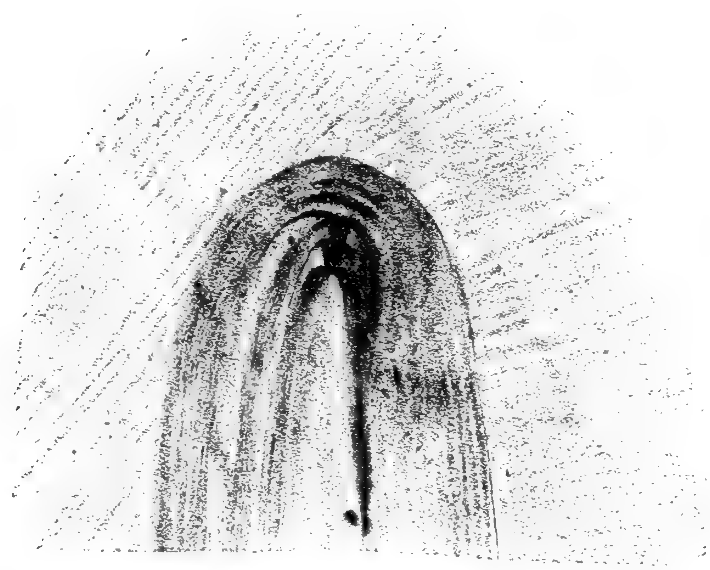

Fig. 1.

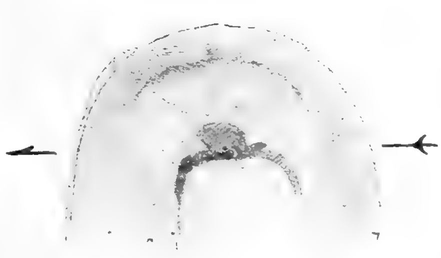

Fig. 7.

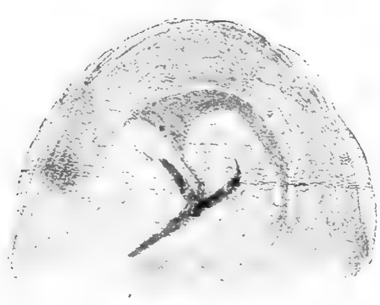

Fig. 6.

Drawings of Comet B, 1881, made by Prof. EDWARD S. HOLDEN, Washburn Observatory, Wis.

The greater part of the head gave a bright continuous spectrum, obliterating the usual cometary bands, but one portion showed three bands, in the green, blue, and violet respectively. Measures of the principal band in the green show that it coincides with the band in the first spectrum of carbon (blue base of flame) at 5165, and not with that of the second spectrum (vacuum-tube) at 5198. The bands in the blue and violet appear to correspond, as nearly as could be estimated, with bands in the first spectrum of carbon. These observations were made with the half-prism spectroscope mounted on the $12\frac{3}{4}$-inch equatorial, a dispersive power of about $18\frac{1}{2}°$ from A to H being used, with a magnifying power of 14 on the view-telescope, as in the measures of star-motions in the line of sight. No decided polarisation was detected either in the head or the tail. Cloudy weather has prevented any observation of the comet since June 25.

THE UNITY OF NATURE.

By the Duke of Argyll.

IX.

THE ORIGIN OF RELIGION CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(*Continued.*)

These conceptions seems to have taken their form from the very violence of the revulsion which they indicate and explain. The peculiar tenet of Buddhism, which is or has been interpreted to be a denial of any Divine Being or of personal or individual immortality, seems the strangest of all doctrines on which to recommend a life of virtue, of self-denial, and of religious contemplation. But the explanation is apparently to be found in the extreme and ridiculous developments which the doctrines of Divine Personality and of individual immortality had taken under the Brahminical system. These developments do indeed seem almost incredible, if we did not know from many other examples the incalculable wanderings of the human imagination in the domain of religious thought. The doctrine of the transmigration of souls at death into the bodies of beasts was a doctrine pushed to such extravagances of conception, and yet believed in with such intense conviction that pious Brahmins did not dare even to breathe the open air lest by accident they should destroy some invisible animalculæ in which was embodied the spirits of their ancestors. Such a notion of immortality might well oppress and afflict the spirit with a sense of intolerable fatigue. Nor is it difficult to understand how that desire of complete attainment, which is, after all, the real hope of immortality, should have been driven to look for it rather in reabsorption into some one universal Essence, and so to reach at last some final rest. Freedom from the burden of the flesh, rendered doubly burdensome by the repeated cycles of animal existence which lay before the Brahmin, was the end most naturally desired. For, indeed, complete annihilation might well be the highest aspiration of souls who had before them such conceptions of personal immortality and its gifts. A similar explanation is probably the true one of the denial of any God. A prejudice had arisen against the very idea of a Divine Being from the concomitant ideas which had become associated with personality. The original Buddhist denial of a God was probably in its heart of hearts merely a denial of the grotesque limitations which had been associated with the popular conceptions of Him. It was a devout and religious aspect of that most unphilosophical negation which in our own days had been called the "Unconditioned." In short, it was only a metaphysical, and not an irreligious, Atheism. But although this was probably the real meaning of the Buddhistic Atheism in the mind of its original teachers, and although this meaning has reappeared and has found intelligent expression among many of its subsequent expounders, it was in itself one of those fruitful germs of error which are fatal in any system of Religion. The negation of any Divine Being or Agency, at least under any aspect or condition conceivable by Man, makes a vacuum which nothing else can fill. Or rather, it may be said to make a vacuum which every conceivable imagination rushes in to occupy. Accordingly, Buddha himself seems to have taken the place of a Divine Being in the worship of his followers. His was a real personality—his was the ideal life. All history proves that no abstract system of doctrine, no mere rule of life, no dreamy aspiration however high, can serve as an object of worship for any length of time. But a great and a good man can always be deified. And so it has been with Buddha. Still, this deification was, as it were, an usurpation. The worship of himself was no part of the Religion he taught, and the vacuum which he had created in speculative belief was one which his own image, even with all the swellings of tradition, was inadequate to fill. And so Buddhism appears to have run its course through every stage of mystic madness, of gross idolatry, and of true fetish-worship, until, in India at least, it seems likely to be reabsorbed in the Brahminism from which it originally sprang.

And so we are carried back to the origin of that great Religion, Brahminism, which already in the sixth or seventh century before the Christian era had become so degraded as to give rise to the revolt of Buddha. The course of its development can be traced in an elaborate literature which may extend over a period of about 2000 years. That development is beyond all question one of the greatest interest in the history of Religion, because it concerns a region and a race which have high traditional claims to be identified with one of the most ancient homes, and one of the most ancient families of man. And surely it is a most striking result of modern inquiry that in this, one of the oldest literatures of the world, we find that the most ancient religious appellation is Heaven-Father, and that the words "Dyaus-pitar" in which this idea is expressed are the etymological origin of Jupiter Ζεύςπατήρ—the name for the supreme Deity in the mythology of the Greeks.

We must not allow any preconceived ideas to obscure the plain evidence which arises out of this simple fact. We bow to the authority of Sanskrit scholars when they tell us of it. But we shall do well to watch the philosophical explanations with which they may accompany their intimations of its import. Those who approach the subject with the assumption that the idea of a Divine Being or a Superhuman Personality must be a derivative, and cannot be a primary conception, allow all their language to be colored by the theory that vague perceptions of "The Invisible" or of "The Infinite," in rivers, or in mountains, or in sun and moon and stars, were the earliest religious conceptions of the human mind. But this theory cannot be accepted by those who remember that there is nothing in Nature so near to us as our own nature,—nothing so mysterious and yet so intelligible,—nothing so invisible, yet so suggestive of energy and of power over things that can be seen. Nothing else in Nature speaks to us so constantly or so directly. Neither the Infinite nor the Invisible contains any religious element at all, unless as conditions of a Being of whom invisibility and infinitude are attributes. There is no probability that any abstract conceptions whatever about the nature or properties of material Force can have been among the earliest conceptions of the human mind. Still less is it reasonable to suppose that such conceptions were more natural and more easy conceptions than those founded on our own personality and the personality of parents. Yet it seems as if it were in deference to this theory that Professor

Max Müller is disposed to deprecate the supposition that the "Heaven-Father" of the earliest Vedic hymns is rightly to be understood as having meant what we mean by God. Very probably indeed it may have meant something much more simple. But not the less on that account it may have meant something quite as true. I do not know, indeed, why we should set any very high estimate on the success which has attended the most learned theologians in giving anything like form or substance to our conceptions of the Godhead. Christianity solves the difficulty by presenting, as the type of all true conceptions on the subject, the image of a Divine Humanity, and the history of a perfect Life. In like manner, those methods of representing the character and attributes of the Almighty, which were employed to teach the Jewish people, were methods all founded on the same principle of a sublime Anthropomorphism. But when we come to the abstract definitions of Theology they invariably end either in self contradictions, or in words in which beauty of rhythm takes the place of intelligible meaning. Probably no body of men ever came to draw up such definitions with greater advantages than the Reformers of the English Church. They had before them the sublime imagery of the Hebrew Prophets—all the traditions of the Christian world—all the language of philosophy—all the subtleties of the Schools. Yet of the Godhead, they can only say, as a negative definition, that God is "without body, parts, or passions." But, if by "passions" we are to understand all mental affections, this definition is not only in defiance of the whole language of the Jewish Scriptures, but in defiance also of all that is conceivable of the Being who is the author of all good, the fountain of all love, who hates evil, and is angry with the wicked every day. A great master of the English tongue has given another definition in which, among other things it is affirmed that the attributes of God are "incommunicable."[4] Yet, at least, all the good attributes of all creatures must be conceived as communicated to them by their Creator, in whom all fullness dwells. I do not know, therefore, by what title we are to assume that "what we mean by God" is certainly so much nearer the truth than the simplest conceptions of a primeval age. It is at least possible that in that age there may have been intimations of the Divine Personality, and of the Divine Presence which we have not now. Moreover, there may have been developments of error in this high matter, which may well shake our confidence in the unquestionable superiority of "what we mean by God" over what may have been meant and understood by our earliest fathers in respect to the Being whom they adored. Some conceptions of the Divine Being which have been prevalent in the Christian Church, have been formed upon theological traditions so questionable that the developments of them have been among the heaviest burdens of the Faith. It is not too much to say that some of the doctrines derived from scholastic theology, and once most widely accepted in the Christian Church—such, for example, as the fate of unbaptized infants—are doctrines which present the nature and character of the Godhead in aspects as irrational as they are repulsive. One of the most remarkable schools of Christian thought which has arisen in recent times is that which has made the idea of the "Fatherhood of God" the basis of its distinctive teaching. Yet it is nothing but a reversion to the simplest of all ideas, the most rudimentary of all experiences —that which takes the functions and the authority of a father as the most natural image of the Invisible and Infinite Being to whom we owe "life and breath and all things." In the facts of Vedic literature, when we carefully separate these facts from theories about them, there is really no symptom of any time when the idea of some Living Being in the nature of God had not yet been attained. On the contrary, the earliest indications of this conception are indications of the sublimest character, and the process of evolution seems distinctly to have been a process not of an ascending but of a descending order. Thus it appears that the great appellative "Dyaus," which in the earliest Vedic literature is masculine, and stood for "The Bright or Shining One," or the Living Being whose dwelling is the Light, and in later times become a feminine, and stood for nothing but the sky.[5] It is quite evident that in the oldest times of the Aryan race, in so far as those times have left us any record, not only had the idea of a Personal God been fully conceived, but such a Being had been described, and addressed in language and under symbols which are comparable with the sublimest imagery in the Visions of Patmos. How firmly, too, and how naturally these conceptions of a God were rooted in the analogies of our own human personality, is attested by the additional fact that Paternity was the earliest Vedic idea of Creation, and Dyaus was invoked not only as the Heaven-Father, but specially as the "Dyaush pitâ ganitâ," which is the Sanskrit equivalent of the Greek Ζεὺς πατὴρ γενετήρ.

When, again, we are told by Sanskrit scholars that the earliest religious conceptions of the Aryan race, as exhibited in the Veda, were Pantheistic, and that the Gods they worshiped were "Deifications" of the Forces or Powers of Nature, we are to remember that this is an interpretation and not a fact. It is an interpretation, too, which assumes the familiarity of the human mind in the ages of its infancy with one of the most doubtful and difficult conceptions of modern science—namely, the abstract conception of Energy or Force as an inseparable attribute of Matter. The only fact, divested of all preconceptions, which these scholars have really ascertained is, that in compositions which are confessedly poetical the energies of Nature were habitually addressed as the energies of Personal or Living Beings. But this fact does not in the least involve the supposition that the energies of Nature which are thus addressed had, at some still earlier epoch, been regarded under the aspect of Material Forces, and had afterwards come to be personified, nor does it in the least involve the other supposition that, when so personified, they were really regarded as so many different beings absolutely separate and distinct from each other. Both of these suppositions may indeed be matter of argument; but neither of them can be legitimately assumed. They are, on the contrary, both of them open to the most serious, if not to insuperable objections. As regards the first of them—that the earliest human conceptions of Nature were of that most abstruse and difficult kind which consists in the idea of Material Force without any living embodiment or abode, I have already indicated the grounds on which it seems in the highest degree improbable. As regards the second supposition —viz., that when Natural Forces came to be personified each one of them was regarded as the embodiment of a separate and distinct Divinity—this is a most unsafe interpretation of the language of poetry. The purest Monotheism has a Pantheistic side. To see all things in God is very closely related to seeing God in all things. The giving of separate names to divers manifestations of one Divine Power may pass into Polytheism by insensible degrees. But it would be a most erroneous conclusion from the use of such names at a very early stage in the history of religious development, that those who so employed them had no conception of One Supreme Being. In the Philosophy of Brahminism even, in the midst of its most extravagant Polytheistic developments, not only has this idea been preserved, but it has been taught and held as the central idea of the whole system. "There is but one Being—no second." Nothing really exists but the one Universal Spirit, called Brahmin; and whatever appears to exists independently is identical with that

[4] J. H. Newman, "Idea of a University," p. 60.

[5] Hibbert Lectures, pp. 276, 277.

Spirit.[6] This is the uncompromising creed of true Brahminism. If, then, this creed can be retained amidst the extravagant Polytheism of later Hindu corruptions, much more easily could it be retained in the early Pantheism of the Vedic hymns.

There is, however, one kind of evidence remaining, which may be said to be still within the domain of history, and that is the evidence derived from language, from the structure and etymology of words. This evidence carries us a long way further back, even to the time when language was in the course of its formation, and long before it had been reduced to writing. From this evidence as we find it in the facts reported respecting the earliest forms of Aryan speech, it seems certain that the most ancient conceptions of the energies of Nature were conceptions of personality. In that dim and far-off time, when our prehistoric ancestors were speaking in a language long anterior to the formation of the oldest Sanskrit, we are told that they called the sun the Illuminator, or the Warmer, or the Nourisher; the moon, the Measurer; the dawn, the Awakener; the thunder, the Roarer; the rain, the Rainer; the fire, the Quick-Runner.[7] We are told further that in these personifications the earliest Aryans did not imagine them as possessing the material or corporeal forms of Humanity, but only that the activities they exhibited were most easily conceived as comparable with our own. Surely this is a fact which is worth volumes of speculation. What was most easy and most natural then must have been most easy and most natural from the beginning. With such a propensity in the earliest men of whom we have any authentic record to see personal agency in everything, and with the general impression of unity and subordination under one system which is suggested by all the phenomena of Nature, it does not seem very difficult to suppose that the fundamental conception of all Religion may have been in the strictest sense primeval.

But the earliest records of Aryan worship and of Aryan speech are not the only evidences we have of the comparative sublimity of the earliest known conceptions of the Divine Nature. The Egyptian records are older still; and some of the oldest are also the most sublime. A hymn to the rising and setting sun, which is contained in the 125th chapter of the "Book of the Dead," is said by Egyptian scholars to be "the most ancient piece of poetry in the literature of the world."[8] In this Hymn the Divine Deity is described as the Maker of Heaven and of Earth, as the Self-existent One; and the elementary forces of Nature, under the curious and profound expression of the "Children of inertness," are described as His instruments in the rule and government of Nature.[9] Nor is it less remarkable that these old Egyptians seem to have grasped the idea of Law and Order as a characteristic method of the Divine Government. He who alone is truly the Living One is adored as living in the Truth, and in Justice considered as the unchanging and unchangeable Rule of Right, in the moral world, and of order in the physical causation.[10] The same grand conception has been traced in the Theology of the Vedas. The result of all this historical evidence may be given in the words M. Renouf: "It is incontestably true that the sublimer portions of the Egyptian Religion are not the comparatively late result of a process of development or elimination from the grosser. The sublimer portions are demonstrably ancient; and the last stage of the Egyptian Religion, that known to the Greek end Latin writers, was by far the grossest and most corrupt."

[6] Professor Monier Williams, "Hinduism," p. 11.

[7] Max Muller, Hibbert Lectures, 1878, p. 193.

[8] Renouf Hibbert, Lectures, 1879, p. 197.

[9] Hibbert Lectures. by Renouf, pp. 198, 199.

[10] *Idem*, 1879, pp. 119, 120.

ANCIENT PLANETARY RINGS, VOLUME, MASS AND DENSITY.

BY EDGAR L. LARKIN.

IV.

In Astronomical literature there is engrafted a venerable doctrine giving details of the processes of evolution of the solar system, from a mass of incandescent gas. The theory is a hundred years old. It says, all matter now in the sun and planets was once in a state of rare gas, extending beyond the orbit of Neptune. The gas was hot; it cooled, contracted, and rotated. When by condensation it had dwindled to the insignificant limits of the Neptunian orbit, its velocity of rotation was so great that a ring of gas was detached from the equator of the shrinking sphere. This ring in time formed Neptune. In like manner all the planets were formed, the residue of the primordial mass being the sun. This error has been taught to children, and so tenacious are the traditions of youth, that geometers have been known to cling to the illusion in mature years. It has but one rival—perpetual motion—and is known as the Nebular Hypothesis. If it is true it can be handled by arithmetic; if false, computation will detect the fallacy.

How shall it be attacked; and what can be learned of the primeval state of matter? Can we peer into the depths of primordial time when worlds were in development? The geologist penetrates strata, and writes the records of the earth. Can the history of Neptune be written? And can we trace the processes of its evolution? If so, the mass, volume and thence the density, of the ring whence it formed must be determined. We know its mass in terms of terrestrial matter, it was 102 sextillion tons, or 204 septillion pounds; because that is the amount of matter now in Neptune. By what possible means can its volume be learned? The problem seems incapable of solution, mathematics apparently being unable to furnish a method of grappling with the question. We have used diligence to find records showing that the volume and density of the ring have ever been calculated, and failed. But there is one way of learning the magnitude of the mass of gas whence Neptune condensed. It is based on the doctrine of the CENTRE OF GRAVITY, and it is a fact in nature which subverts the Nebular Hypothesis. We know that if the revolving sphere discarded equatorial matter to make Neptune, the planet formed in the line of its centre of gravity. There are formulæ for the determination of the distances of centres of gravity of segments from the centre of the circles whence they were cut. There are only three possible forms of rings that can be cut from the periphery of a sphere—segmental, cylindric and another, whose sections are in shape like sections cut by a perpendicular plane passing through a bi-convex lens. This geometrical figure is formed by the revolution of a segment of a circle about its chord held quiescent; and the solid generated is a circular spindle. This form we overlooked in the previous paper. The volumes of these rings are sought, the data being the distances of their centres of gravity from the centre of the sphere, which is the distance of Neptune from the sun—2,780,000,000 miles. It has been shown in these notes that the radius of the only sphere large enough to afford a segment of sufficient size to have its centre of gravity coincide with Neptune's orbit, was three (3) billion miles. The dimensions of this segmental ring cut off by passing the chord of the segment around the sphere, were: chord, 2,600,000,000; altitude, 300,000,000; and length, 17,500,000,000 miles, the length of the path of Neptune. Therefore its volume was nine (9) octillion cubic miles, and as this number of miles had to contain 204 septillion pounds, one cubic mile held .0224 pounds, or 157 grains, 45 cubic miles being required to contain one pound of gas.

"At 15.5° C, (60° F.), and 30 inches barometric pressure, 100 cubic inches of Hydrogen weigh 2.14 grains."

Fowne's Chemistry, p. 137. Thence one cubic mile of hydrogen weighs over five (5) trillion grains, or 777 million pounds. And as the ring was of such density as to require one cubic mile to contain 157 grains of matter, the Neptunian mass was thirty-four(34) billion times less dense than hydrogen.

The volume of the sphere, radius three (3) billion miles, whence Neptune was detached, the ring being segmental, was $\frac{4}{3}\pi R^3 = 113$ octillion cubic miles. But the ring was in volume nine octillion cubic miles, nearly $\frac{1}{12}$ the entire mass!

What unheard of convulsion took place to disrupt the mass and cause it to part with $\frac{1}{12}$ its bulk! What inconceivable power was displayed if the dogma is true, yet all the force present was gentle centrifugal tendency caused by slow rotations of 3.36 miles per second!

The mass of the first world is $\frac{1}{20000}$ of the solar system, but $\frac{1}{12}$ the volume was required to make it. The volume of a sphere bounded by the orbit of Neptune is $\frac{1}{6}\pi D^3 = 90$* octillion cubic miles, and as it contained 4 nonillion pounds of gas, each cubic mile held 44 pounds,—17,500,000 times less in density than the lightest body on earth, the mass being homogeneous. But it was not since the centre must have been compressed.

The density of the segmental ring was 34 billion times less than hydrogen, therefore, the sphere was old when it cast away its first world, having had time to acquire internal density, greater than peripheral, in the proportion of 17 millions to 34 billions. All along we have been quoting Helmholtz, where he says:—"It required several cubic miles to weigh a single grain,"—not having made calculation, but now we do not see how he arrived at these results, as one cubic mile, by following the principle of centre of gravity, is found to have contained 157 grains.

He probably alluded to the mass when expanded larger; but if extended to half the distance of the stars, a thousand cubic miles might have been required to contain one grain of matter. And we feel that we are traversing solid ground, in basing these deductions on the doctrine of the centre of Gravity. The volume of a cylindric ring to form Neptune must have been the same as the segmental, the density being nearly equal. The diameter of a section of a cylindric ring whose length was equal to that of Neptune's orbit, in order to have the required volume, was 822,000,000 miles. Since the planet coalesced in its centre of gravity, which was its geometrical centre, the material of the ring extended 411,000,000 miles above, and the same distance below the orbit. This added 822,000,000 miles to the equatorial diameter of the mass, retarded its assumed rotation, and prevented detachment of any particle of matter. The disrupting force had to be applied, not where Neptune revolves, but 411,000,000 below, at a point where force was weakest, and resistance strongest. And then such a ring was subjected to lateral pressure, and could not be severed on that account.

The ring made up of an infinite number of solids generated by revolution of circular segments about their chords, to have the same volume as segmental and cylindric, was in radial diameter 380,000,000, and in diameter north and south 2,090,000,000 miles. If this form of ring was discarded the break took place 190,000,000 below the orbit, and along a line more than two billion miles long. Rotation was slower than the orbital velocity of Neptune, and the separation was again required to be made where the force to cause it was in minimum, and its opposing powers, gravity and cohesion, at a maximum. We reassert that in no possible case could the Neptunian matter have been detached from the primeval mass when it was a *sphere*.

Neither could it have been separated when sections of the protuberance were parabolic; thus a chord, or limiting plane, the base of a parabolic segment, in order to cut out a ring in volume sufficient to contain the Neptunian gas, was in length 634,000,000, in altitude 1,250,000,000, the vertex extending 750,000,000 above, and base descending 500,000,000 miles below, the orbit, in order that Neptune might condense in its centre of gravity. This would make the equatorial diameter of the mass 7,060,-000,000 miles a physical impossibility, for rotation would have come to a dead rest ages before such elongation. Consider the curvature of sections semi-elliptical and results are still more absurd. Since the mass was unable to part with rings whose sections were hyperbolic, parabolic or semi-elliptic, we dismiss as untenable all varieties of such ring-shaped masses.

Now conceive the mass a sphere again and at rest; let rotary motion be imparted, and should the velocity become sufficient, the equatorial regions will become a swelling tide. But when a protuberance elevated motion waned, and the equator subsided. When at greatest altitude the whole mass was an ablate spheriod. Therefore, we lay down this proposition which must have obtained if the Hypothesis of ring displacement is true. If the equatorially expanding mass parted with a ring, it did so at the first opportunity.

And such fullness of time was when a segment of an ellipse could be cut away large enough to have Neptune in its centre of gravity. When the mass was an ablate spheroid, sections cut by passing planes through the entire mass, at right angles to the equator, bisecting the poles, would all be plain curves—ellipses. And a segment severed from the equator to make Neptune, was a segment of an ellipse, whose centre of gravity coincided with that planet's present track. The dimensions of this ring were, height, 319,000,000, and chord 2,350,000,000 miles, to have volume sufficient, to contain gas enough to solidify into the most remote member of the solar system. The axes of the ellipse whence this segment was cut were transverse, 5,800,000,000, and conjugate, 5,400,000,000, the diameter of the mass when spherical being 5,560,-000,000 miles. Therefore, we say that during all mutation in form of the primordial cosmical mass, admitting the hypothesis true, its equatorial diameter was never augmented more than 240,000,000 miles, as is seen in these dimensions.

Mathematical instruments of delicacy are required to measure such small deviation from a sphere. Yet, it was able to discard a ring having a volume of nine octillion cubic miles. Basing conclusions on the sure foundation of the principle of the centre of gravity, we assert that the mass never detached rings whose sections were of any form of curvature known to geometry. Then none were cast off, since every department of celestial mechanics is known to be subject to rigid mathematical laws.

The theory of cosmic evolution, which holds that planets were formed of masses detached from an aeriform sphere belongs in that list of delusions which retarded the early progress of astronomy—the "Geocentric System," the "Firmament," and "Music of the Spheres."

NEW WINDSOR, ILL., *July* 10, 1881.

* Ninety Octillion.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE":—

Dr. J. J. Mason in his second rejoinder to the criticism made by a reviewer in SCIENCE, leaves his unknown critic to the contemplation of the latter's clause. "Notwithstanding the construction which Dr. J. J. Mason now desires to see placed on his words," and does him "the justice" of supposing that the critic

knows what he insinuated. This the critic did indeed know! In view of the inconsistency between the text of his article, and the *dementi* of his first rejoinder, he considered himself justified in insinuating that Dr. Mason had come to recognize that one of his views was untenable, and preferred to deny having entertained that view to admitting its errors publicly. If I have misinterpreted Dr. Mason's paper, I fail to see it even now, for the doctor fails to answer my question: What other than the size of the cells and their nuclei, does Dr. Mason refer to when he speaks of a structure universally admitted to be motor?" Until he answers this, I would suggest that further correspondence on this head is a waste of the space in your periodical, and that demands for "customary regrets" are premature. I am as willing now as I have been throughout not only to withdraw my original stricture, but also the statements that have grown out of the controversy, if Dr. Mason can explain this passage and those with which it is associated, differently from my interpretation, and the meaning evident on their face.

Such an explanation should, however, avoid the incongruity existing between the text of Dr. Mason's article and the explanation he now gives of his real intention in polemicizing against Stieda, which I must confess I have not been able to assimilate. Dr. Mason might also answer this question. Why has he, if his "three brief articles" relate throughout to reptiles, and Batrachians referred to the bat as bearing out his theory, and why has he incorporated an explanation as his own, which I published two years before, without even mentioning my name, or that of some one else who may have anticipated me? My publication was certainly known to Dr. Mason, and he cannot fall back upon the flimsy excuse that it was a "preliminary" communication, and had nothing to do with his subject. If the explanation was worth while incorporating in Dr. Mason's paper, it was worth while giving its author credit for it, just as it was worth while referring to the author of the Iguana article by name, if it was worth Dr. Mason's while to offer suggestions in a patronizing way, which were altogether unnecessary as a matter of instruction, and as which they seem intended to appear.

I consider this subject closed, as far as I am concerned, until such time as the main question here repeated, is properly answered. E. C. SPITZKA.

NEW YORK, 130 E. 50th Street, July 19, 1881.

NOTES.

The *Chemiker Zeitung* states that all the English and French professors at the University of Yeddo, Japan, have been dismissed, and their places filled with Germans. The Japanese Minister of Public Instruction is a German professor. The Chinese are about establishing a German University at Pekin. These facts should be duly weighed by those who still doubt the superiority of German research over English cram and examinations!

ACCORDING to M. A. Gaudry the Permian reptiles of France diminish the vast interval which exists at present between the reptiles and the monotrematous mammals.

THE ferment which M. Béchamp supposed he had discovered in chalk has been traced, by MM. Chamberland and Roux, to an experimental error.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING JULY 16, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height from ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

JULY.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 10	30.044	30.094	9 a. m.	29.982	12 p. m.	72.6	68.6	82	4 p. m.	74	4 p. m.	66	4 a. m.	65	4 a. m.	134.
Monday, 11	29.943	29.996	12 p. m.	29.900	2 p. m.	72.0	69.3	80	11 a. m.	74	11 a. m.	65	12 p. m.	63	12 p. m.	129.
Tuesday, 12	29.956	30.000	9 a. m.	29.900	12 p. m.	69.7	65.7	76	4 p. m.	70	5 p. m.	63	4 a. m.	62	5 a. m.	131.
Wednesday, 13	29.805	29.900	0 a. m.	29.744	6 p. m	79.6	73.6	90	4 p. m.	80	4 p. m.	68	3 a. m.	66	3 a. m.	135.
Thursday, 14	29.889	29.976	12 p. m.	29.794	0 a. m.	80.0	71.3	86	3 p. m.	74	2 p. m.	75	6 a. m.	69	6 a. m.	143.
Friday, 15	30.002	30.022	9 a. m.	29.976	0 a. m.	76.3	69.7	82	3 p. m.	72	1 p. m.	70	12 p. m.	67	6 p. m.	138.
Saturday, 16	29.801	29.984	0 a. m.	29.618	12 p. m	78.6	71.6	83	3 p. m.	73	3 p. m.	69	5 a. m.	67	5 a. m.	134.

Mean for the week .... 29.920 inches.
Maximum for the week at 9 am., July 10th .... 30.094 "
Minimum " at 12 pm., " 16th .... 29.618 "
Range .... .476 "

Dry. Wet.
Mean for the week .... 75.5 degrees .... 69.9 degrees.
Maximum for the week, at 4 pm. 13th 90. " at 4 pm 13th, 80. "
Minimum " " 4 am. 12th 63. " at 5 am 12th, 62. "
Range " " .... 27. " .... 18. "

JULY.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of Rain and Snow in Inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE.
Sunday, 10	s.	s.	s.	98	¾	3.30 pm	.577	.704	.668	84	73	85	10	2 cir. cu.	3 cir.	-----	-----	-----	--	0
Monday, 11	s. w.	s.	e. n. e.	106	11¾	11.30 am	.668	.799	.591	85	86	89	10	10	10	0.15 pm	2.00 pm	1.45	.09	2
Tuesday, 12	e. n. e.	e.	s. s. e.	113	¼	5.30 pm	.516	.604	.622	83	72	85	9 cu.	5 cu.	5 cu.	10 pm	11.30 "	1.30	.02	0
Wednesday, 13	s. s. w.	w. s. w.	w. s. w.	137	2¾	2.00 pm	.644	.836	.787	85	65	74	10	0	3 cu.	4.00 pm	5.00 pm	-----	--	4
Thursday, 14	n. w.	n. n. w.	n. w.	122	1½	9.30 am	.601	.691	.664	65	57	69	0	3 cir. cu.	8 cu.	-----	-----	1.00	.01	1
Friday, 15	e. n. e.	e. s. e.	s. s. e.	130	3¼	10.00 am	.568	.677	.666	68	66	77	6 cir. cu	9 cir. s.	0	-----	-----	-----	--	0
Saturday, 16	s. w.	s. w.	w. s. w.	135	3¼	4.40 pm	.655	.677	.717	81	60	70	1 cir.	6 cir. cu.	0	-----	-----	-----	--	7

Distance traveled during the week .... 841 miles.
Maximum force .... 11¾ lbs.

Total amount of water for the week .... .12 inch.
Duration of rain .... 4 hours 15 minutes.

DANIEL DRAPER, Ph. D.
Director Meteorological Observatory of the Department of Public Parks, New York.

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SATURDAY, JULY 30, 1881.

There appears to be an open question between Professor Ormond Stone, of Cincinnati, and Mr. Rock, of Washington, as to whether the nucleus of Comet b, 1881, divided on the night of the 6th instant.

Both astronomers appear to have observed the comet at the same time, but have recorded somewhat different results.

On reference to "Science," July 16th, page 334, will be found a statement of what Mr. Rock saw, as follows:

On the 6th of July the comet was observed by Mr. Rock of the Naval Observatory:

"A bright tongue of light about one revolution long in direction of tail, with a slight node near end and curved."

In explanation of this Mr. Rock said: "I observed the comet at the time of its lower culmination about twenty minutes after midnight. The nucleus did not appear to be divided, but a bright band streamed out in the direction of the tail. This band was about fifteen seconds of the arc in length. Near the end of it was a bright spot, and that portion of the band extending beyond it was curved in the same general direction as the tail, but in a somewhat shorter arc."

And then referring to Professor Stone's report of a division of the nucleus, he adds:

"It is possible that the observer at Cincinnati was not able to distinguish the band of light which I saw uniting the nucleus and the node, and so concluded that he saw two nuclei. When I first observed the comet, on June 28, the coma was apparently homogeneous as it also was on July 2. On June 28, however, there were two spurs of light spreading away from the opposite sides of the head like angel's wings. On July 2, I did not observe these at all or they were very faint. On July 6, I observed the appearance that I have described. It may be that this was the same thing that I saw on June 28, observed from a different point of view. It is not improbable, however, that the nucleus has really divided. Comets appear to have a tendency to do that."

In another part of this issue will be found a letter from Professor Stone, reiterating his former claim of having observed a division of the nucleus of this comet on the night of the 6th instant.

"He states that on the 6th of July, during observations made between 10 p. m. and 3 a. m., he saw a bright red jet projected from the nucleus into the dark region on the side of the nucleus opposite the fan, which was totally different in appearance from those usually seen. There was a dark line separating it from the nucleus. During the first few minutes a decided change took place. The jet *seemed to separate* and form a nucleus of its own, so that for a time the comet appeared double."

It may assist in a solution of this subject if our readers inspect the continuation of the interesting drawings of this comet, made by Professor Edward S. Holden, to be found on another page of this issue.*

The drawings we published last week showed the appearance of the comet on 24th, 25th, 26th, 27th, 28th and 29th of June, and the nights of the 8th and 11th of July. Those presented in this number give views of the comet for the nights of the 13th, 14th, 17th and 18th of July.

The drawing for the 11th of July is interesting as showing "a dark narrow channel between the following side of the nucleus, and the envelopes," but, added Professor Holden, "the nucleus is not double." But the drawing we offer this week for the 18th instant, is quite remarkable as showing a decided division of the nucleus, and Professor Holden remarks in his note to it, "The nucleus is double (it has not been previously)," and those who inspect this drawing will find two nuclei.

The drawings of Professor Holden and the observations of all who have watched this comet, show conclusively that the form of the nucleus changed very rapidly and continuously, and as we have the best evidence that the nucleus divided on the 18th instant, it makes it very probable that a similar phenomenon occurred on the 6th of the same month, especially as Professor Stone is an accomplished observer, and not likely to be mistaken in his description of the optical appearance of a celestial object.

An experiment illustrating "fatigue" in the sense of hearing (corresponding to fatigue of the retina) has been described by Herr Urbantschitsch. Two tubes are adapted to the ears, and adjusted, so that a given tuning-fork is heard equally on both sides. Now strike the fork strongly, and let it sound a little through one tube; then deaden it somewhat by touching. The ear on that side fails to catch the weakened sound, but on transferring the fork to the other ear, the sound is heard distinctly. If the weaker tone presented be of different pitch from the stronger, it is heard on both sides equally. The failure of sensitiveness in the other case is very transient.

* On account of delay in engraving these drawings, they are reserved until next week's issue.

LATENT SOLAR LIGHT.*

Translated from the French, by the Marchioness CLARA LANZA.

A remarkable stone, which plays quite an important role in ancient history, is the carbuncle, literally translated, glowing coal, which shines and glimmers in the dark. Lucien relates that in the Temple of Hieropolis there is the statue of a Syrian goddess in whose forehead is placed a stone called *lychins* or lamp. This stone was moderately brilliant during the day, but at night it illuminated the temple from one end to the other. Shakespeare, in Titus Andronicus, says, while speaking of Prince Bassianus' body:

> "Upon his bloody finger, he doth wear
> A precious ring, that lightens all the hole,
> Which, like a taper in some monument,
> Doth shine upon the dead man's earthy cheeks."

It is said that formerly dwarfs and gnomes wore one of these stones upon their heads as miners carried their lamps. We have likewise been told that certain birds knew where to find them and make use of them to illumine their nests. The tendency which has been remarked in birds, notably crows, to pick up brilliant objects, has naturally given rise to numerous legends and anecdotes among all people, and it is declared that in America numbers of birds light up their nests by placing therein fire-flies. The carbuncle has still another secret property, for it renders the object it adorns, invisible both to man and beast. The question may therefore properly arise, how did man happen to discover this treasure which birds alone were apparently able to distinguish? Poetic fancy, we may say, has answered this query. The invisibility is caused by a ray of light which blinds the eye. A mirror, however, does not become so easily dazzled, and if, while walking along the edge of a brook, you perceive the reflection of a nest in the water, while with your naked eye you are unable to discover it, you may be sure that the stone is there. The legend of the carbuncle first arose in India, the land of precious stones, and it was founded upon the remarkable capacity possessed by many diamonds and a few rubies of shining for a long time in the dark after being exposed a few moments to the sun or merely broad day-light. This phenomenon appears to have been studied and experimented upon for the first time in Europe somewhere about the seventeenth century, by the celebrated naturalist, Boyle. In India, however, the knowledge of it can be traced back to the furthest antiquity, as can be proved by referring to a passage in the famous drama called *Sakuntala*, whose author certainly lived long before the beginning of our era. The passage is this:

"Among the just whose souls enjoy the most complete repose, there is a hidden radiance, which illumines them with its faint glimmer. Thus shines the precious sun stone, as soon as an outward ray of light strikes it."

At Bologna, which, as we know, is a well-known scientific centre, there lived at the beginning of the seventeenth century, a shoemaker named Vincenzo Cascariolo, who like many other men of his time, determined to discover primitive matter in the shape of the philosopher's stone, and by means of it,change the vilest and most worthless metals into pure gold. He had already experimented with fire and water upon all possible substances, organic and inorganic, when in 1604, some writers say 1612, he found, one day upon Mount Paderno, close to his own dwelling, a grayish-white stone, of a fibrous structure, and whose weight being considerable, made him suspect some unusual property. He calcinated a portion of it with some coal, and night falling while he was engaged in the operation, he saw with utter stupefaction, that the entire contents of his crucible, shone with a ruddy glow, although the furnace had become quite cold. With trembling hands he seized the stone, not doubting in the least that it was the famous philosophical one, of which he had so long been in search—still less did he doubt, when he observed afterwards, that only the fragments which were exposed to the sun or broad daylight were brilliant. Alchemists in those days called the sun a golden planet. In their works they employed an identical sign to designate both the luminary and the metal, and they firmly believed that the rays of the former penetrated the latter, as water is soaked into a sponge. This mysterious connection is clearly indicated in a brief opuscule discovered during the middle ages, no one knows exactly where, and of which there exists now only a Latin translation, the original, however, doubtless having been found in some Egyptian tomb. It is called "The Emerald Table of Hermes Trismegistus," and among other things it is therein stated that "the father of the Philosopher's Stone, is the sun, its mother the moon. Separate the earth from fire, and you will obtain the wonder of the world, all shadows will flee before you." These obscure words were applied to the new luminous body called phosphorus, and the phosphorescent stone of Bologna, excited the young disciples of chemistry, to the highest pitch of interest.

Although this substance did not at once realize the great expectations set abroad concerning it, and notwithstanding the fact that it was obliged to renounce entirely the *rôle* of philosopher's stone, it nevertheless caused its discoverer to make a considerable sum of money, for men seeking knowledge and instruction came from all countries to Bologna, and purchased this natural curiosity to a great extent. Poets likewise wrote laudatory Latin verses to the now celebrated shoemaker, comparing him to Prometheus who stole fire from heaven, and placed it on the earth. Enormous enthusiasm was manifested everywhere for this remarkable stone. Volumes were written about it, and it was even stated that the sun and moon were nothing more than huge masses of Bologna phosphorus. For a long time it was thought that the stone existed nowhere but at Bologna, but later it was discovered that it was composed principally of spar or sulphate of baryta, which was to be found in numerous places.

Alchemists gathered fresh hope in 1674, when Christian Balduinus, intendant at Grosenhain in Saxony obtained an analogous luminous body by the calcination of nitrate of lime. He called it hermetic phosphorus or solar gold, and in several works he declared that this was indeed the veritable philosopher's stone whose properties he was engaged in studying. The only German Naturalistic Society at that period was the "Leopold Academy of Natural Curiosities," and this organization received the new inventor into their midst under the honored title of Hermes, which has ever remained in the chemical world. Since then, it has always been supposed that the hermetic or philosophical stone must be luminous, and Dickinson, physician to Charles II, of England, relates in his "Old Physical Truths" (1702) that Noah, whom he regarded as one of the ancestors of hermetic science, had placed a large gleaming stone of some sort, called *zohar* in Hebrew, upon the top of his ark, so that he might have perpetual light during the night, and that moreover the scientific knowledge of this same Noah had caused him to nourish every animal in the ark with an extract made from the meat or plant which the creature preferred, thus economizing space and doing away with the necessity of removing from the ark such bones, leaves, skins, etc., which might otherwise have been there.

Chemical researches advanced with singular activity; for about the same time that Balduinus was performing his experiments, Brand, of Hamburg, an obstinate investigator discovered a substance which produced luminous vapor, and condensed itself into yellow drops that shone in the dark without being exposed to the sun. Professor Kirchmayer, of Wurtemburg, announced to

* This article was originally written in German and published a short time ago in the *Gartenlaube*.

the world emphatically, that the long sought for "perpetual light" had at last been found, while another enthusiastic novice wrote a work upon the *Phosphorus mirabilis* and its marvellous brilliancy. Here again the future did not justify all the hopes which might have been expected. This substance, however, which still goes by the name of phosphorus has become one of the necessities of our age.

Phosphorus gradually entered the scientific period. In 1768 an English chemist, Canton, obtained a new kind by calcinating oyster shells with sulphur, and it was finally discovered that the best absorbents of light were combinations of sulphur, calcium, baryum and strontium. However, other metallic sulphurets and various substances are equally capable of making in the dark what is called solar, magnetic or electric light. The method of preparation, of course, has considerable influence and lights of divers colors can be obtained according to the process employed. By calcinating sulphates with organic substances, or carbonates with sulphur, a very brilliant phosphorus can be obtained, consisting principally of baryta, another of lime, less luminous and a third of strontium, which gives forth a very feeble light. Sulphate of baryta gives a phosphorescent product of an orange color. When the sulphate is prepared artificially the light is greenish.

Later, Ozarm obtained other luminous bodies by calcinating lime with sulphate of arsenic or sulphate of antimony, while another chemist, Bach, by heating sulphur with calcinated oyster shells which had probably been washed with a solution of ammoniac and realgar, procured a phosphorus so brilliant that its light was even visible during the day.

It is by this means, or others which are similar, that the luminous flowers are prepared which lately have appeared to such an extent. They are covered with some phosphorescent substance which makes them glimmer in the dark with a beautiful bluish light. The luminous matter is pulverized and applied to the object by means of a varnish or anything else that will stick. By employing phosphorus of different colors very pretty effects can be produced, bouquets of all shades, glittering butterfles, luminous inscriptions, etc. But the most interesting of all is undoubtedly luminous photography, which is made by placing a paper covered with phosphorescent powder behind the glass negative of a photograph. Heat brings out the luminous qualities as well as light, and very peculiar and beautiful effects can be obtained by writing upon such a paper as has just been described with a pointed piece of heated metal.

Unfortunately these interesting amusements are not eligible as regards trade, for as soon as it is exposed to the air sulphuric luminous matter gradually loses its properties and acquires the disagreeable odor of spoiled eggs, while the object by the end of a week or two is not phosphorescent at all. On the other hand, it can be very well preserved by putting it into air-tight glass tubes, and phosphorus of all colors thus prepared can be had from Geissler's establishment in Bonn. It has been proposed to make inscriptions of these tubes for the night bells of hotels, physicians' houses and druggists' shops, the daylight being sufficient to make them very luminous at night. Another idea, conceived by Gustave Ullig, is to make the faces of watches and clocks phosphorescent, as the glass covering them would be a protec ion against destruction.

As to the physical explanation of phosphorescence, it was thought for a long time that the light was composed of little eddies or whirlpools of subtile matter, and that sunlight became condensed and accumulated in them. Later, when it was known that light is only a vibratory movement, and that the phosphorus on the end of matches only burns because it is united with the oxide in the air, it was thought that in all the old phosphorescent substances the light was produced alone under the influence of a slight oxidation. This explanation, however, is false, and only during the last century was the true one made known by a celebrated German physician named Euler.

It is generally believed that the planets, the tops of mountains, and all celestial bodies, are visible, simply because they reflect the rays of the sun. This is also false. Brilliant surfaces alone, more or less, reflect light, others absorb it on the contrary, and cause vibration just as a musical sound makes all the objects which it strikes vibrate. Certain surfaces, however, can only reproduce certain vibrations (blue or red for example) of solar light, which is composed of the vibrations of the seven prismatic colors, and when these vibrations are repeated in our eye, the surfaces appear to us blue or red, as the case may be.

In the same way that consecutive vibrations can be determined after sound, so phosphorescence succeeds the action of light. Euler affirmed that the greater number of bodies would present these luminous vibrations if they were observed immediately after they had been exposed to the sun, and if a continued sitting in the dark had rendered the eyes of the observer very sensible. The French physician, Becquerel, constructed an instrument about twenty years ago called the phosphoroscope, by means of which he demonstrated that most substances, paper, stone, oyster shells, etc., shone for a short time after being exposed to the light, that is to say, a second or the fraction of a second, and that solar phosphorus was only distinguishable from other bodies by the persistence of this property. But whether this assertion be true, generally speaking, or not, the subject itself is not by any means simple, and there are a mass of circumstances of which we must take account.

Modern physics teach us that a number of bodies, notably colored organic matter and some metallic combinations, become phosphorescent, but only when they are lighted. This sounds like a paradox, but facts can prove the assertion. There are certain substances, both liquid and solid, which by reflected light appear to have another color than the one transmitted. A peculiar emission of rays can also be observed upon the surface. Petroleum, solutions of sulphate of quinine, decoctions of Indian bark, etc., emit bluish rays; the etherized extract of green leaves, blood red rays; uranium glass, which is pale green and used principally in the manufacture of Rhine wine glasses, emits reddish yellow rays. If any of these dichroic subtances are selected and placed in a dark room lighted only by an electric current traversing a glass tube, they will shine brilliantly, each one in its particular color, certainly with more splendor than the electric light, and yet only while the latter illumines them. How is this curious phenomenon to be explained? How can a feeble light produce such a brilliant one?

It has been said above that white light is composed of seven colors, or, more properly speaking, of an infinite number of colors, which after their dispersion from the prism separate one from the other and form a long band. The red rays are those which vibrate the slowest, and the violets those which vibrate the most rapidly. But just as there are in addition to the red rays others which vibrate slower still and are manifested not as luminous rays, but calorific ones, so there are besides the violets, ultra violet rays which vibrate so quickly that we cannot directly perceive them, although they are known by their energetic chemical action. This is notably the case in photography, and for this reason they are termed chemical rays, or invisible light. A pale, electric light, is a peculiarity of these rays, and the latter give to certain bodies that remarkable dichroic radiance which has been called fluorescence, because it was observed for the first time in fluor-spath. However, if on one side these rays produce a light which cannot be perceived by our retina owing to the extreme rapidity of their vibrations, on the other, the bodies thus illuminated should be able to

diminish the rapidity by vibrating themselves more slowly, and thus render the rays visible. Ultra violet rays could consequently be transformed into violet, blue or green; blue rays into yellow or red. What generally happens, however, is that they change red rays to purely calorific ones and thus make them invisible.

We must here make several important observations. First of all, violet rays do not only produce the greatest fluorescence, but also the greatest phosphorescence. Red rays produce neither the one nor the other. Luminous or dichroic substances give a light differing from that which they receive. It has been demonstrated, finally, that the closest relationship exists between the two phenomena—That fluorescence can be considered as an intense phosphorescence which can be seen in broad daylight, but which dies with the light which gave it birth, while phosphorescence is only a feeble but persistent fluorescence.

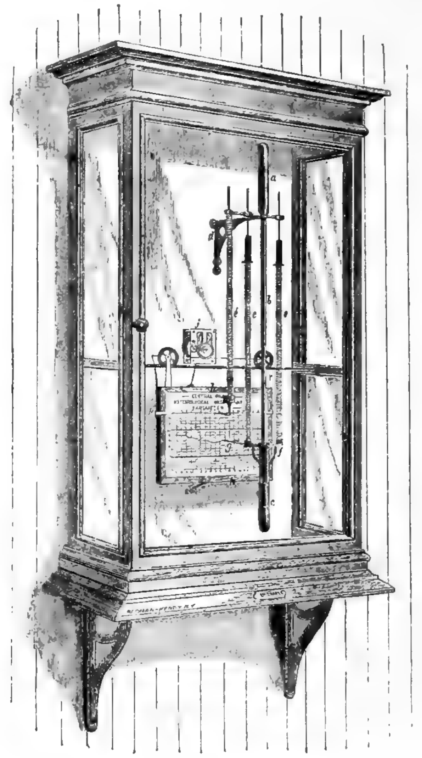
"Solar phosphorus" generally reproduces luminous vibrations even when it has ceased to receive the latter, and it can transform calorific rays into luminous ones. A diamond acts in this way, also fluor-spath, and nearly all artificial phosphorus. One of the last named gives forth a light of various colors, if it is heated to different degrees after being exposed to the light. Sulphate of strontium produces a deep purple light at 20°, a violet light at 15°, blue at 40°, bluish-green at 70°, greenish-yellow at 100°, and reddish-yellow at 200°,

Moreover, phosphorescence, like fluorescence, can be produced by means of an electric light rich in chemical rays. If you expose to such a light a flower, a butterfly or any other object covered with phosphorescent powder, it will assume a magnificent appearance. The English chemist, Crookes, prepared diamonds and rubies in this way, by enclosing them in an air-tight glass ball placed in the immediate vicinity of the negative pole, from which a luminous current issued. The effect was superb, recalling all sorts of fairy stories. Some African diamonds shone with a brilliant blue light, and a large greenish one produced such an intense radiance that it almost looked like a lighted candle. In fact the light was quite sufficient to read by, and the history of that famous stone in the Temple of Hieropolis seemed really probable. A collection of small diamonds from various countries, placed in any receptacle that is air-tight, will produce parti-colored fiery lights, blue, pink, red, orange, yellow, green and pale green, all mingling together.

In a third recipient, Crookes placed a quantity of uncut rubies, which, when the electric light fell upon them shone with such a gorgeous red flame that they appeared to be incandescent. Artificial rubies prepared by Feil in Paris gave as brilliant a light as the real ones, and white crystals became rose-colored or deep red. Such wonderful carbuncles would have astonished even the authors of the old legends.

A CURIOUS thing occurred lately in the works of M. Fleury, at Cette (Hérault). The feed-water of the boiler giving much incrustation, M. Fleury was advised to put into the boiler some fragments of zinc as a de-incrustant, and did so. In a few days, spite of oiling, the steam-engine began to work very badly, the piston catching a great deal, and it soon became necessary to stop and make examination. The piston was found to be covered with a thick adherent layer of copper. It was put on the lathe, and at certain ovalised points, the metallic layers were so thick that the tool worked in copper alone. The explanation given by M. Fleury is this: The boiler was connected with the engine by copper pipes. Particles of zinc carried off by the steam would form with the copper numberless small galvanic couples; hence the transport of copper to the piston, which would principally attract them by reason of its motion, and of the heating produced. It is remarked in *Les Mondes*, that the eminently electric properties of expanding steam may have helped in development of the phenomenon.

DRAPER'S SELF-RECORDING, MERCURIAL BAROMETER.



We are indebted to Dr. Daniel Draper for preparing an abstract of his weekly Meteorological report for this journal, the third of which appears this day in another column.

Dr. D. Draper is director of the Meteorological Observatory of the Department of Public Works, Central Park, where all observations are made by self-recording instruments, especially designed and arranged for this purpose.

The great object Dr. Draper had in view when designing these instruments, was to combine simplicity of construction with perfect efficiency. His great success is well known to all familiar with Meteorological Science, and we propose in the course of a few articles to fully describe these instruments, and illustrate the subject with excellent wood cuts.

We commence the series with a description of the apparatus for recording Barometric observations.

"I was led to construct this form of barometer from the fact that with the photographic one it cannot be told what the atmospheric fluctuations are until the next morning, when the photographic plate is developed. Even then, if there has been much variation in temperature, it alters the sensitiveness of the collodion film, so that it is very difficult to read the tracing. The construction of the pencil instrument is as follows:

In the pencil barometer the glass tube is 36 inches in length, the upper portion being of larger diameter than the lower; it is held firmly in a fixed position, and filled in the usual manner with quicksilver; its lower or open end dips into a tube or reservoir containing the same metal. This reservoir is suspended on two spiral steel springs, and has freedom of motion up and down. When the pressure of the atmosphere diminishes, a portion of the mercury flows out of the tube into the reservoir; this becoming heavier, stretches the steel springs, causing the ink pencil fastened to them to mark downwards. If the pressure increases the reverse movement takes place. The ink pencil makes its mark on a ruled paper register,

carried at the rate of half an inch per hour from right to left by a clock.

There is a third steel spring of the same length and strength as those on the reservoir, stretched by a weight to a distance equivalent to 30 inches on the barometer scale. The object of this spring is to give the correction of temperature for those sustaining the reservoir. The register paper should always be set to the same line on which the pencil of this spring marks.

The movements of the mercury on the register can be magnified to any required extent by increasing the length of the spiral springs. In this instrument it is multiplying twice.

DESCRIPTION OF INSTRUMENT.

The tube marked *a b* is of glass; the upper part is of a larger diameter than the stem, *a* being ¾ of an inch internal diameter and 10 inches long, while the stem, *b*, is ⅛ of an inch bore and 26 inches long. The total length of the tube is therefore 36 inches. The reservoir, *c*, is suspended from a brass frame, *d*, fastened to the back of the case. This frame also holds the upper ends of the steel springs, *e*, *e*, *é*. The glass reservoir, *c*, is of the same diameter and length as the upper part of the tube, *a*; on its open end is turned a flange to hold it in a brass frame, *f*, to which are fastened the lower ends of the steel springs, *e e*; it also carries an ink pencil, *g*, that touches the ruled paper on the board *h h*, which is drawn aside by the clock, *i*. The spring, *é*, is for the correction of temperature on the other springs. Heat has a slight effect on them, causing them to lengthen about $\frac{1}{16}$ of an inch from 90 degrees Fahr.; to allow for this, the third spring, *é*, is weighted with a lead weight and pencil, it marks its fluctuations on the upper line of the register sheet. In this manner this instrument gives the correction for temperature (or reduction to 32°) from the fact that it weighs the mercury instead of measuring its length, which is affected by heat.

Ink pencils of the barometer and other instruments are made by drawing narrow glass tubing to a fine point, which lightly touches the paper register, leaving a mark of red ink that has been diluted with about one quarter of its volume of glycerine. The glycerine prevents the ink from drying too rapidly. The advantage of this form of pencil over lead ones is that it requires little or no pressure to produce a mark.

To receive the atmospheric fluctuations a suitable ruled paper is fastened by means of small brass clamps, *k k*, to the board, *h h*, which is hung by rollers to the thick steel rod fastened to the sides of the case, on which the paper is carried from right to left by the clock, *i*, at the rate of ½ an inch per hour, by means of the pulley on the hour arbor of the clock. The wire that connects the register board to the clock is soft steel, number 28 wire gauge; having only one turn round the pulley it readily slips so that the board can be pushed sideways for the adjustment of time, or for the renewal of the sheet of paper."

ON AN OCCURRENCE OF GOLD IN MAINE.*

By M. E. Wadsworth.

The gold under consideration here is found on Seward's Island, a small island in the town of Sullivan, Hancock County. The gold is found in quartz veins cutting an eruptive mass of diabase. This diabase forms a dike of about forty feet in thickness, lying approximately parallel to the bedding of an indurated fine-grained argillaceous mica schist; all dipping nearly S. 30° W., 24° to 42°. The dip averages about 35°, and the strike is far from being uniform. Crossing the diabase at various angles, but generally from north to south, are segregated quartz veins. In some places the rock is a confused reticulated mass of these veins, with patches of diabase lying between them. The veins vary in width from a mere seam to even a foot in breadth. Starting where only one or a few of them are visible, they gradually increase in number, until they become quite numerous, while they will doubtless be found to fade away as they began. The diabase and schists are cut by several dikes of diabase running approximately at right angles to the strike of the schist, or parallel to the veins. The vein stone is quartz, together with some calcite, tremolite and chlorite, and carries tetradymite and gold.

So far as examination has been made, the veins in the diabase carry gold, and the decomposed diabase immediately adjacent to the quartz veins also contains that metal to a greater or less extent. The gold occurs principally in small grains in the vein in conection with the tetradymite, bits of decomposed diabase, and in the cavernous portions, but not in the compact quartz of the vein itself. The tetradymite is in irregular grains and masses, showing a brilliant metallic lustre, and a well-marked basal cleavage. The locality is worked for it gold, and was visited by the writer in December last

Cambridge, Mass.

*From the *Bulletin of the Museum of Comparative Zoology*.—Harvard College.

ELEMENTS AND EPHEMERIS OF COMET (*c*), 1881.—SCHÆBERLE.

The elements and ephemeris of the comet, given below, are those computed at the observatory of Lord Crawford, at Dun Echt, Scotland, and cabled to the *Science Observer* by means of the code adapted by S. C. Chandler, Jr., and John Ritchie, Jr.

ELEMENTS.

Perihelion Passage, 1881, Aug. 21ᵈ .50. Greenwich Mean Time.

	°	′	
Long. Perihelion	= 218	45	Eq. 1881. o.
Dist. Perihelion from Node	= 121	9	
Long. Node	= 97	36	
Inclination	= 140	37	

log. Perihelion Distance, 9.8069.

EPHEMERIS.

Greenwich midnight 1881.	R.A. h.	m.	s.	Decl. °	′
Aug. 3	6	43	4	+ 47	46
7	7	11	24	50	11
11	7	54	56	52	20
15	8	59	24	52	57

Computed by Drs. Copeland and Lohse, at Dun Echt Observatory, from observations at Vienna and Dun Echt.

The following elements have kindly been furnished by Prof. Ormond Stone, of Cincinnati:—

$T =$ August 19.202.

	°	′	″
$\omega =$	122	30	21
$\Omega =$	98	42	41
$i =$	141	35	2

log. $q = 9.79590$.

Science Observer Special Circular No. 16

The following simple electrical experiment is described in *L'Electricien*. A small box of pasteboard is closed with a lid of fine glass, on the upper surface of which collodion is applied several times (but not so much as to render the lid opaque). In the box are placed insect forms, made of sponge or cotton. On rubbing the collodion surface with dry fingers, in dry weather, the insects move about in a curious manner.

PHOTO-MICROGRAPHY.

By Dr. Carl Seiler.

All workers in microscopy, doubtless, appreciate the necessity of correctly recording, not only in writing, but also by means of pictures or drawings, many of the appearances seen in the field of the microscope. We can do this by drawing an outline of the objects observed, by the aid of the *camera lucida;* but not only does this require some practice, but also a considerable amount of time, and even then the resulting picture will not be a correct representation of the field of the microscope, because it will always be tinged more or less by the imagination of the draughtsman, and will be more or less diagramatical in consequence.

With photography, on the other hand, an exact reproduction of the image thrown upon the screen can be obtained, and in much less time than it takes to make even a comparatively simple drawing with the *camera lucida*. It is the object of this chapter to give an idea of the means employed to obtain a photographic picture of a microscopic object—means which are in the hands of every microscopist, and which do not require a great outlay of money.

A room with a southern exposure, which can be darkened; a mirror, movable in all directions, outside of the window; an achromatic combination of lenses of from eight to ten inches focal length; a microscope which can be tilted so as to be horizontal, and a stand to hold the screen and sensitive plate, are all the apparatus absolutely necessary besides the chemicals used in ordinary photography.

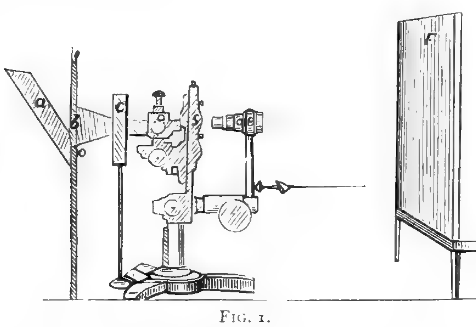


Fig. 1.

These different pieces are disposed of as follows (Fig. 1): The mirror (*a*), which should be eight or ten inches long by about four inches wide, is attached to a board which fist into an opening in the dark shutter of the southern window, and is to be moved by rods from the inside. Instead of this mirror, or in conjunction with it a heliostat is of great advantage to throw the light of the sun constantly in one direction, for, if once adjusted, it need not be disturbed, and thus a great deal of time is saved. Until recently such an instrument was too costly for the use of students, but of late Mr. Kuebel, of Washington, D. C., has put a heliostat in the market which works very satisfactorily and which is sufficiently low in price to be within the reach of many who desire to work in photo-micrography. The board in the shutter has in its centre a circular opening containing an achromatic combination of lenses (*b*), such as the back combination of a one-fourth portrait photographic lens.

The microscope is secured on the window-sill in a horizontal position, so that the axis of the tube is in a line with the axis of the achromatic combination, and at such a distance from it that the burning focus is about half an inch from the back combination of the sub-stage condenser (*d*). The eye piece is then removed from the microscope, and the tube lined with black velvet, to prevent internal reflection, as far as possible, and the whole apparatus is covered with dark cloth, to prevent stray rays of light entering the darkened room. This done, the sun's rays are reflected from the mirror outside the window, through the achromatic combination, which acts as a concentrator and throws a powerful light through the condenser, through the object on the stage (*s*), and thus a brightly illuminated image is formed by the objective (*o*) on the screen, which latter, when the negative is to be taken, is replaced by the sensitive plate.

This image, when thus formed, must be focused with the greatest care and accuracy, in order to obtain a sharp negative; and as the screen must be at some distance from the microscope in order to obtain the necessary magnification of the object, it is necessary to have some contrivance for turning the fine adjustment at a distance. For this purpose it will be found that a small pulley, placed alongside of the microscope, having an endless band running over it and the milled head of the fine adjustment, answers the purpose very well, when the axis of the pulley is connected by means of a universal joint to a fishing-rod, which by its sections can be made longer or shorter, thus bringing its end close to the screen.

The tube of the microscope, even when all internal reflection has been obliterated, still remains a drawback, inasmuch as it reduces the size of the image, or rather the disk of light, the more, the longer it is. There are, however, some stands made in which the tube can be entirely removed, such as the old Ross stand, and they are therefore very desirable for photo-micrographic purposes.

Any good objective of wide angular aperture and good definition can be employed for photography, provided monochromatic light is used in making the negative. When such is the case the visual and chemical foci fall in the same plane and a special correction of the objective for photography becomes unnecessary.

Such a light is obtained by passing the rays of the sun through a cell containing a strong solution of ammonio-sulphate of copper (*c*) before they enter the sub-stage condenser. I have found some difficulty in making the cell containing this solution, as the copper salt will dissolve almost any cement, and if exposed to the action of the air, very rapidly becomes decomposed, and the solution is thereby rendered useless for the purpose. I have used with satisfaction a cell made of a brass ring, lined on its inner side with lead or tin, having a thread cut on its outside, to which flanged rings are secured. Upon the edges of the inner ring a ring of rubber packing is applied, and upon it a disk of plate glass is laid, which is tightly pressed upon the rubber by the flanged ring. Thus a cell is obtained very similar to the round, flat spirit levels, and which will hold the ammonia sulphate of copper solution for months without change. In filling the cell care should be taken to leave room for a small air bubble, for if the cell is completely filled the heat of the sun's rays will expand the solution sufficiently to cause leakage.

This solution, besides giving monochromatic light, at the same time filters out almost all the heat rays from the light, so much so that an immersion lens may be used for any length of time without the drop of water evaporating.

At the present time, when dry plate photography has been developed to such an extent that it has superseded, in a great measure, the wet process, it has been thought that it would be the most simple, economical and satisfactory for photo-micrography; but after repeated trials by myself, as well as many others working in the same direction, it has been found that it is not only more expensive, but also takes more time, in the long run. The reason of this is that it is impossible to judge, with any degree of certainty, as to the actinic power of the light forming the image on the screen by merely looking at it, and that a trial plate only will give an idea of the length of exposure necessary for a given day, time of day

objective, and subject, to be photographed. It is true we can expose a dry plate for trial, but then we must develop it immediately, and the time of developing a dry plate is about three times that of developing a wet one, and a dry plate is also about three times as costly as a wet one. Therefore the old wet collodion process is the best.

The collodion to be used should be an old one, and contain some free iodine. I have found that a mixture of "Anthony's red labeled" and "McCollin's delicate half-tone" collodions—both commercial articles—some five or six months old, gives very satisfactory results. The nitrate bath should contain forty grains of nitrate of silver to the ounce of water, and should be slightly acidulated with nitric acid. The developer should be a weak one: twelve to fifteen grains of the double salt ammonio-sulphate of iron to the ounce of water, containing a few drops of a solution of gelatin and acetic acid as a restrainer.

After the negative has been fixed in the usual way, with hyposulphite of soda or cyanide of potassium, it is almost always necessary to intensify it, which is easily done by flowing the plate while wet with a watery solution of iodine until the film becomes white; then it is to be washed under the tap and flowed with a solution of sulphide of ammonium, which imparts to the negative a dark brown color, and thus strengthens its printing quality.

The object to be photographed should be as thin as possible, because the lens will depict only one plane of it, and it should present as much contrast and differentiation of its elements as possible; this is especially the case in animal tissues, and when high powers are used, the focus should be taken with the greatest care *for one particular point to be brought out;* a general focus not particularly sharp in any one point, will not give a satisfactory negative.

The screen upon which the image is focused should be of plate glass, having an extremely fine ground surface on one side—the side next to the object. Such a surface can easily be prepared by flowing the glass plate with a good negative varnish, and when this is set but not yet dry, lightly breathing on it, when an extremely fine and even frosting of the surface will show itself, sufficient to arrest and reflect the rays of light forming the image.

In photo-micrography, as well as in ordinary microscopy, proper illumination of the object is of the greatest importance, and frequently a poor objective will show a better definition in the hands of a skilled manipulator than the best objective can when the light is not properly managed. In this one point lies the difficulty of photo-micrography, and it is the stumbling block over which so many fall who undertake to photograph microscopic objects.

As a general rule the best light is obtained when the back lens of the sub-stage condenser is about half an inch beyond the burning focus of the larger condenser in the shutter, that is about eight and a half inches from this condenser, and when the light is *absolutely central.* But this distance cannot be strictly adhered to, inasmuch as different objectives require different illumination. In practice, I find that in order to obtain the proper distance of the condenser for a particular objective, it is best to put a blood-slide, upon which the corpuscles are in one layer only, on the stage, and project the image on the screen, moving the condenser backward and forward until, when sharply focused, no concentric rings are seen in the disks. The object to be photographed can then be substituted for the blood-slide, and the light will be found to be all that is desired. (*Compendium of Microscopical Technology.*)

PROFESSOR HELMHOLTZ will issue a collection of his scattered scientific memoirs in the autumn.

PLANTÉ AND FAURE BATTERIES.

The annexed illustrations of the secondary batteries, which are exciting so much interest at the present time will, with the accompanying description, enable the reader to understand their construction. At the recent soirée given by the Council and academical staff of King's College, several forms of electric-lighting apparatus were used; but that which attracted most attention was a battery of forty-four accumulators of Faure's design, working twenty of Swan's lamps. The cells were charged in Paris by a Gramme machine, and were arranged in groups of four in cubical boxes, the whole being coupled up in series. The current supplied by this arrangement, shown by a galvanometer in the circuit while the lamps were alight, was about twenty-three webers, and was perfectly steady—the Faure battery yielding an almost equal current during the whole time, until the charge becomes exhausted, when it breaks down suddenly, without any noticeable warning. Mr. Spottiswoode also uses the Faure battery to work Swan

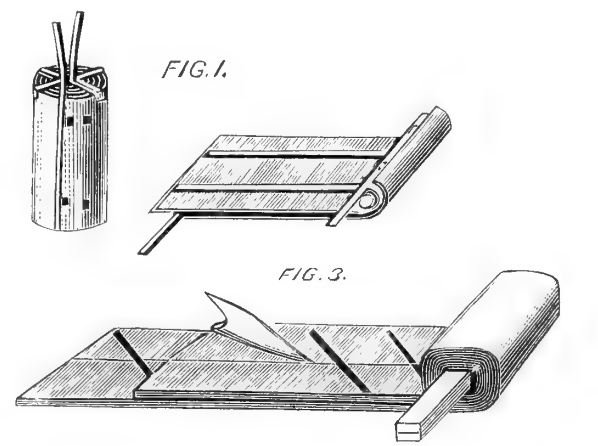
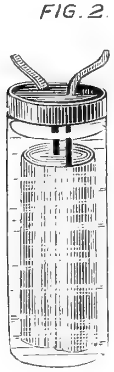


FIG. 1.

and Maxim lamps in his private house. Figs. 1 and 2 represent the Planté cell. The preparation is as follows: Two sheets of lead (it may be as thin as stout lead-foil) are laid the one on the other, separated by two strips of india-rubber, the whole being rolled up as shown in Fig. 1. The roll having been completed, the cylinder used in its formation is withdrawn, and it is consolidated by a wrapper of gutta-percha, and inserted in a glass jar filled with water and 1-10th part acid. An electric current is then made to pass through the cell; oxygen is given off, and produces a thick cushion of peroxide of lead on one sheet; hydrogen is given off at the other sheet. If the current with which the cell has been charged be cut off, and the two sheets are connected, a current will be produced, owing to the presence of the oxygen, which leaves the sheet where it has accumulated and attacks and oxidises the other sheet. This secondary current, which is very small at first, gains strength each time the operation is repeated; in course of time the surfaces of the sheets are changed, the one being covered with a cushion of peroxide of lead, the other with lead reduced to a spongy mass. The cell is then complete, and in a state of electrical accumulation. That was Planté's first successful battery. Subsequently he tried the plan of separating the two sheets of lead by canvas, the cell taking the form of Fig. 2. He then found that it was necessary to leave a small space between the sheets to provide for the escape of the gases which were produced at the end of the charge; subsequently india-rubber bands were employed in preference to canvas. M. Planté also tried carbonate of lead, minium, &c., but without improving upon the results already obtained. The



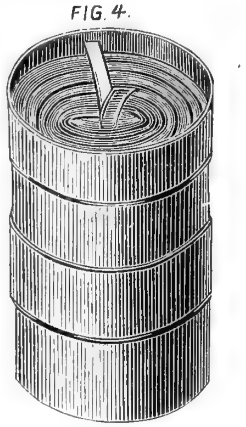


Fig. 2.

Faure battery is similar to the above:—Two sheets of lead are taken, about 7½in. wide; one about 23in. long and about 1-25th of an inch thick, the other 15in. long and 1-48 in. thick. Each of these is furnished with a strong strip of lead at one of its ends. Each sheet has a layer of red lead spread on its surface, the lead being made into a paste with water, the larger sheet having about 800 grammes on its surface, and the smaller 700 grammes. On each surface a sheet of parchment is laid, and the whole is introduced into a sheathing of thick felt. The sheets are laid one above the other; at the same time several bands of india-rubber are placed in an oblique fashion, as shown in Fig. 3. The roll is placed in a leaden jar strengthened by copper bands, and covered in the interior with red lead and felt. The cell then presents the appearance shown in Fig. 4. One of the pieces of lead which jut out is curved and soldered to the outer jar, acidulated water is put in, and the battery is ready for work.

We give the above figures as a guide, but there is no special reason for adhering to them, and it may be doubted whether either the parchment or felt is an absolute necessity; for good batteries have been constructed by painting stout lead-foil with red lead made into paste with water slightly acidified with oil of vitriol, and wrapping the plates in flannel or canvas which has been previously coated with the red lead paint. The painted surfaces are of course put together. Thin lead is used to keep the weight down as much as possible and to reduce the cost.

THE LESSON OF THE COMET; DOES IT SHOW A NEW FORCE?

By Samuel J. Wallace, Washington. D. C.

There is one important consideration in relation to a comet and its tail which does not seem to have been properly noticed. A comet is generally supposed to be a mass, cloud or assembly of masses, particles and possibly gases, which travel together through the heavens, but do not actually form a single cohering body.

Now the remarkable point is this. When this assembly of matter of various sizes and conditions approaches the sun at a great velocity it seems to be acted upon by two forces in opposite directions at the same time, the one driving it forward toward the sun and the other driving it out away from the sun, and apart laterally.

And these two forces seem to act at different rates on different parts of the matter, so as to drive some parts forward, forming the head of the comet; to drive other parts forward with a less force, and spread them apart, forming the brighter part of the tail; while they act to actually drive other parts away into space, as the brush of the tail.

This is an action like that familiar to us in concentrating ores and in separating grain from the chaff. When ores are powdered fine and sifted down a shaft, up which a strong current of air is blown, the heaviest and richest particles fall through the opposing current to the bottom while the lighter and worthless particles are blown up and away. In this manner the rich ore is separated from the poor, and in a like way grain is separated from the chaff. This occurs because there are two forces acting against each other—the wind and gravitation—which act at different rates on the different particles and separate them.

The comet looks as if it was undergoing this very operation of concentration, or separation of the heavy parts from the light parts, under the action of gravity driving inward to the sun or some other opposing force driving outward and apart.

What makes this so remarkable is that the substance of the planets seems to have been separated in this very same manner. If we take the recognized specific gravities of the several planets and set them down in the order of their occurrence from Neptune, the furthest, inward to Mercury, the nearest the sun, beginning with one as the unit, we will find a gradual increase in weight per cubic foot from one for Neptune up to about nine for Mercury. If we set down the velocity of the planets in the same manner we will find the singular fact of an increase in the same way, from one for Neptune to about nine for Mercury. So that the velocity and the weight per foot increase together in a way that looks very suspicious of some connection between them.

What makes it look so singular is that the distance from the sun decreases almost in the very ratio of these two proportions multiplied into each other; or in the very way which it would do if the planets were formed of matter which had been concentrated by the heavy parts being driven toward the sun by gravity and the lighter parts being driven away by some other force—such as that which seems to be driving off the tail of the comet—so that each planet was formed of matter separated by its specific gravity in a general way, according to its distance from the sun and its velocity. Another thing which confirms this singularity is that the average weight of the meteoric masses which fall on to the earth, made up mostly of iron and some lighter rock, is very nearly that of the earth itself, taken as a whole, or about five and a half on the same scale, due to its position and velocity.

All this leads us to suppose that there is a force driving outward from the sun, as gravity drives toward it, but acting in proportion to the size of particles as gravity acts in proportion to their weight, which separates matter so that its average distance from the sun and its velocity shall conform to its average weight.

If this is true, as it seems, it throws light upon an obscure point, which may be considered as one of the most sublime within the reach of science; the nature of that wonderful mystery of gravitation itself, which holds and moves all the innumerable hosts of heaven in their everlasting circuits.

The course of modern thought is to render inconceivable the action of gravity as of an immaterial agent.

The theory of Lesage that it is the result of converging corpuscles of wave beats from all sides tending to drive bodies together is both sublime and in accordance with the habits of modern thought. But it utterly fails in one half of the problem. It does not explain what becomes of the dynamic energy of this force after it strikes a mass of matter, by which disappearance it is supposed to produce a shadow outward on all sides, to which the result of gravitation of masses to each other is attributed.

But if it should appear that there is a force thus going outward from the sun and other matter, as comets and planets in this way seem to indicate, then we are compelled to account for it also, which is the very force that Lesage's theory failed to show, and which his force requires for its complement.

This would require only to suppose the form of the force changed, in quantity proportionate to the quantity of matter, by passing through it, so as to act against particles in proportion to size, and to some other features, of which velocity and kind are elements, instead of in proportion to weight only, as before.

We cannot blame Lesage for overlooking the inconsistency of the utter disappearance of so much dynamic energy as his theory requires, because in his day the idea of the conservation of energy had not grown up; and it was a great, a sublime, grasp of thought, to conceive of a relation of mechanical action which was parallel in its nature to that utter, that bewildering, mystery of gravitation, which seemed as if it could only be due to the fiat or action of Creative Energy itself, acting forever and everywhere *de novo;* yet, at the same time, always with an absolutely steady and measured force and relation to quantity of matter, to distance in space, and to length of time, which indicated kinship in character to the other proximate and not ultimate forces of nature.*

But we cannot so easily overlook the failure of those who have later considered this theory to notice this great dynamic hiatus, and to follow it up to some conclusion.

These facts, stated, of the comets, of the planets, and of meteorites, indicate very clearly that there is a peculiar propulsive force acting outward from the sun.

And this force is of the general nature required to fill this hiatus.

Can we further determine anything of its nature?

We have already seen that it seems to act upon some kinds of matter in preference to other kinds; and that there seems to be different varieties of this selective difference caused by and in some proportion to velocity.

This last is a curious feature. How can velocity act to increase the action of a force on one kind of matter more than on another? Can any of the facts of ordinary knowledge give us any indications?

If we subject different substances to dry friction, electro-static disturbance is produced; the different kinds of substance will be acted upon differently, and perhaps the difference may be increased by the increase of the friction.

Now the condition shown in the comet is very much like that of an electrified body. But we must not jump to conclusions without examining the attendant conditions which would govern the facts.

We can suppose that the velocity of a body or assemblage of bodies through the ether, required to transmit light, or through a space containing other stray particles of matter, might produce a friction that would set up in it an electrified state; and which would be increased by increase of the velocity.

We can suppose that the light and electrical bodies, and the heavy metals would be electrified to different degrees; or at least that there would be different electric states produced,

And we can suppose that THE FORCES ACTING OUTWARD FROM THE SUN ACT ON PARTICLES IN SOME PROPORTION TO THEIR ELECTRIFIED STATES; and that on striking an assembly of particles it is reflected from their members, something like light is, in a great number of directions, which tends to drive them outward, and, in a less degree, to disperse them apart, as shown by the tail of the comet.

These suppositions show that the requirements which observation seems to call for have parallellisms within our knowledge, and indicate the course of new enquiries.

As a result of these and other considerations we may be led to infer that the growth of the solar system has been affected by such causes. That the heavy metals have, in coming into it, taken positions at last, very much dependant upon their weight and kind, in which respect the Earth, Venus and Mars, in their great interior masses, may represent the region of iron, while Mercury may represent the region of still heavier metals, and the outer planets the great mass of lighter substances; the average or mean distance of a body from the sun being governed inversely as the square of its mean velocity.

Thus a comet and its tail may become the missing link in astronomy and in science.

* We may believe that under the whole face and system of Nature there is an ultimate creative force which acts immediately each instant, to keep alive, to measure, and to guide, all of the actions and reactions taking place; but that is a conclusion and not a "knowledge." If it is true, yet it chances that the character of the action is such that we recognize all actions and reactions as taking place in chains having equality of links and certain peculiarities we call laws; which constitute proximate causes.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE":—

I have been interested in reading Mr. Rock's account of his observation of the great Comet on the 6th of July. On that evening the comet was hidden at this Observatory by clouds until about ten o'clock, local time, when Mr. Wilson went into the dome to observe its position with the eleven-inch refractor. He soon returned, however, and called my attention to the remarkable appearance of the nucleus. I went to the dome and from that time until three o'clock we alternately examined the Comet, making sketches and measures. The fan had its usual appearance, but when first observed a bright red jet projected from the nucleus into the dark region on the side of the nucleus opposite the fan. This jet was totally different in appearance from those usually seen. It was at first straight and in brightness rivalled the nucleus itself; in fact at the first glance it seemed to form one with the nucleus. On a closer inspection, however, I saw that it had a transparent appearance but still intensely bright and red. The next glance showed that there was a dark line separating it from the nucleus. Mr. Wilson had already called my attention to this dark line before I went to the dome. During the first few minutes a decided change took place. The jet seemed to separate and form a nucleus of its own, so that for a time the comet appeared double; gradually, however, the detached portion grew fainter, until when last seen, at about three in the morning, although plainly visible, it was no brighter than the fan-shaped appendage on the opposite side of the nucleus. I noticed the band of light which Mr. Rock speaks of as connecting the "node" with the nucleus, and mentioned it to Mr. Wilson at the time, but this afterwards disappeared, leaving a separate mass floating like a cloud in the dark region opposite the fan.

There can be no question that a great outburst took place in the comet on that evening, nor that a portion of the nucleus became detached. The phenomenon was watched very carefully for five hours and I think I could hardly be mistaken in what I saw. ORMOND STONE.

CINCINNATI OBSERVATORY, *July* 19, 1881.

To the Editor of "SCIENCE."

In Mr. Rachel's reply, in No. 52, to my letter in No. 47 of "SCIENCE," he appears to entertain a different conception of the law of gravitation from that which I supposed to be usually entertained by astronomers. As there may be many others who share his view, it seems advisable to give a more detailed exposition of what I think was Sir Isaac Newton's own conception, and is that of many more recent astronomers.

Newton's law of gravitation is that "every particle of matter in the universe attracts every other particle with

a force that is directly proportioned to the mass of the attracting particle and inversely to the square of the distance between them." The first question arising is, what are we to understand by "particle" in this theory? Certainly not a mass of utterly indefinite size. Undoubtedly Sir Isaac meant a mass of unit size, since the very terms of the proposition require this. For if every particle attracts every other particle with a certain vigor, then it must necessarily attract two particles with twice the vigor with which it attracts one. Or if a particle of unit mass attracts another with unit vigor, then it must attract two others, or one other whose mass is double the unit vigor. And as each of these two others attracts it with the unit vigor, then their sum, or their double mass, must attract it with double the unit vigor. It is simply the principle, still older than Newton in its expression, that "action and reaction are always equal and opposite."

But if it be granted that such will be the reaction between a unit and a double unit mass, the whole question is settled. For if the second mass may be doubled it may be quadrupled, or may be increased a million fold, without any difference in the principle. And likewise the first mass may be increased without affecting the principle. A mass of one unit attracts a mass of ten units with an energy equal to ten units, since it attracts each of the ten with unit energy. And each unit of the ten reacts on the one with unit energy, so that their combined attraction equals ten units. Again, if the first mass contain two units, each of these separately acts upon each of the ten with unit energy. Thus as each unit of the two exerts ten units of energy, the two together exert twenty units. In other words, the energy which the first mass exerts upon the second is proportioned to the product of the number of mass units in the first into the number of mass units in the second; and the action of the second upon the first also is in proportion to the product of their units.

This is the true principle of the attraction of gravitation. We may take the unit mass of any size we wish. In the action between the earth and the moon, for instance, we may take the mass of the moon as the unit, that of the earth being about 75 units. The moon will attract each unit of the earth with unit energy, and the whole earth with an energy of 75 force units. But each unit of the earth will react upon the moon with unit energy, and the whole earth will exert on the moon an energy of 75 force units. Thus the moon attracts the earth with precisely the same vigor as the earth attracts the moon. Of course the resulting motions are not the same, but the resulting momentums are precisely equal. As the earth is 75 times the weight of the moon a motion of one foot per second in the earth would give it a momentum equal to that given the moon by a motion of 75 feet per second. It is well known that the moon does not revolve around the centre of gravity of the earth, but that these two bodies revolve around their common centre of gravity. But this common centre is within the mass of the earth, and may be found by dividing the distance between the centres of the earth and moon by the ratio of their weights. If we take this distance as 240,000 miles, and divide by 75—the weight of the earth as compared with the moon—the common centre of gravity will appear to be 3200 miles from the earth's centre.

Or we might consider this case from the principle of inertia. The earth having 75 times the mass of the moon has 75 times the inertia, or resistance to exterior forces. Thus its movement in response to lunar attraction is only 1-75th that of the moon in response to terrestrial attraction. But its weight being 75 times greater, its momentum in response to lunar attraction must be precisely equal to the moon's momentum in response to the earth's attraction; or, in other words, their vigor of action upon each other must be precisely equal. This movement of the earth under the action of the moon does not affect the line of its orbital movement, since it is less than the length of the earth's radius. Its movement is like that of a bead with a large aperture, which advances along a string moving from side to side, but not leaving the string. But as the earth moves about 46 millions of miles in its orbit while completing one of these gyrations, the effect is excessively minute. That of the moon, in fact, which swings 240,000 miles to each side of the orbit each fortnight, is very slight when compared with the length of the orbit.

But if, the earth being 75 times the mass of the moon, it also attracted the moon 75 times more vigorously than the moon attracts the earth, this common centre of gravity would be found by dividing 240,000 by 75^2 and would be but 36 miles from the earth's centre. As to which of these results is the more correct the books will show.

I am, therefore, obliged to repeat the idea advanced in my former article. An atom falling towards the earth attracts it with as much energy as the earth attracts the atom, and they move toward each other with equal momentums. But the great weight of the earth reduces its rate of motion towards the atom to a speed inconceivably small, while the small weight of the atom gives it an excessively rapid speed towards the earth. It would be strange if, at this late date in the history of the theory of gravitation, I had been the first to advance this idea as Mr. Rachel seems to suppose. Perhaps my mode of presenting it may be original, but I can readily quote other expressions of the same idea. Thus Dr. Ball, Royal Astronomer of Ireland, speaks as follows, in his article on Gravitation in the new edition of the Encyclopedia Britannica: "It has been found that the intensity of the attraction of gravitation between two masses is directly proportional to the product of those masses." This is precisely the result I have reached in the above argument. Again he says: "Let m, and m' be the masses of two bodies, and let r be their distance. The force with which m attracts m' is equal in magnitude though opposite in direction to the force with which m' attracts m. The reader may perhaps feel some difficulty at first in admitting the truth of this statement. We speak so often of the effects which the attraction of the sun produces on the planets that it may seem strange to hear that each planet reacts upon the sun with a force precisely equal and opposite to the force with which the sun acts upon the planets." He illustrates as follows: "Suppose the earth and the sun to be at rest in space, and prevented from approaching each other under the influence of gravity by a rigid rod extending from one to the other. If now the sun pressed toward the earth more vigorously than the earth toward the sun the greater pressure of the sun must overcome the lesser pressure of the earth, and the whole arrangement would be driven through space in the direction in which the rod points outward from the sun. For there would be a motion producing vigor in the sun unopposed by a sufficient resistance in the earth. And yet, in the event of such a movement, we would have the kinetic energy of their motion created out of nothing, which is now well known to be impossible." Such is Dr. Ball's argument briefly stated. It leads to the same result as mine, and I therefore claim to be in full accord with the Newtonian law of gravitation.

In regard to the other points of Mr. Rachel's letter there is nothing on which I desire to dwell. As to the use of the phrase "Latent Heat," the scientific world will be very ready to give it up if a term can be suggested more significant of the character of the energy indicated. But there would be nothing gained by simply substituting one unmeaning name for another. Mr. Rachel himself uses the phrase "Radiant Heat," yet he must be aware that the mode of motion so called is very different from ordinary Heat Motion. Radiant Heat is readily convertible into Static Heat; but so is

Electricity; and we have the same warrant to consider Electricity as some modification of Heat. In fact the term "Radiance" would be a more distinctive appellation than "Radiant Heat."

As to trust in authorities, of course we must trust in them as long as their explanations seem most in accordance with facts, but no longer. Well-established facts are the only trustworthy data of Science. No theory can be sustained against the pressure of unconformable facts. In short, every theory is in danger while a single fact remains unexplained. For the facts of nature are so closely linked that each in some way bears upon all, and all upon each. And yet it is by no means advisable to stop theorizing, for correct theories are themselves facts of science—facts concerning forces and relations as deduced from facts concerning things. And every partially correct theory is a footstool through which higher levels of conception may be reached; while every theory proved incorrect is a warning board, advising all future scientists not to waste time in following a path that leads nowhere.

CHARLES MORRIS.

2223 SPRING GARDEN STREET, PHILADELPHIA.

BOOKS RECEIVED.

TEXT-BOOK OF EXPERIMENTAL ORGANIC CHEMISTRY for Students, by H. CHAPMAN JONES. D. Van Nostrand. New York, 1881.

Although termed a text-book, the author admits that this little volume will be found of greater use as a companion for the student in the laboratory, who wishes to study organic chemistry both practically and theoretically.

We recommend this volume to those who have a limited time at their command for study, and are not overburdened with cash, the author having wisely restricted the number of experiments, and suggested only such as are available in a laboratory of the humblest pretensions, and the use of expensive chemicals is altogether avoided. The author has shown considerable judgment in arranging this work, the plan of which is excellent, because while the subject has been reduced to its simplest form, the instructor will find all that is necessary for teaching the elementary stages of practical organic chemistry, and it will serve as a reliable guide to the average student who relies on his own resources for instruction.

CONTRIBUTIONS TO METEOROLOGY: being results derived from an Examination of the Observations of the United States Signal Service, and from other sources. By ELIAS LOOMIS, Professor of Natural Philosophy in Yale College.

A pamphlet reprinted from the *American Journal of Science*, being the subject matter of a paper read before the National Academy of Sciences. Washington, April 19, 1881.

ON THE GROUP "*b*" ON THE SOLAR SPECTRUM. By WILLIAM C. WINLOCK. From the proceedings of the American Academy of Arts and Sciences. Presented by Professor Wolcott Gibbs. June 9, 1880.

The most complete charts of the solar spectrum now available are Kirchhoff's, which were published in 1861, and Angström's, published in 1869. Kirchhoff employed a battery of four flint-glass prisms, with a collimator and observing telescope each of about 4 centim. aperture and 49. centim focal length; while Angström used telescopes of about 4.6 centim. aperture, and 36.3 centim. focal length, and a diffraction grating made by Nobert, containing about 133 lines to the millimetre.

Such great advances have been made very recently in the construction of optical instruments, and more especially in the ruling of diffraction gratings, that it would now be possible to enlarge Angström's great chart almost as much as he improved upon Fraunhofer's first maps. But it would be an almost endless undertaking for a single observer to attempt a map of the whole spectrum, from the ultra-violet to the invisible red, brought to light by our most powerful instruments, and accordingly most physicists who have paid especial attention to solar spectroscopy have devoted themselves to a careful study of detached portions which appear of unusual interest. As a contribution to this work, the following observations upon the group of dark lines "*b*," of the solar spectrum, were undertaken by Mr. Winlock, at the suggestion of Dr. Gibbs, and carried on under his immediate supervision.

A PRACTICAL TREATISE ON THE MANUFACTURE OF STARCH, STARCH-SUGAR AND DEXTRINE, based on the German of Ladislaus Von Wagner and other authorities, by JULIUS FRANKEL. Edited by Robert Hutter. Illustrated by 58 engravings, covering every branch of the subject. Henry Carey Baird & Co., 810 Walnut street, Philadelphia, 1881. Price, $3.50.

The increased manufacture of Glucose and the prospect of this substance becoming a staple article of produce in the United States, makes this volume a welcome addition to the excellent series of technical works published by this house.

Those about to engage in the manufacture of Glucose will find this treatise an indispensable guide, and, as we understand, it is the only work in the English language describing in detail the processes and machinery made use of in this important class of industry.

It is stated in the preface that this subject has been heretofore surrounded by more or less mystery than any other manufacture of recent years, and that access to factories has been barred to all but workmen, and that inventors and manufacturers of the necessary machinery have refused to furnish drawings of the machines. It is therefore evident that the present work, which has been prepared with care, intelligence and zeal by one who is a master of the subject, must be a valuable acquisition to those interested in this industry.

Mr. Frankel introduces the subject by describing the Chemistry of Starch, its technology and methods of manufacture. The Chemistry of Starch-sugar is then taken up and its manufacture in all its branches explained in detail. The author concludes with an exhaustive description of Dextrine and its manufacture.

It was Professor Kirchhoff, of St. Petersburg, Russia, who made the important discovery in 1811, that starch boiled in diluted sulphuric acid is transformed into sugar, but the origin of glucose manufacture dates from the time of Napoleon I., when the Engl sh were blockading the Continent. At the time it caused a great and general sensation, as it was then thought that grape sugar was identical with cane sugar, and hence could in every respect be substituted for that product. This new branch of industry was, therefore, pursued with energy, and immense quantities of starch-sugar were manufactured, but subsequently, when it was proved that this material was by no means identical with cane sugar, being less soluble, of less sweetness, and not at all suitable to serve as a substitute for the former, then for a number of years the demand ceased. Of late years a revival has taken place in this industry, and in 1876 Germany alone produced in her 47 glucose, starch-sugar and syrup factories 100 million pounds, and as we stated in a recent article 500 tons a day of glucose are now produced in the United States.

It is singular to observe that such substances as Starch, Grape-sugar and Cane-sugar, which have such opposite properties in some respects, are almost chemically alike. If starch absorbs two molecules of water, it becomes transformed into glucose (grape or starch sugar), while cane sugar contains one molecule more than starch and one molecule less than the starch sugar. The chemical

composition of these substances may be compared by arranging their formulæ in the following manner:

$$C_{12}\ H_{20}\ O_{10},\ \text{or}\ C_{12}\ H_{10}\ O_{10} = \text{Starch.}$$
$$C_{12}\ H_{22}\ O_{11},\ \text{or}\ C_{12}\ H_{11}\ O_{11} = \text{Cane Sugar.}$$
$$C_{12}\ H_{24}\ O_{12},\ \text{or}\ C_{12}\ H_{12}\ O_{12} = \text{Grape Sugar.}$$

Grape-sugar is largely diffused throughout the animal kingdom, and is found in most of the sweet tasting fruits. It is contained in the honey of the bee, and is separated in large quantities in the urine of those unfortunates who suffer from that disease of the kidneys called *diabetes mellitus*. Grape-sugar is not only found in nature but can be produced chemically. Thus it is formed as a result of the action of diluted acids, diastaste, gluten, saliva, etc. on starch, and for this reason starch is used for its production on a large scale.

The fullest directions are given in this work for the manufacture of glucose from starch, and we congratulate the publishers on producing a book at a moment so *apropos*, and we regret we cannot devote more space to the subject; we advise, however, all interested in this new and rising industry to obtain a copy of the work, for it apparently presents all the facts bearing on the manufacture of glucose, in a very convenient form.

REPRODUCING DRAWINGS, DESIGNS, &c.

The following method of reproducing drawings, &c., in any desired color, has been patented by M. M. Tilhet, of 18 Rue de la Paix, Paris. The paper upon which the design is to be reproduced in order to prepare a negative copy is first passed through a bath composed of the following materials in about the proportions given: White soap, 30 parts by weight; alum, 30 parts; Flanders glue, 40 parts; the white of eggs or albumen beaten up, 10 parts; glacial acetic acid, 2 parts; alcohol at 60 degrees, 10 parts; water, 500 parts. The paper, after having been removed from this bath, is passed through a second bath composed as follows: Burnt umber, ground in alcohol, 50 parts by weight; black pigment, 20 parts; Flanders glue, 10 parts; water, 500 parts; bichromate of potash, 10 parts. The paper having been thus treated must be kept when dry in a dark place. In order to prepare positive paper for the prints, a bath is used similar to the last, but without the umber, for which black pigment is substituted. Or, if it is desired to obtain colored proofs instead of black ones, the black pigment is replaced by a pigment of red, blue, or any other desired color. To prepare the copies, the design or drawing is placed in an ordinary photographic printing frame, the back of the design being next to the glass, and a sheet of negative paper prepared in the way first described is placed in contact with it. The frame is then exposed to light, two minutes exposure being sufficient in good weather. The sensitive paper is then removed from the frame in a dark place and is placed in water, when the design becomes visible in white, and the paper is then allowed to dry. In order to obtain positive pictures from the negative thus prepared, the latter is placed in the printing-frame with a sheet of the positive paper prepared in the manner above described in contact with it, and after exposure to light for a sufficient time, that is to say, about two minutes, the positive paper is removed in a dark place, and is plunged into water, which removes the part of the pigment which has not been affected by the light, without its being necessary to touch it. Any number of copies of the design or drawing may be produced by the novel method described upon any kind of paper, and in any color or colors. The proportions of the different materials used to prepare the baths as above described may be varied to suit varying circumstances, such as the weather and the character of the design or of the paper.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING JULY 23, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

JULY.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.
Sunday, 17	29.579	29.618	0 a. m.	29.508	5 p. m.
Monday, 18	29.523	29.596	0 a. m.	29.446	4 p. m.
Tuesday, 19	29.611	29.690	12 p. m.	29.542	0 a. m.
Wednesday, 20	29.686	29.742	9 a. m.	29.632	12 p. m
Thursday, 21	29.553	29.638	12 p. m.	29.500	2 p. m.
Friday, 22	29.596	29.638	0 a. m.	29.546	6 p. m.
Saturday, 23	29.646	29.722	12 p. m.	29.586	3 a. m.

JULY.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 17	73.6	66.6	81	2 p. m.	72	2 p. m.	64	12 p. m.	60	12 p. m.	131.
Monday, 18	69.3	61.3	74	4 p. m.	63	4 p. m.	62	6 a. m.	59	6 a. m.	126.
Tuesday, 19	74.7	65.0	84	3 p. m.	68	4 p. m.	66	6 a. m.	60	6 a. m.	130.
Wednesday, 20	77.3	68.0	83	5 p. m.	71	5 p. m.	69	6 a. m.	65	6 a. m.	123.
Thursday, 21	78.3	69.3	87	3 p. m.	72	2 p. m.	69	12 p. m.	64	12 p. m.	135.
Friday, 22	73.3	66.0	77	3 p. m.	69	6 p. m.	66	5 a. m.	62	6 a. m.	127.
Saturday, 23	72.0	65.7	79	3 p. m.	68	3 p. m.	65	5 a. m.	61	5 a. m.	139.

Mean for the week .... 29.599 inches.
Maximum for the week at 9 am., July 20th .... 29.742 "
Minimum " at 4 pm., " 18th .... 29.446 "
Range .... .296 "

	Dry.		Wet.	
Mean for the week	74.0	degrees	65.9	degrees.
Maximum for the week, at 3 pm. 21st	87.	" at 2 pm 21st,	72.	"
Minimum " " 6 am. 18th	62.	" at 6 am 18th,	59.	"
Range " "	25.	"	13.	"

JULY.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.
Sunday, 17	w. n.w.	w. n.w.	n. n.w.	217	6	4.40 pm
Monday, 18	n. w.	w. n.w.	w. n.w.	238	9½	11.40 am
Tuesday, 19	w. n.w.	n. w.	w.	219	6½	1.50 pm
Wednesday, 20	w.	w. s. w	s. w.	156	2¼	1.30 pm
Thursday, 21	w. s. w.	n. w.	n. n. w.	227	4¾	3.20 pm
Friday, 22	n. w.	n. n.w.	n. n. e.	82	1½	8.50 pm
Saturday, 23	n.	n. e.	n. n. w.	140	2	2.30 pm

JULY.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.
Sunday, 17	.537	.663	.495	71	63	70
Monday, 18	.433	.429	.449	73	51	61
Tuesday, 19	.457	.460	.554	69	42	64
Wednesday, 20	.537	.534	.612	71	49	62
Thursday, 21	.614	.609	.568	68	51	67
Friday, 22	.495	.564	.568	70	61	67
Saturday, 23	.489	.564	.595	74	61	76

JULY.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water.	OZONE. 1 10
Sunday, 17	3 cir.cu	2 cir. cu.	0	-----	-----	-----	--	4
Monday, 18	1 cir.	7 cir.cu.	0	-----	-----	-----	--	2
Tuesday, 19	0	1 cir. cu.	0	-----	-----	-----	--	1
Wednesday, 20	4 cir.cu	3 cir.cu s	10	-----	-----	-----	--	1
Thursday, 21	3 cir.cu	4 cir. cu.	0	-----	-----	-----	--	8
Friday, 22	2 cir.	9 cu.	0	-----	-----	-----	--	8
Saturday, 23	8 cir.cu	4 cir. cu.	1 cir. s.	-----	-----	-----	--	1

Distance traveled during the week .... 1279 miles.
Maximum force .... 9½ lbs.

Total amount of water for the week .... 0 inch.
Duration of rain .... 0 hours 0 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

TERMS:

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SATURDAY, AUGUST 6, 1881.

The crime of Guiteau has directed public attention to the subject of mental diseases; we will therefore endeavor to explain the teachings of some of the most prominent of modern alienists who have recorded the results of their investigations, and classified the various phases of this, the greatest curse of humanity.

The first point of interest to be discovered is, can any line be drawn between partial and absolute insanity; if one faculty of the mind is affected do all succumb?

On this point, as on most others bearing on this subject, there is much difference of opinion; but the most advanced alienists appear to be now satisfied that a partial form of insanity exists, which is termed monomania. The German and French alienists have long since recognized this distinction and invented terms to express it, but it appears to be due to Dr. Edward C. Spitzka to have introduced this term with its proper modifications into English psychological literature.

The delusions of the monomaniac are what would be day-dreams in other people, "but which have become fixed realities for the former, owing, it is said, to a faulty cerebral association system, which permits collateral circumstances to act as supports for the patient's erroneous conception."

The general intellectual status of monomaniacs, though rarely of a very high order, is moderately fair, and generally the mental powers are sufficient to keep the delusion under check for the practical purposes of life, and although many are what is termed crotchety, irritable and depressed, yet the sole mental symptoms of the typical cases of this disease consists of the fixed delusions.

Without describing in detail the various features of monomania, let us take an imaginary case of this character, and sketch its leading characteristics. To protect us from any reproach of exaggeration or of drawing a fictitious image, we will take an extract from a paper by Dr. Edward C. Spitzka read before the New York Neurological Society, as far back as November 1880.

The monomaniac after experiences incidental to the early stages of the disease at length concludes that he is a person of some importance.

"*Some great political movement now takes place, he throws himself into it either in a fixed character that he has already constructed for himself, or with the vague idea that he is an influential personage. He seeks interviews, holds actual conversations with the big men of the day, accepts the common courtesy shown him by those in office as a tribute to his value, is rejected, however, and then judges himself to be the victim of jealousy or of rival cabals, makes intemperate and querulent complaints to higher officials, perhaps makes violent attacks upon them, and finally is incarcerated in a jail.*"

The writer of this paper had no intention of being prophetic in his utterances, but our readers cannot fail to observe the very close relation the above picture bears to any mental portrait which might be drawn of the assassin Guiteau.

It is curious that all through this train of ideas to which the monomaniac abandons himself there is seen a chain of logic and inferences; there is no gap anywhere. If the inferences of the patient were based upon correctly observed facts and associated with a proper correlation with his actual surroundings, his conclusions would be perfectly correct.

We have therefore in the monomaniac an individual with full reasoning powers, and intellectually the equal of most men. In what respect does his status differ from the sane man? The answer is, that he is possessed *with a fixed delusion* or insane project.

To follow the subject intelligently, let us now enquire what an alienist terms a delusion, and analyze its nature. This can be done profitably, for we are told that such a preliminary investigation is the most direct step for those who would be initiated into the mysteries of the insane mind.

Genuine delusions are divided into two fundamental classes; the first styled SYSTEMATIZED DELUSIONS as contrasted with the second class of UNSYSTEMATIZED DELUSIONS.

It may be here stated, that assuming Guiteau to be a monomaniac, his delusions would be of the first class.

The highest general mental development among constitutional lunatics is found among those who cherish

systematized delusions of social ambition; the delusion being often the outgrowth from a dream, or from an actual hallucination. These men usually imagine themselves the worst enemies of mankind, or are social reformers, inventors, poets, &c.; but as Spitzka remarks, it is often noticed, especially with patients of high culture, that the delusions are not so monstrous as to lead to an error in the patient's sense of identity, but limited to his self-esteem in the abstract.

Systematized delusions may also be of an expansive errotic character, when the patient constructs an ideal of the other sex, and may on some occasion discover the incorporation of his ideal, in an actual personage, usually in a more exalted position than his own. Systematized delusion may also be of an expansive religious nature, or lastly of a DEPRESSIVE CHARACTER.

We would like to give our readers a formula by which they might detect a *systematized delusion*, but although alienists are specific in their language and ample in their detail, brevity is not attempted, as perhaps not possible in treating so complicated a subject.

When, however, we see an individual without any manifest disturbance of his emotional and effective states, in full possession of the memories accumulated in the receptive sphere, and able to carry out most or all of the duties incident to his social position, yet firmly believing in the reality of that which from his education and surroundings, we should expect him to recognize as absurd, or radically wrong, the probability is that the phenomenon is due to a *systematic delusion.*

The one fundamental character which distinguishes the delusions of systematic delusional lunatics, is the correlation with their surroundings, or of their unelevative physical status. However falsely the patient's sensations and external circumstances may be interpreted, yet, after all, there is a *pseudo* logical chain running from them to the delusion which they keep to create and to sustain. This is absent in the case of patients exhibiting unsystematized delusions. Again, up to a certain stage, the *systematized delusion* is analogous to a healthy conception, this is never the case in an *unsystematized* delusion.

The factors engaged in producing the systematized delusion are two-fold. One, the predisposition we have recognized as presumably based upon anomalous condition of the brain, and the other some exciting cause which must be studied.

For instance; the general mental tone of the patient. If he be of sanguine disposition, the delusion is often the outgrowth of a day-dream, on the plan of the saying that the wish is father to the thought. If he be of a suspicious turn, delusions of persecutions are apt to arise.

Again, the physical state may influence the patient. If this be fair, delusions are apt to be expansive, and to involve social and sexual matters.

And lastly, the circumstances of the patient, as the age in which he lives, the education he receives, his social condition. All these modify the character of the delusions of this class of the insane. It is admitted that while all these factors are of the highest importance, they will never create a *systematic* delusion, unless the cerebral predisposition exists.

Of the *unsystematic delusions* we shall be brief, as they are characteristic of the acuter insanities, and are, therefore, more easily recognized. The patient exhibiting them never acts in strict accordance with his assumed character, and there is no consistency in his behavior. The unsystematized delusional lunatic will tell you that he is possessed of a million dollars, but he cannot account for his being richer today than he was yesterday.

It is pointed out that the great line of demarcation between the two classes of delusions lies in the fact, that, in the systematized delusion all the powers of logic and mental qualities that the man ever had are utilized by him in the construction and defense of his delusion, and as Spitzka points out is of great medico-legal importance, are also utilized in the carrying out of his schemes of defense or *revenge.* On the other hand, the unsystematized delusionist is deprived of his logical power, and apart from his hallucinations is unable to specify any support for his morbid ideas, and his actions betray that same lack of system which his delusions do.

We are indebted to two papers* by Dr. Edward C. Spitzka in presenting this classification of delusions of Monomaniacs, and we are somewhat surprised to find that he appears to ascribe all these classes of delusions to direct cerebral troubles, in fact he ridicules their being attributed to functional complications and diseases of other organs of the body.

The expert alienist can no doubt draw the distinction, and decide correctly on the true source of the mental disturbance, but it cannot be doubted that much error in this respect is exhibited by the inexperienced, for delusions of every kind are manifested at least temporally in many forms of disease, which in some cases may be so persistent as to appear chronic. As Dr. Spitzka himself frequently protests in his papers on the indiscrimate power which courts and physicians possess, who often consign "useful members of society to the living tomb of an asylum, and to the tender mercies of an ex-horse car conductor, or ex-night watchman or other politician," he will excuse the exception we take to the too ready desire of many persons to place in the category of "maniacs," men who are merely hypochrondriacal or depressed and vicious in disposition.

We have indicated the form of insanity, which may be ascribed to the case of Guiteau, but we have no desire to prejudge the case. The crime was barely committed before Cabinet Ministers, Physicians, Editors, and a large portion of the public, immediately jumped to the conclusion that the assassin was mad; that such a verdict was hastily given all must now admit. Whether the evidence, which will undoubtedly be produced at the trial, will justify the first impression, and release the prisoner from the responsibility of this crime, will be a matter to be watched with considerable interest.

*Insane delusions, their mechanics and their dragnostic bearing. The Journal of Nervous and Mental Disease, January, 1881. Monomaniac or "Primære Verruecktheit." Read before the Neurological Society, Nov. 5, 1880. Reported in St. Louis Clinical Record.

WE recently called attention to a report made by Professor Leeds to the Chemical Society, of New York on the adulterations of certain articles of food. The tenor of the report was to show that food products in general were unadulterated and pure, and to cast ridicule on those who asserted to the contrary. Among other specific statements Prof. Leeds stated that he had made a special examination of sugar syrups, and asserted that the result of his investigations showed, that they were free from any admixture of glucose.

Side by side with Prof. Leeds' report we gave the statement of Prof. Wiley that 500 tons of glucose was made daily in the United States, the bulk of which was used for adulterating cane sugars, and that the glucose of commerce as sold in the Western States was largely composed of syrup made from starch.

We publish in this issue a letter from Prof. Wiley in confirmation of his report, stating that the manufacture of a sugar, which is a mixture of glucose and cane sugar, is carried on in New York city or its vicinity.

AMYLOSE.

As a thousand tons of sugar made from starch will within a few months be placed on the market daily, half that amount being already the consumption of that article of commerce, it appears desirable to make use of some name by which this substance may be known and at the suggestion of Prof. Wiley, we propose "AMYLOSE" as an appropriate term.

AMYLOSE will include all varieties of syrups and sugars manufactured from starch. (Lat. *Amylum*, *Starch*).

NOTE ON PHOTOGRAPHS OF THE SPECTRUM OF THE COMET OF JUNE, 1881.

BY PROFESSOR HENRY DRAPER, M. D.

The appearance of a large comet has afforded an opportunity of adding to our knowledge of these bodies by applying to it a new means of research. Owing to the recent progress in photography, it was to be hoped that photographs of the comet and even of its spectrum might be obtained and peculiarities invisible to the eye detected. For such experiments my observatory was prepared, because for many years its resources had been directed to the more delicate branches of celestial photography and spectroscopy, such as photography of stellar spectra and of the nebulæ. More than a hundred photographs of spectra of stars have been taken, and in the nebula of Orion details equal in faintness to stars of the 14.7 magnitude have been photographed.

It was obvious that if the comet could be photographed by less than an hour's exposure, there would be a chance of obtaining a photograph of the spectrum of the coma, especially as it was probable that its ultra-violet region consisted of but few lines. In examining my photographs of the spectrum of the voltaic arc, a strong band or group of lines was found above H, and on the hypothesis that the incandescent vapor of a carbon compound exists in comets this band might be photographed in their spectrum.

Accordingly, at the first attempt, a photograph of the nucleus and part of the envelopes was obtained in seventeen minutes on the night of June 24th, through breaks in the clouds. On succeeding occasions, when an exposure of 162 minutes was given, the tail impressed itself to an extent of nearly ten degrees in length.

I next tried by interposing a direct vision prism between the sensitive plate and object glass to secure a photograph which would show the continuous spectrum of the nucleus and the banded spectrum of the coma. After an exposure of eighty-three minutes, a strong picture of the spectrum of the nucleus, coma and part of the tail was obtained, but the banded spectrum was overpowered by the continuous spectrum.

I then applied the two-prism spectroscope used for stellar spectrum photography, anticipating that although the diminution of light would be serious after passing through the slit, two prisms and two object glasses, yet the advantage of being able to have a juxtaposed comparison spectrum would make the attempt desirable, and moreover, the continuous spectrum being more weakened than the banded by the increased dispersion the latter would become more distinct.

Three photographs of the comet's spectrum have been taken with this arrangement with exposures of 180 minutes, 196 minutes and 228 minutes, and with a comparison spectrum on each. The continuous spectrum of the nucleus was plainly seen while the photography was in progress. It will take some time to reduce and discuss these photographs and prepare the auxiliary photographs which will be necessary for their interpretation. For the present it will suffice to say that the most striking feature is a heavy hand above H which is divisible into lines and in addition two faint bands, one between G and *h* and another between *h* and H. I was very careful to stop these exposures before dawn, fearing that the spectrum of daylight might become superposed on the cometary spectrum.

It would seem that these photographs strengthen the hypothesis of the presence of carbon in comets, but a series of comparisons will be necessary, and it is not improbable that a part of the spectrum may be due to other elements.

271 MADISON AVENUE, NEW YORK.

OBSERVATIONS ON SIREDON LICHENOIDES.*

BY WM. E. CARLIN.

Como Lake is a body of water about two miles and a half in circumference. It has no known outlet, but is fed by a stream of pure spring water about two feet wide and a foot deep, which, continually running, prevents the lake's absorption by evaporation. The lake is quite shallow and can be easily waded at almost any part, being not more than 10 feet deep in the deepest place that I have been able to find. The bottom of the lake is soft and is covered in most places with grass and weeds. The water is strongly impregnated with alkali, and a large number of cattle are said to have died a number of years ago from drinking it. It is very disagreeable to the taste. The amount of water varies about 14 inches during the year, being highest in the spring from the melting snows, and lowest in the autumn. This is the home of the *Siredon lichenoides* (Baird). They never enter the stream of fresh water, preferring the alkali water of the lake. They seem to suffer no inconvenience, however, if placed in fresh water. I have caught as many as a hundred and fifty and

*From the Proceedings of United States National Museum.

placed them in a cauf, and have never had one die from the change. The change to fresh water undoubtedly hastens the metamorphosis into the *Amblystoma* form, as I have noticed quite a change in the course of twenty-four hours in individuals placed in the cauf, while an equal number kept in the alkali water in the boat have shown no change in any of them in several days. I have kept six at different times in jars of fresh water until they have completed their metamorphosis. I made no systematic note of appearance from day to day, but my observation was careful and regular. In two cases the change in external appearance was so abrupt that I would have been almost certain that another salamander had been substituted for the one in the jar had I not had him so completely under observation that it was impossible. The gills had assumed a stubby form about half the length that they were the night before, and the gill on the back on the back of the body was nearly half gone; it took air quite often, and I removed it from the jar and placed it in a box with some lake grass around it to keep it moist. It completed the metatmorphosis in a few days. I did not feed it any during this time. While it was in the jar it was well fed with flies. The jar was placed upon a table in the telegraph office. The flies at first had to be pushed in front of it with a pencil. It finally got to know that tapping the jar with a pencil meant a fly, and would rise to the surface immediately and snap at whichever it saw first, pencil or fly. It furnished train-men continual amusement while here, and they kept it constantly gorged. Those that I kept well fed in jars and seldom changed the water, say once in three days, usually began to show a slight change in from two to three weeks, and all of them completed the change into the *Amblystoma* inside of six weeks, while I have had but three changes of those kept in the cauf (sixty of them) in three months. During that time they have not been fed at all. The *Siredon mexicanus* is said to never undergo the transformation in its home, and Professor Marsh doubts that it ever makes it here. This doubt I can put at rest. They do make the change here, and in large numbers. During the latter part of the month of July and the entire month of August, if the day is rainy or misty, they come from the lake to the shore in large numbers, and secrete themselves under some piece of wood or rock where they can keep moist. Sometimes they venture out in a shower, and the sun catches them before they can obtain shelter either in the lake or under cover, and in a few minutes kills them. They can be found dried hard anywhere about the lake, on the shore or in the grass. While catching *Siredon* I have seen and caught a number of *Amblystoma* in the lake, with the metamorphosis, as far as I could see, as complete as those we find half a mile from the lake. They cover the ground by thousands during a warm summer rain, coming from every conceivable place where they could have found shelter, from under rocks, boards, old ties, and out of gopher holes. I have a cat that eats them greedily. She has fished several out of jars on the table and devoured them during the night when there was no one to watch her; and I am told by a resident that the numerous skunks that live around the lake live principally on them. They are of two colors, a blackish green and a yellowish green color. I have had two of the blackish green complete the change in sequence, while one of the yellowish green was completing it under the same circumstances of change of water and food. I think this will be found to be the result in all similar cases. I have caught them in all stages of growth and in all stages of their changes into the *Amblystoma* state. During the months of July and August they lie close to the shore of the lake, where it is shallow; but after the first frost they disappear completely, or at least I have never been able to find them. I think they must bury themselves in the mud at the bottom of the lake, as I have stirred up the grass often and have not seen them issue from it.

AN ANALYSIS OF WATER DESTRUCTIVE TO FISH IN THE GULF OF MEXICO.*

BY F. M. ENDLICH.

Having completed the examination of sea-waters from the Gulf of Mexico, so far as the scant supply would permit, I have the honor to offer the following report thereupon, the water in which the fish die being designated as A, the good water as B:

	A.	B.
Specific gravity....................	1.024	1.022
Solid constituents (total), per cent...	4.0780	4.1095
Ferric compounds, per cent.........	0.1106	0.0724
Injurious organic matter...........	ratio=3	ratio=2

I find that the water A contains a large quantity of *Algæ* and *infusoria*. It is eminently probable that the former may have had an injurious effect upon the fish. Specimens of the algæ have been submitted to Professor Goode, who will send them to some expert, in order that their specific gravity may be determined.

The "dead fish" in possession of the United States National Museum are such that any examination of the organs of respiration will be of no avail.

I cannot find, even by spectroscopic analysis, any mineral constituents in the water A which could noxiously affect the fish.

In my estimation the death of fish was caused by the more or less parasitic algæ, which are found in large quantities in water A, but do not occur at all in water B.

In case the same phenomenon should recur, the presence of an expert in the questions involved, more particularly chemistry and botany, would most likely lead to definite results,

Prof. S. F. BAIRD,
Secretary the Smithsonian Institute,
WASHINGTON, D. C.

A MICROSCOPICAL STUDY OF THE IRON ORE, OR PERIDOTITE OF IRON MINE HILL, CUMBERLAND, RHODE ISLAND.†

BY M. E. WADSWORTH.

The attention of the writer was first particularly called to this formation by some specimens presented to him by Mr. H. B. Metcalf in the Spring of 1880. These did not appear to the writer to be any common ore of iron, but rather fragments of a basic eruptive rock containing much iron. Sections were accordingly made which revealed its true character.

The formation was described by Dr. Charles T. Jackson in his report on the Geological Survey of Rhode Island in 1840. He states that Iron Mine Hill "is a mountain mass of porphyritic magnetic iron ore, 462 feet in length, 132 feet in width, and 104 feet in height above the adjoining meadow. From these measurements, which were made over only the visible portion of this enormous mass of iron ore, it will appear that there are 6,342,336 cubic feet of the ore above natural drainage. ...Its specific gravity is from 3.82 to 3.88....This ore is remarkable both on account of its geological situation and its mineralogical and chemical composition. It appears to have been protruded through the granite and gneiss at the same epoch with the elevation of numerous serpentine veins which occur in this vicinity. This will appear the more probable origin of this mass, when we consider its chemical composition in comparison with that of the iron ore, which we know to have been thrown up with the serpentine, occurring on the estate of Mr. Whipple, and the fact that the ore at Iron Mine Hill is accompanied by serpentine mixed with its mass in every

* From the Proceedings of United States National Museum.

† From the *Bulletin of The Museum Comparative of Zoology.*—Harvard College.

part, gives still greater reason for this belief." (*l. c.*, pp. 52, 53.)

He gives as the result of his chemical analysis of the "Porphyritic Iron Ore from Iron Mine Hill, Cumberland," the following (*l. c.*, p. 53):—

SiO_2	23.00
Al_2O_3	13.10
Fe_2O_3	27.60
FeO	12.40
MnO	2.00
MgO	4.00
TiO_2	15.30
H_2O and loss	2.60
Total	100.00

In 1869 the Rhode Island Society for the Encouragement of Domestic Industry published a report relating to the coal and iron in Rhode Island, from which we glean the following. The iron ore is regarded as practically inexhaustible, the mass at Iron Mine Hill visible above drainage being estimated at two millions of tons.

"It is also conceded, as regards quality, that the Cumberland ore is free from sulphur and phosphorus, the most common and worst impurities, and that it contains manganese, the most prized of all the elements found in connection with iron. For these reasons the Cumberland ore is sought by manufacturers at a distance, to mix with softer ores and improve their quality, and is now exported from this State for that purpose."

It seems that this Iron Mine Hill ore was employed in 1703, mixed with the hemitite of Cranston, R. I., for the casting of cannon. The work was done at Cumberland, and, in part at least, "the cannon used in the celebrated Louisburg expedition, in 1745," were cast from these ores. The manufacture was abandoned in 1763, owing to an explosion of the furnace, by which the proprietor was killed.

During the administration of John Adams the same ores were also used for the manufacture of cannon. It seems that the Cumberland (Iron Mine Hill) ore was employed in the manufacture of charcoal iron at Easton, Chelmsford, and Walpole, Mass., as late as 1834. "The Cumberland ore, mixed with equal quantities of Cranston hematite or bog ore, produced, for a long period, a charcoal iron unsurpassed in this country. . . . The Cumberland ore contains an uncertain percentage of titanium, which, while it improves its quality, helps make it refractory. The ore is porphyritic, the magnetic oxide being associated with earthy minerals, principally feldspar and serpentine." It would seem that in 1869, and before, the ore was largely shipped to Pennsylvania to mix with other ores.

A letter of Professor R. H. Thurston, published in this report, states: "The Cumberland iron ore is of the kind known to mineralogists as 'ilmenite;' among mettalurgists as 'titaniferous magnetic ore,' and iron manufacturers, on account of its peculiar value for producing steel, would term it a 'steel ore.' . . . The Cumberland ore is conveniently located and of inexhaustible extent; it is perfectly free from noxious elements, though somewhat refractory; it will furnish a very strong iron or a most excellent steel; it can be smelted within the State at a profit; it can be made directly into steel at a much greater profit; steel made from it will bring the highest prices in the market."

Professor Thurston states that the mean of various analyses made of this ore is about as follows:

SiO_2	22.87
Al_2O_3	10.64
Fe^2O^3 / FeO	44.88
MnO	2.05
CaO	0.65
MgO	5.67
TiO	9.99
Zn	0.20
H_2O and loss	3.05
Total	100.00

The ore on one side of the hill, where it has been most extensively quarried, shows a dark, somewhat resinous groundmass, holding large striated crystals of feldspar. The resinous lustre and greenish-yellow color, as observed under the lens, are caused by the presence of olivine. The olivine becomes more strongly marked on the slightly weathered surfaces seen on the faces of the quarry. Under a lens of high power, the olivine shows clearly on the fresh fractures. The olivine in weathering decomposes to a yellowish and reddish-brown ferruginous powder, leaving the other constituent of the rock, the magnetite, well marked. The magnetite decomposes more slowly, and forms an incoherent mass after the decay of the olivine. The rock gelatinizes with hydrochloric acid, and yields a titanium reaction. A fragment allowed to stand a day or two in weak hydrochloric acid yielded gelatinous silica copiously.

A section made with special reference to the feldspar crystals shows large porphyritic crystals of the latter enclosed in a mass of magnetite and olivine.

The magnetite forms irregular, more or less connected masses, making a sort of sponge-like structure. Its rounded and irregular cavities are filled with olivine, which also occupies the interspaces between the magnetite masses. The olivine is in rounded forms, which sometimes show one or more crystal planes. It is cut through by numerous fissures, that usually show a ferruginous staining along their sides. The olivine also holds grains of the magnetite. Except the fissuring and ferruginous staining, the olivine is comparatively clear, and shows little signs of alteration.

The plagioclase feldspar shows well-marked lines of cleavage and fracture, and is somewhat kaolinized along these lines. It contains a few irregular flakes of biotite together with grains of olivine and magnetite.

The order of crystallization appears to have been, first the magnetite, then the olivine, and lastly the feldspar.

This rock is similar to the celebrated iron ore of Taberg, Sweden, as described by A. Sjören in the Geologiska Föreningens Forhandlingar (1876, III. 42-62; see also Neues Jahrbuch für Mineralogie, 1876, 434, 435.) The Taberg rock has been worked as an iron ore for over three hundred years. This Swedish ore is called by Sjören "magnetite-olivinite."

The feldspar is confined to the peridotite found on one side of the hill, where the peridotite passes into a compact greenish-black rock, showing patches of serpentine and grains of magnetite. From this fact it seems necessary to regard the feldspar as abnormal and local in the rock, which in general is composed of olivine and magnetite or their alterative products.

The structure remains about the same in the non-feldspathic portions as it is in those before mentioned as holding feldspar. But the olivine is entirely changed to a greenish serpentine which shows beautiful fibrous polarization. The serpentine retains the form of the olivine grains, their inclusions, and the network of fissures before mentioned. In some of the sections considerable carbonate was seen, presumably dolomite. In one section part of the olivine grains, especially towards their interior, remained unchanged, but on their edges they were altered to serpentine. Another change was observed here: the formation of secondary crystals of irregular outline that belong probably to actinolite. Some are elongated and narrow; other are short and broad, traversed, by cleavage planes. They evidently belong to the monoclinic system.

The origin of this rock could not be told from its field relations, as its contact with any other rock could

not be found. Since the only method in which its origin can be absolutely shown cannot be used without expensive excavation, it only remains to give the probabilities so far as ascertainable from the mass itself. Such microscopic characters and mineral association have been, so far as we know, only found in eruptive rocks when the origin of such rocks has been studied with sufficient care to determine it. Hence we must conclude it is most probable that this mass is eruptive also, until found to be otherwise.

It closely resembles in structure and composition some of the meteorites, except that its iron is oxidized and not in a native state—a resemblance which for others of the peridotites has long been pointed out. It is rocks of this character, as has been suggested by others, that give us the most probable clew to the interior composition and structure of the earth.

The rock in the field shows, to our mind, no signs of structural planes that should be referred to sedimentation. On one side the rock is massive and jointed, and on the other it is jointed in fine parallel planes. This portion of the rock is more highly metamorphosed than the other, and, as is usual in highly altered eruptive rocks, joints parallel to certain lines of pressure occur. The writer has seen this structure in many rocks that were indisputably eruptive, forming well marked dikes in other rocks.

A rod away from the main mass of the iron ore, near one end, some serpentine appears that cannot be directly connected with the other peridotite. Microscopically its characters and structure are the same as the main rock, and there is no reason to regard it as distinct. The rock nearest to the peridotite is a mica schist some hundred feet away. It shows no characters that would indicate the transition of the ore into it.

The locality was visited by the writer in October last, in company with Professor A. S. Packard, Jr., of Brown University, and Mr. T. S. Battey, of the Friends' School, Providence, R. I. To the latter gentleman I am especially indebted for a copy of the paper of the Rhode Island Society before mentioned, and for other favors.

This examination may serve as an illustration of the aid that microscopical lithology may be to the practical side of life, since now, for the first time since this rock has been worked, can the ironmaster who wishes to use it approach understandingly the metallurgical problems it presents; whether he desires to employ the rock as a whole, or to concentrate the magnetite first.

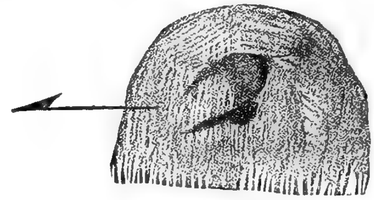
In direct-vision spectroscopes the number of prisms involves a considerable loss of light. M. Zenger now uses a liquid prism of ordinary form, having attached on its anterior plane a quartz prism of the same refringent angle, but arranged in opposite direction. The posterior face of the liquid prism carries a plane parallel plate. The rays fall normally on the quartz. The loss of light is by this arrangement reduced to a minimum. The spectra obtained are very intense, and the lines are well defined. A single parallelepiped of the kind decomposes the D line to the naked eye, and with a small Galilean telescope, magnifying five times, one can distinguish the difference of breadth of the two lines, and easily see the extreme red and ultra-violet rays, though there are only two prisms of 60 degrees.

M. Poliakoff, the distinguished Russian naturalist, has examined a horse presented by Colonel Prejvalsky to the St. Petersburg Academy, and decides it to be a new species, which he has named *Equus Przewalskii*. A translation of his memoir appears in the "Annals of Natural History," and from this it appears that the new representative of the family of undivided-hoofed mammals is in some respects intermediate between our domestic horse and the wild ass, but it differs from the asinine genus in having four callosities, one on each leg. In the form of skull, absence of dorsal stripe, and other particulars it resembles the domestic horse. This newly-recorded animal is indigenous to the plains and deserts of Central Asia, and has not hitherto fallen under the dominion of man.

COMET (*b*), 1881.

We continue the interesting series of sketches of this comet, made by Professor Edward S. Holden with the 15-inch equatorial at the Washburn Observatory, Madison, Wisconsin.

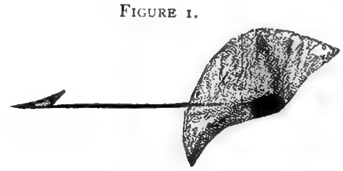
July 13, 1881.



9h. 30m.

FIGURE 1.

July 14, 1881.



10h. 20m.

FIGURE 2.
Moonlight.

July 17, 1881.

10h. 45m.

FIGURE 3.

July 18, 1881.

9h. 30m.
11h. 0m.

FIGURE 4.

The nucleus is DOUBLE (it has not been previously) $p = 275^\circ \pm$, $s = 1''.5$, with a dark space between the parts.

DO WE SEE NON-LUMINOUS BODIES BY REFLECTED LIGHT?

By A. G. Gaines, Pres. St. Lawrence University, Canton, N. Y.

All who have treated this subject have answered the above question with an unequivocal *yes*.

It may appear presumptuous to call the answer in question. Nevertheless, while reflecting recently on some of the peculiar facts of light and vision the thought came to me to doubt this universally accepted proposition; and now I wish to express my more confirmed doubts, and give some reasons for thinking we must revise our views on this point to some extent.

What I now hold is that neither ***transmitted*** nor ***reflected*** light reveal to us in vision either the body transmitting or the body reflecting, but that ***radiant*** light does reveal in vision the radiant body, and that the light by which any non-luminous body is visible is essentially of the nature of radiant light, and is properly to be so called. Paradoxical as these views may seem on bare statement, I think that a little consideration of the facts involved will soon convince us that they must be accepted as true, and show us that the present paradox is due to the illusions of an erroneous point of view.

It is a known and universally accepted truth that ***transmitted*** light does not reveal the transmitting medium. It may be refracted, little or much, but when it reaches the eye it reveals, not the refracting medium, but the body from which it was emitted. The refracting or transmitting body may be ***visible***, but is not visible by transmitted light. Were it perfectly transparent, that is, were it to transmit ***all*** the light coming to it, it would be invisible. This is no new truth, but one universally held and taught; and thus far we are all agreed.

Now let us attend to *reflected* light. As we attend to it we shall learn that reflected light does not reveal the reflector but the body emitting it. If bodies are seen by reflected light, they should be more clearly seen in proportion as they reflect more perfectly the light falling on them. The facts are exactly contradictory to this. In proportion as any given surface is a good reflector it is to that degree invisible, and when a surface becomes a perfect reflector it becomes invisible. Can it then be true that bodies are seen by *reflected* light when it is palpably true that the better they reflect light the less visible they are? The reflected light makes visible the body emitting it, not the reflecting body, and it results that, in studying the stars, the astronomer uses nearly indifferently a reflecting or a refracting telescope. Plainly then, we would say, it is not by reflected light that bodies are visible. This conclusion cannot be escaped by any conjectures as to the extent and form of the reflecting surface. The minutest surface reflecting the sunlight gives a brilliant, dazzling star, not a revelation of itself. Curved, convex or concave, or variously warped surfaces give only images variously enlarged or diminished, or variously distorted, of the body emitting the light, and not at all of the surfaces reflecting it. If the microscope be applied to the surface, the facts are still found to be as above stated. No theory of minute reflecting surfaces changes any of these facts, unless it were imagined that a surface might be so small as to *decompose* the light falling on it, but this result would be destructive of the theory now objected against. Thus it appears from all the facts stated and referred to that the proof is conclusive that, in no case is a body seen as such by the light it *reflects*.

If, now, we go on to inquire as to the light by which bodies are seen, we may find some good reasons for believing it to be essentially *radiant light*, even when proceeding from non-luminous bodies. Note, then, that it is the peculiarity of radiant light that it is emitted in straight lines in every possible direction from every luminous point. The light, hence, by which such a point or body is seen is *divergent* light, and the office of the optical apparatus is to bring it to a focus on the retina. It is not possible for a single point (the minimum of visible surface,) in any reflecting surface to reflect light in every direction; and for light thus to proceed in every direction from a luminous point is the distinguishing characteristic of radiant light. What thus characterizes the light of what are called luminous bodies will be found to characterize the light by which all non-luminous bodies are visible. From every point of any such visible body the light proceeds in every possible direction; whence we note that every such point is a point of dispersion or radiation, and not a point of reflection. Here, as we learned in the case of luminous bodies, the light by which any ordinary non-luminous body (so-called) is seen is *divergent*, and the office of the optical apparatus is to bring it to a focus on the retina. This brings before us the perfect similarity of the conditions under which luminous and non-luminous bodies are seen; and which seem to compel us, hence, to regard the light by which non-luminous bodies are seen as having essentially the same qualities and relations as radiant light.

If, now, we seek to know how this can be explained, seeing that non-luminous bodies are not original sources of light, I think we may find a nearly perfect analogy in the facts of heat that may afford us much help. We are tolerably familiar with radiant and reflected heat. The heat which a body reflects follows all the laws of reflected light, and has this peculiarity, that it does not change the temperature of the reflecting surface. For the rest, the heat which falls on a body, and, as it is said, is *absorbed* by it, raises the temperature of the absorbing body, and immediately said body begins to *radiate* heat, and the heat thus radiated shows all the essential characteristics of radiant heat. What we wish to have particularly noted here is, that this *heat* has been all along said to be *radiated, not reflected*. By the principle of the correlation of forces the heat which is said to be absorbed is transformed first into increased molecular activity in the absorbing body, and then again transformed into what is emitted as radiant heat; and this emission is in straight lines in every direction from every point in the surface of the body radiating. All this is plain, and in perfect agreement with the accepted theory of heat. We have now only to apply these facts and principles, by analogy, to light, and we may obtain an equally plain and consistent theory of light as to visible bodies.

We have already called attention to the fact that the light which a surface *reflects* does not reveal that surface. The light by which any non-luminous body is seen is emitted, let us say, *radiated*, from every point of its surface. This may now be explained by supposing the light (luminous energy) received by such a body as in some degree or manner absorbed by the superficial particles of the body, and then radiated from every such particle as a centre, analogous to what we believe of heat. The light thus taken in appears to be always *decomposed*, with numberless variations of results; so that the light emitted or radiated is always of a different *color* from that received. This difference of color affords us another contrast between the light by which bodies are seen and reflected light; this last being always of the same color as the incident light. In making this statement we have in mind the fact that the same surface may both reflect and radiate light; and that, hence, in each case we must take care not to confound the one with the other in making our observations. When this caution is observed, the statement above concerning the color of reflected light will not, we think, be called in question.

The explanation, then, that I would offer is, that the light which falls on non-luminous bodies (so far as it is not reflected) is somehow absorbed by them, decomposed, and then radiated, at least in part, that the body is visible by this *radiated* light, and not at all by that light which it reflects. In these actions and reactions between the luminous energy falling on a non-luminous body and the body itself, we think it not improbable that there are some correlations of force; and that these may be essential parts of the change that enables the light radiated to make visible the non-luminous body.

If the views presented in this paper be allowed, they enable us to place the facts of phosphorescence, and may be of fluorescence, in harmony with the action on light of ordinary non-luminous bodies; and differing from these chiefly, if not wholly, in *degree* only. And is it not true that this so-called phosphorescence is possessed in some degree by every visible body? We do not now speak of cases of slow combustion, like exposed phosphorus, but those continuing to emit light for a time after being cut off from extraneous light, like snow and the diamond. We would look for the explanation of these greater degrees in phosphorescence in the power of the bodies exhibiting it to absorb and decompose light more deeply, and then more tardily radiate the luminous energy, than is true of non-luminous bodies generally.

It may be proper here to notice the facts of *iridescence*, with which our theme may have some interesting connections. Inasmuch as the facts of iridescence are explained by the interference of the luminous waves, caused by the reflection of light from very thin laminæ, it might be thought the same explanation would apply to *decomposition* of light by ordinary non-luminous bodies. We think the facts in the two cases so different that the same explanation is not applicable to both. In the first place, the facts of iridescence agree with the usual characteristics of *reflected* light; while, on the contrary, we have noted in this paper that the facts in the case of ordinary visible bodies do not so agree. And, in the second place, the results of the decomposition of light in iridescence

agree with the results obtained by prismatic decomposition; while the results in the other case do not. We think it would be correct to say that iridescence does not reveal non-luminous bodies in the same way, nor with the same certitude, as that light reveals them by which they are ordinarily visible. In making this last statement we have in mind the fact that the iridescent surface, in addition to its iridescence, also emits or radiates light in the same manner as ordinary visible bodies; and that these two facts are not to be confounded in our observations and reasonings. Without pursuing the subject further into details, these are some of the reasons why we think the facts of iridescence are not inconsistent with the main doctrine of this paper.

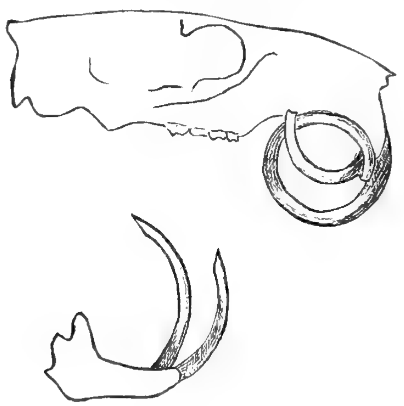
We conclude then, by reason of the facts and relations to which we have now called attention, we cannot believe that it is correct to say that non-luminous bodies are seen by *reflected light*; and we offer the suggestion that the light by which such bodies are seen should fairly and properly be called *radiant light*, as manifesting all the essential qualities of such light.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

In an article on overgrown teeth of Fiber Zibethicus (which by a singular typographical error is printed Fiber Wibethicus) in "SCIENCE" for July 16th, the writer describes a not very uncommon phenomenon among rodents to which I can add an interesting example.



The inclosed drawing represents a similar case, being a woodchuck (Arctomys monax); it will be noticed that one of the upper teeth has grown far enough to form a semicircle while the other upper incisor has described a somewhat larger curve and finally thrust itself through the first and then continued to form a *complete circle*, as will be evident from the figure. This specimen was mounted here (with one other similar but not so extreme a case) and is now in the Museum of Comparative Zoology at Cambridge.

F. W. STAEBNER.

July 20, 1881.

WARD'S NATURAL SCIENCE ESTABLISHMENT, Rochester, N. Y.

COMET (c) 1881.

To the Editor of "SCIENCE."

The comet discovered by Mr. Schaeberle at Ann Arbor, July 13, promises to become a very interesting object, not only because it will soon be visible to the naked eye, but also because its orbit shows great similarity to the great comet of 1337, as may be seen by the following comparison:

	1881 (Stone)	1337 (Hind)	1337 (Lanzier)
Distance of perihelion from node	122° 30′	108° 44′	90° 41′
Longitude of node	98 43	99 6	93 1
Inclination	141 35	137 6	139 32
Logarithm perihelion distance	9.7959	9.97	9.92

The difference between the orbits of the two comets is perhaps not greater than the uncertainty of that of 1337. The latter was first seen in China on the 26th of June, and afterwards in Europe on the 24th of October.

Schaeberle's comet has been observed here on a number of mornings, and its increase in brightness has been quite perceptible. This morning the tail was very apparent, the sky was very cloudy, or I presume it would have been visible to the naked eye. It ought to be quite plainly visible at any rate before the end of this week. It will be at perihelion and nearest the earth about the 20th of August, and will remain at approximately the same distance from us for a week or more. A few days before that time its right ascension will have become equal to the sun, so that when at its greatest brilliancy it will be visible in the evening. While it will undoubtedly become a magnificent object, it will not probably equal the great comet now receding from us.

ORMOND STONE.

MT. LOOKOUT, O., *July* 25, 1881.

ASTRONOMICAL NOTE.

WASHBURN OBSERVATORY, UNIVERSITY OF WISCONSIN, MADISON, WIS., July 17, 1881.

To the Editor of "SCIENCE."

Among the new red stars found here, the following is by far the finest and may be of interest:

Anon. 9 mag. R. A. $1^h\ 48^m\ 45^s$; Dec. = + 58° 40′.2 1880.0.

EDWARD S. HOLDEN.

ADULTERATION OF SUGAR.

To the Editor of "SCIENCE."

DEAR SIR—In the leading editorial of "SCIENCE" of June 18, you speak of the different results obtained by Prof. Leeds and myself of examination of commercial sugars and syrups for glucose and grape sugar. I can only take exception to one statement contained therein, *i. e.*, the one which intimates that these different results form the theme of a scientific controversy. Since the reception of your letter I have renewed my inquiries for statistics, and can now say that I do not believe my estimates of the quantities made in the United States are very wide of the truth. Dealers and manufacturers are extremely reticent on the whole subject, and it is only by hard work and often indirection, that one can get at the truth. In your own city, New York, there is a large establishment for making "New Process Sugar," the Manhattan Refining Company, unless it has lately changed its name. Yet a prominent New York chemist stated publicly, and published over his own signature, that he had made diligent search for this establishment, and it could not be found. At the same time, to my personal knowledge, a western firm had just received a large consignment of "New Process Sugar" from this firm.

At the Boston meeting of the A. A. A. S., I stated on the strength of this personal knowledge that I believed the Manhattan Company was no myth. This statement was published in the Boston and New York papers, and was seen by the proprietors of the Manhattan Company. They wrote to assure me that I was right in my statement, sending me at the same time samples of their different sugar for analysis.

Within the past year the mixing of sugars has largely increased, and is now carried on in New York, in Buffalo, in Chicago, and at other points. A prominent

sugar dealer has just told me "some of these establishments turn out five hundred barrels daily."

From the best information I can get, I would place the daily yield of mixed sugars at 1500 to 2000 barrels. This, remember, is only a careful estimate from all the data I can get. The process is increasing just as fast as *dry white* grape sugar can be made.

With regard to table syrup, I can only reiterate my former statements. In response to a recent inquiry a prominent dealer has just written me as follows:

"My observation leads me to believe that fully four-fifths of all the syrup sold throughout the western and northwestern States and Territories are composed of glucose, with enough cane syrup or molasses added to give it the color most desirable to the buyer or consumer."

I have no accurate information respecting the eastern trade. I am not without a suspicion, however, from the appearance of some syrups which I have lately seen on Boston tables, that the beautiful syrup made from the corn of our western prairies, has invaded the very stronghold of the dirty refuse syrups of the sugar refineries.

Respectfully,

H. W. WILEY.

BOSTON, *July* 27, 1881.

WATER AS FUEL.

To the Editor of "SCIENCE":

I am interested in the paper of Dr. Rachel, in "SCIENCE," July 9, on the use of water in combustion.

The subject is a most important one; and I regret a mistake which a reader might fall into from an inadvertency, I suppose, in not more clearly distinguishing between the degrees of temperature at which the transfer of oxygen takes place from the hydrogen of the water to the carbon set free by the dissociation of the naphtha, and the number of heat units set free or absorbed by such transfer, which is a very different thing.

It is not a case of the dissociation of water, but of the dissociation of naphtha, and the *transfer* of oxygen from the hydrogen to the carbon so set free; a carbon which was very loosely held by the hydrogen in naphtha.

It is only a trade of so much carbon for so much hydrogen; the absolute heat of which, according to authorities, are almost exactly equal in complete burning.

But while the trade is thus equal in absolute heat, there is practically an enormous gain; and it is very important to see just what it is.

We get our heat all in hydrogen instead of in carbon, and so avoid the enormous practical losses which attend the usual mode of burning carbon.

We get our heat in hydrogen, which is the easiest of all things to get all of it out by burning, instead of in carbon which is one of the hardest of all things to get it all out of, the difference being much like that between the ease of getting our money out of a bank or out of a lot of debts where from half to three-fourths or nine-tenths is almost always finally lost.

The practical man does not understand very well what it means when he is told that the use of coal under boilers only produces five per cent of its energy in work. It means this: that a series of enormous losses is made in trying to get the heat out of the carbon in burning. An enormous quantity goes off as black smoke and soot, not burned. An enormous quantity goes off as carbonic oxide, giving up only a third of its heat, instead of giving it all up by burning to carbonic acid. An enormous quantity of heat is lost in heating up the great quantity of gases and air, that pass off without being fully burned, which prevents a high temperature. An enormous quantity of heat is carried up the chimney, because a little coat of soot and ashes on the boiler does not allow it to go into the boiler fast enough while passing so rapidly to the chimney. Is it any wonder that an enormous revolution is to be made by a mode of burning where water is used not for any energy to be got out of it, but by which the heat is taken from carbon, and put into hydrogen, where it all flashes into usable form the instant the air can reach it? Where the whole heat is liberated at one point where it is wanted, instead of in a long, imperfect flame? Where there is not a particle of soot or dust to tarnish the boiler and prevent the heat from passing into it wherever it strikes? And where the heat is nearly all in that low form of invisible radiance which is best suited to radiate on to, and be absorbed by, the boiler without waiting for the heated gases to actually strike it to give up their heat by slow convection?

It is a fact well known in the use of the alcohol and gas flames, that if you want them for heating alone you want the non-luminous flame only; because with a luminous flame a large part of the heat is in the form of light, which is not so readily absorbed by substances as heat, as it is with the non-luminous flame; and another large part is held by the gases as convective heat, and cannot so freely pass into substances by radiation, as in the radiation from the non-luminous flame.

Thus the revolution in combustion by the use of water consists in transferring the heat from carbon to hydrogen, by which all the great losses of burning are avoided, and the entire heat is obtained clean, without smoke, at the exact point desired, and in the form which takes right hold of the work without loss of time or energy.

These things are of an incalculable value; very much better than any mysterious supposed increase of heat from the water itself.

Though it is important to form a non-luminous gas by means of water to utilize the carbon, yet the water so added takes up a share of the heat to raise its temperature along with the other gases, and so reduces the temperature in proportion to its quantity.

So, if we need high heats we must use the least quantity of water, which will turn all the carbon into a suitable gas.

If we add water enough to make the carbon of 100 pounds of naphtha into carbonic acid, it will take 250 pounds of water, if we assume that the naphtha averages a composition of $C_6 H_{14}$, containing 84 pounds of carbon and 16 pounds of hydrogen; and the result would be 30 pounds of hydrogen and 310 pounds of carbonic acid. This would require 1530 pounds of air to burn it, and produce 1880 pounds of gas, of which over one-seventh would be due to the water, and the temperature would be less than $\frac{5}{7}$ of what it would have been without, if an equally perfect burning could have been secured without; because the water, as steam, has about twice the capacity for heat, as the other gases.

If we only add enough water to make all the carbon into carbonic oxide, it will take only half as much water; 125 pounds to 100 pounds of naphtha making about one-thirteenth of the gas after burning; and reducing the temperature only one-seventh.

But if we use only 36 pounds of water to 100 pounds of naphtha, one gallon of water to four gallons of naphtha, the gases may be something like this:

Marsh gas, CH_4, 80 pounds.
Carbonic oxide, CO, 56 pounds.

which with 1530 pounds of air (20,000 cubic feet) to burn it, will make 1666 pounds of gas; and the water added will only reduce the temperature one-twenty-fifth part, instead of so much as before.

In each case the air to burn it, and the units of heat produced, will be the same; but the temperature will vary with the proportion of water added, and, also, with the quantity of unburned air passing through.

One of the great advantages of the water process is the condition of blast with which the fuel unites with the air, by which the thorough mixture and burning is secured. And it is very important that the quantity of air going into the furnace be regulated so as to furnish only about

the right amount. All that is more or less than the exact amount required, reduces the temperature so much.

In ordinary furnaces, it has been estimated that much more than half the heat is lost by this one item alone. If the air passes freely in above the coal, twice as much goes in as is burned; if it all passes in under the grate, then only one-third the heat is given off, as only carbonic oxide escapes.

Probably the advantages with crude petroleum or with coal, of the water process, would be of still greater value.*

SAMUEL J. WALLACE.

WASHINGTON, D. C.

BOOKS RECEIVED.

OBSERVATIONS OF DOUBLE STARS made at the United States Naval Observatory by ASAPH HALL, Professor of Mathematics, U. S. N. Rear Admiral Rogers, U. S. N., Superintendent, Washington, 1881.

In introducing this work Professor Hall gives some very interesting details respecting the methods used in making observations at the Naval Observatory and the condition of the instruments.

He also presents his reasons for undertaking these observations and indicates the scope of the present work.

He states that his regular observations with the 26-inch refractor of the Naval Observatory were begun in the spring of 1875, the instrument at that time being in charge of Professor Simon Newcomb. "Professor Newcomb gradually withdrew from observing with this instrument, which came under my direction sometime in July of the same year. The micrometrical measurements which had been made by Professors Newcomb and Holden were chiefly of the satellites of Uranus and Neptune, and the discussion of these measurements of the two outer satellites of Uranus brought out very clearly what had been indicated before by Von Asten; viz, the existence of a large constant difference in the angles of position measured by Mr. Otto Struve, director of the Imperial Observatory at Pulkowa. As it is our intention to repeat the measurements of the satellites of Uranus and Neptune after a few years, and as it seemed probable that similar differences might exist in the observations of double stars, it occurred to me that the best way of comparing and uniting the observations of different astronomers would be for each one to observe the same double stars at nearly the same time. I wrote to Struve proposing that this should be done, and that he should select the list of stars. In reply he informed me that such a series of observations was already in progress between himself and Baron Dembowski, and after adding to the list of stars a few of greater distances, this list and an account of the proposed work were published by Struve in the "Vierteljahrsschrift der Astronomischen Gesellschaft." Band xi, p. 227.†

It was understood that each observer should avoid all knowledge of the observations of other astronomers, in order that his work might be done independently, and in my own case this rule has been carefully adhered to. But now nearly four years have elapsed since Struve's publication, and it is probable that all the astronomers engaged in this work have collected such a number of observations that the publication of my own results will not influence the independence of theirs. Moreover, the end of the year 1879 seems to be a favorable epoch for publishing my observations of double stars made before 1880, since I hope to make some changes which in the future will enable me to observe under conditions more favorable to accuracy.

I have therefore collected and revised all my observations of double stars, and the results are given in the following pages. In order to make this collection complete I have concluded the few observations made in the year 1863 with the equatorial of 9.6 inches aperture. The whole number of observations is 1614.

It will not be necessary to give any general description of the 26-inch refractor made by Alvan Clark and Sons for the Naval Observatory, since such descriptions can be found in the annual volumes of the Observatory for 1873 and 1874. It will be sufficient to say that the form of the mounting adopted by the makers for this Equatorial is such that the instrument, notwithstanding its great size, is handled with ease; and the harp-shaped piece that supports the polar axis is very convenient when observing near the zenith. Generally the instrument is pointed on a star by means of what are called the "rough circles." These circles are the edges of the hour and declination circles, which were painted white, and then divided by lines of black paint, the hour circle into spaces of ten minutes of time and the declination circle into degrees. This method of pointing is usually accurate enough to find the object, but as the painting was not well done errors as great as 15′ to 20′ could be made in some parts of the rough declination circle. An accurate reading for the position could be made by means of the finely divided circles, but this involves considerable time and trouble. On account of the delay in the observations which would be caused in making the change, and of the natural inertia in getting rid of a poor thing to which one has become accustomed, this defective circle for the declination was used until June, 1879, when the circle was painted white and divided again under the care of Mr. Gardner, the instrument maker of the Observatory. The settings are now much more accurate and give but little trouble, and the saving of time is very great. It is possible that a few cases may be found where, on account of an erroneous setting in declination, I have observed a different object from the one supposed.

The ease and rapidity with which observations can be made with a filar micrometer depend largely on the performance of the driving-clock. The accuracy of the observations also is in a measure dependent on this performance, but patience and skill on the part of the observer will in a good degree make up for a poor performance of the clock. The motive power of our driving-clock comes from a small water-wheel which is driven by water drawn from the Potomac water pipes. At first the water was applied directly to the conical pendulum, but the pressure of the water was so variable that weights attached to an endless cord (Huygen's loop), were placed between the water-wheel and the pendulum by Professor Newcomb. When this had been done the performance of the clock is said to have been tolerable; but in the autumn of 1875 it became very troublesome, and the observer was frequently annoyed by the stopping of the clock. This trouble continued and became worse until July, 1876, when the clock was dismounted by Mr. Gardner and myself. The lower end of the shaft of the conical pendulum had been given a conical shape, and had rested in a conical cup. The friction and heat had been so great that the lower end of this shaft had become very rough and twisted to a gimlet shape, thus stopping the clock. The bearing of the shaft was changed and made of a plane agate surface, the lower end of the shaft being rounded to a slightly curved surface. The friction of the upright shaft of the water-wheel was also diminished by clamping a set of friction wheels to this shaft and letting them play on a horizontal iron surface. The weights on the Huygen's loop were changed for cups carrying shot. With an average pressure of the water, and the machinery well oiled, these

* This superiority of the non-luminous combustion for heating was discovered by Professor Henry. He says: "With this arrangement the light of the flame was increased, while the time of bringing the water to the boiling point was also commensurably increased, thus conclusively showing that the increase of light was at the expense of the diminution of the temperature."

† Mittheilung über unternommene Beobachtungsreihen zur Vergleichung von Mikrometer messungen. 1876, Anfang Juni. OTTO STRUVE.

weights are 7½ and 3½ pounds, but the weights can be varied to suit the resistance and the pressure by changing the shot. Since these changes the performance of the clock has been tolerably good. Still this clock needs much care, and being dependent on an unsteady pressure of water a delay in the observations sometimes occurs. The great length of the telescope, which exposes it to the action of the wind, is also a hindrance to the steady driving of the clock.

The difficulty in turning the dome, of about 42 feet diameter, has increased. This difficulty is caused probably by the uneven settling of the supporting walls, and the bulging of the dome in the direction of the slit. The labor of turning the dome through a revolution is so great that lists of north and south objects are prepared beforehand by the observer in order to avoid as much as possible the turning of the dome.

After some practice, and on becoming familiar with the instrument and micrometer, my manner of observing a double star has been as follows: In order to measure the angle of position the two wires are separated a convenient distance and the stars are placed between them. The position-circle is turned by the hand until both stars appear midway between the wires, and then the circle is read. The light having been taken out of the micrometer, the wires are turned thirty or forty degrees forward and backward several times before the light is thrown on the wires again for the purpose of making the settings of the circle as independent as possible, and another reading is made. Generally four readings of the position-circle are taken. Then this circle is turned 90° from the mean of the readings and the double distance is measured. First the stars are bisected by the wires and the micrometer is read; then the wires are reversed and the stars are bisected again. The wires are then restored to their original position and another double distance is measured. Two such distances are generally observed. An estimated value of the angle of position is always recorded, as well as the sidereal time of the observation, and also an estimate of the weight of the observation. This weight depends simply on the condition of the images of the stars, and the numbers **1** to **5** are used for expressing the weights; **1** denoting a very poor condition of the images, **3** an average condition, and **5** a perfect condition. I have very rarely observed double stars when the images were so poor as to be given the weight **1.** As far as possible I have avoided all knowledge of the angles and distances observed by other astronomers. In my observing-list these quantities are omitted, and no comparison with other observations is made until my own observations of a star are completed. It is possible, therefore, that in some cases my angles may differ by a multiple of a quadrant from those observed elsewhere.

I have omitted observations of color and of magnitude. These observations have now become a specialty, and such observations as I could make would not do much more perhaps than tend to introduce confusion. In the case of stars observed by the Struves, to which most of my observations belong, I have adopted their magnitudes. In most cases these magnitudes are brighter than those of the scale to which I have been accustomed; thus what the Struves would call a 7th or 8th magnitude I would call an 8th or a 9th.

Very few of the observations have been made in the twilight, which offers the best conditions for observing double stars, since, the observer residing at a distance from the observatory, it has not been convenient to do this.

With such a large objective great changes occur in the appearance of the stars during a single night. Generally so long as rapid changes of temperature are going on the performance of the object-glass is not good. But on a few nights of the year, when all the atmospheric conditions are favorable, the performance of the glass is excellent, and its separating power is all that could be desired. Usually ruddy and reddish stars are the most difficult to observe, a result which may be caused by the figure of the objective. After having been in use two years the form of the lenses seemed to have undergone a slight change, and in the beginning of May, 1876, the surfaces of the flint lens were refigured by Mr. Alvan Clark and his son, Mr. Alvan G. Clark. This is the only change that has been made in the objective. On a single occasion water collected between the lenses, and they were taken out, cleaned by Mr. Gardner, and returned to their cell with very little trouble.

Until March, 1878, all the observations were made with my left eye; but having used my eyes very much during the preceding year, and having done a good deal of computing by gaslight, my eyes became weakened. In March, 1878, while observing the stars in the Trapezium of Orion with a field illumination which was very unsteady, my left eye suddenly became bloodshot. After a rest of a week the eye resumed its natural appearance, but on observing again the blood reappeared in the eye. I then began to use my right eye, and have used it since in most of the observations. From a number of trials I think that this change of eyes has produced only a small change in my habit of observing an angle of position. Still it is possible that some systematic difference in the angles may exist on account of this change, as there was at first some awkwardness in observing with my right eye. In all my observations the head of the observer was kept in an upright or natural position."

The elaborate introduction of Professor Hall leaves us little scope for further explanation. We may state, however, that the tables in which these observations are condensed cover nearly 150 folio pages, and will be accepted as a valuable addition to the literature of this subject, which has been much developed of late by the researches of Mr. Burnham and others.

A PARASITE IN ÆGERIA SYRINGÆ. HARR.

By G. H. French, Carbondale, Ill.

When examining the stems of some lilac bushes in my yard, I found a place in the bark of one where it seemed that an Ægerian pupa might soon protrude for the purpose of liberating the moth. Upon cutting away the thin film of bark, I found the end of a chrysalis visible. I carefully cut away the wood, took this out and put it in a jelly dish surrounded with lilac leaves to prevent its drying up, and waited for the imago to come forth. June 5th, a week after the chrysalis had been put into the jelly dish, I saw something among the leaves which I supposed was the expected moth, but which proved to be a hymenopter. I did not know but the insect might be one of the boring bees that often resort to the holes left by Ægerians in which to rear their young, but an examination of both the insect and the empty pupa case assured me that I had a parasite. The chrysalis was certainly that of an Ægerian, having all the characteristic marks of the pupæ of that family; and the insect in emerging from it had gnawed a hole near the end on the left side instead of the usual method of emergence of insects from their own pupa cases. More than this, the specimen was a true Ichneumonide and not a Crabronide as I at first thought it might be. This is the first time I have known of any parasite working in the Ægerians.

I make the parasite to be *Phæogenes Ater*, Cres. It is shining jet black, 40 of an inch long, the antennæ 25 jointed, the first 8 black, the next 4 white and the rest dark brown. The joints of the legs are a little pale.

It is impossible for me to say when the parasite was introduced into its host; but it must have been before it pupated, because the chrysalis when taken from the bush was entire, showing no broken place. That the Ægerian was *Æ. Syringæ*, I have no doubt, I do not know of any other boring in the lilac.—*Papilio.*

TABLES FOR QUALITATIVE CHEMICAL ANALYSIS, with an Introductory Chapter on the Course of Analysis, by Professor HEINRICH WILL of Giessen, Germany. Third American, from the eleventh German, edition. Edited by CHARLES F. HIMES, Ph. D. Henry Carey Baird & Co., Philadelphia, 1881. Price, $1.50.

In this work a series of fourteen tables are presented which will be found of the highest value to the chemical student, and will be the means of saving a large amount of time if used by those engaged in chemical analysis. These tables are compact, but sufficiently explicit, and the summary view of the general course of qualitative analysis, and of the classification of compounds, according to the properties relied upon for their detection, afford a thread, as it were, around which chemical facts may crystalize as they accumulate. These tables appear well adapted for a course of college studies, and their popularity and scientific character is indicated by their general adoption in the German Universities. With such an endorsement, we anticipate a large sale for this book among American students of Chemistry.

TRICHINÆ IN RATS.

In regard to Dr. GLAZIER's belief that rats are not the "headquarters" of trichinæ elaborate, expressed in his official report on trichinæ and trichinosis, the following, taken from the *Geitschrift für unkros Kopische Fleischschan*, is of interest:

Dr. MERKEL, County Physician at *Nuremberg*, *Bavaria*, had asked the Microscopical Society at that city to examine as many rats for trichinæ as they could collect for the purpose. He distributed blanks among the members, which he requested to be filled. Within six months 111 of these troublesome animals had been so examined, with the following result:

Of 40 rats caught at or near abattoirs, 8 (20%) showed trichinæ, while 71, caught on private property, showed 8 (11.27%); total, 111 rats, showed 16 (14.4%).

This would certainly confirm the idea that the neighborhood of those places where swine will devour anything that offers—which they would presumptively do otherwise only after having been fed—rats are more dangerously infected than where the porcine tribe is more regularly cared for.

SOME NEW FACTS ABOUT RABIES.—It is known that M. Pasteur is directing his attention to the subject of rabies. The virus of that disorder of course exists in the saliva, but M. Pasteur has now proved that it does not exist there only. The brain substance also contains it, and, used to inoculate healthy animals, will reproduce the disease as effectively as the saliva. Matter from the medulla oblongata and the frontal portion of one of the brain hemispheres and the liquid of the brain have been thus employed with success. Again, one of the great difficulties in research on rabies arises from the uncertainty of development of the evil after inoculation or a bite, and the long time of incubation. M. Pasteur is now able to communicate the disease surely, and to shorten considerably the time of incubation. His method is to inoculate directly the surface of the brain, having recourse to trepanation, and using as inoculating matter the cerebral substance of a mad dog as pure as possible. In that case the first symptoms of rabies appear infallibly in a week or two, and death ensues in less than three weeks. In these researches, of which we may expect to hear more shortly, M. Pasteur has the co-operation of MM. Chamberlain, Roux and Thuilier.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING JULY 30, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

JULY.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Maximum. Time.	Maximum. Wet Bulb.	Maximum. Time.	Minimum. Dry Bulb.	Minimum. Time.	Minimum. Wet Bulb.	Minimum. Time.	Maxi'm In Sun.
Sunday, 24	29.812	29.862	12 p. m.	29.722	0 a. m.	76.3	68.3	85	3 p. m.	73	3 p. m.	65	5 a. m.	63	5 a. m.	143.
Monday, 25	29.809	29.862	0 a. m.	29.744	6 p. m.	77.6	72.0	86	4 p. m.	76	4 p. m.	71	5 a. m.	67	5 a. m.	138.
Tuesday, 26	29.730	29.748	0 a. m.	29.704	5 p. m.	76.3	70.3	81	3 p. m.	72	10 a. m.	71	12 p. m.	69	12 p. m.	130.
Wednesday, 27	29.726	29.776	9 a. m.	29.698	6 p. m.	71.3	67.0	80	5 p. m.	70	6 p. m.	65	5 a. m.	65	5 a. m.	127.
Thursday, 28	29.751	29.836	12 p. m.	29.702	0 a. m.	71.3	67.0	78	1 p. m.	70	5 p. m.	63	5 a. m.	63	5 a. m.	143.
Friday, 29	30.017	30.102	12 p. m.	29.836	0 a. m.	68.0	67.6	72	3 p. m.	70	3 p. m.	64	5 a. m.	64	1 a. m.	126.
Saturday, 30	30.164	30.190	1 p. m.	30.102	0 a. m.	64.6	64.0	68	11 a. m.	66	3 a. m.	64	12 p. m.	64	12 p. m.	94.

Mean for the week........................ 29.858 inches.
Maximum for the week at 1 pm., July 30th........ 30.190 "
Minimum " at 6 pm., " 27th........ 29.698 "
Range........................ .492 "

		Dry.		Wet.
Mean for the week		72.2 degrees		68.0 degrees.
Maximum for the week	at 4 pm. 25th	86. "	at 4 pm 25th,	76. "
Minimum "	" 5 am. 28th	63. "	at 5 am 28th,	63. "
Range "	"	23. "		13. "

JULY.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE.
Sunday, 24	n. n.w.	s. w.	s.	77	1½	5.50 pm	.537	.597	.628	71	53	72	0	2 cir. cu.	0	-----	-----	-----	--	0
Monday, 25	s. w.	s.	s.	187	4	6.15 pm	.641	.746	.744	76	64	86	2 cir.	7 cir.cu.	2 cir. cu.	-----	-----	-----	--	0
Tuesday, 26	s. s. w.	w. n.w.	w. s. w.	203	4	0.20 am	.731	.585	.666	90	55	77	9 cu.	7 cu. s.	5 cu.	-----	-----	-----	--	1
Wednesday, 27	n. n. w.	n. w.	s.	88	1	4.00 pm	.626	.591	.595	94	68	76	9 cu.	7 cir. cu.	0	-----	-----	-----	--	2
Thursday, 28	n. w.	w. n.w.	s. s. e.	102	1¼	3.00 pm	.536	.614	.668	84	68	85	1 cir.	9 cu.	0	-----	-----	-----	--	0
Friday, 29	n. e.	e.	e.	123	4¼	0.50 pm	.617	.745	.662	100	95	100	10	8 cir. cu.	5 cu.	-----	-----	-----	--	9
Saturday, 30	n. e.	n. e.	n. e.	199	5¾	12.00 m	.583	.583	.596	94	94	100	10	9 cu.	10	0.30 pm 8.00 pm	2 pm. 12 pm.	1.30 4.00	.05 .15	6

Distance traveled during the week........................ 979 miles.
Maximum force........................ 5¾ lbs.

Total amount of water for the week........................ .20 inch.
Duration of rain........................ 5 hours 30 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

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JOHN MICHELS, Editor.

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SATURDAY, AUGUST 13, 1881.

The Proceedings for the past year of the American Association for the Advancement of Science have been distributed to the members; they do honor to the Society by whom they are issued, and hold forth the brightest hopes for its future.

The friends of the Association will learn with satisfaction that the number of members steadily increase, and that the roll of honor now comprises one thousand five hundred and fifty-five names, a glance at the list showing that it represents the intelligence of the United States.

The very laudable objects of the Association are the advancement of Science, which it endeavors to carry into effect by arranging annual meetings of its members, "to promote intercourse between those who are cultivating Science in different parts of America, its Constitution expressing the desire to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness."

It will thus be seen that the leading feature of the Association is *co-operation*, the secret of all success and the keystone of human progress. Perhaps in no country in the world does this necessity for co-operation exist to a greater degree than in the United States, with its vast amount of territory and great area.

Men of education, with minds specially adapted for the highest scientific work, are often isolated from their fellow workers, and thousands who are "cultivating" Science are spread over the States and Territories, silently plodding over problems of vital interest or investigating the great scheme of Creation. Surely an Association which is a bond of union between such a widely dispersed class should be recognized on its merits by those for whose benefit it is established, and we may add, that the only practical sign of appreciation of the advantages offered, is active membership.

The Association at present numbers fifteen hundred members, and has an income of less than six thousand dollars, a sum which is well husbanded and turned to the best advantage by the executive officers of the Association, who are enabled this year to present two handsome volumes to each member, which are alone equivalent in value to the subscription paid.

We desire, however, to see the list of members largely increased, and considering the Association has existed over thirty years, the number should not be less than five thousand, an income would then be at the disposal of the Executive Committee which would enable it to encourage scientific research in a manner worthy of the Association and the cause of human progress which it represents.

We desire also to see the permanent fund of the Association placed on a more substantial footing, and supported by those who can strengthen it from their superabundant wealth, without a financial effort on their part.

We speak within bounds when we assert, that it is a standing scandal and reproach on the *men* of intelligence of the United States, to find that the *single patron* of the "American Association for the Advancement of Science" is a *woman*. Is there no American gentlemen with sufficient chivalry to follow so bright an example? We trust that the meeting of the Association, which will open next week, will not close without at least one response, to the challenge we now make.

ASTRONOMICAL OBSERVATORIES.

By Simon Newcomb.

Among the contributions of public and private munificence to the advance of knowledge, none are more worthy of praise than those which have been devoted to astronomy. Among all the sciences, this is the one which is most completely dependent upon such contributions, because it has the least immediate application to the welfare of the individual. Happily, it is also the science of which the results are best adapted to strike the mind, and it has thus kept a position in public estimation which it could hardly have gained if it had depended for success solely upon its application to the practical problems of life. That the means which have been devoted to its prosecution have not always been expended in a manner which we now see would have been the best, is to be expected from the very nature of the case. Indeed, a large portion of the labor spent in any kind of scientific research is, in a certain sense, wasted, because the very knowledge which shows us how we might have done better has been gained through a long series of fruitless trials. But it is due both to ourselves and the patrons of astronomy that as soon as any knowledge bearing upon the question of

the past application of money to the advance of science is obtained, use should be made of it to point out the mistakes of the past and the lessons for the future. It is now patent to all who have made a wide study of the subject that large amounts have been either wasted or applied in ways not the most effective in the erection and outfit of astronomical observatories. Since Tycho Brahe built his great establishment at Uraniburg, astronomical research has been associated in the public mind with lofty observatories and great telescopes. Whenever a monarch has desired to associate his name with science, he has designed an observatory proportional to the magnitude of his ambition, fitted it out with instruments on a corresponding scale, and then rested in serene satisfaction. If we measure greatness by cubic yards, then Peter the Great and "Le Grand Monarque" were the founders of two of the greatest observatories ever built. That of St. Petersburg was completed in 1725, the year of Peter's death, and was an edifice of two hundred and twenty-five feet front, with central towers one hundred and forty feet high. It had three tiers of galleries on the outside for observation, and was supplied with nearly every instrument known to the astronomers of the time, without reference to the practicability of finding observers to use them. It was nearly destroyed by fire in 1747, but was partially rebuilt, and now forms part of the building occupied by the Imperial Academy of Sciences. The Paris Observatory, built half a century earlier, still stands, its massive walls and arched ceilings reminding one rather of a fortress than of an astronomical institution.

Notwithstanding the magnificence of these structures, they have had little essential connection with the progress of astronomy. It is true that the work done at both establishments takes a prominent place in the history of science, but most of it could have been done equally well under wooden sheds erected for the protection of the instruments from the weather. In recent times, the St. Petersburg Observatory has been found so unsuitable for its purpose that no observation of real value can be made, and its existence has been nearly forgotten. The great building at Paris, though associated with a series of astronomical researches second to none in the world, has really served scarcely any other purpose than those of a physical laboratory, store-house and offices. The more important observations have always been made in the surrounding garden, or in inexpensive wings or other structures erected for the purpose.

With these establishments it will be instructive to compare the Greenwich Observatory. The latter has never won the title of great. It was originally established on the most modest scale, for the special purpose of making such observations as would conduce to the determination of the longitude at sea. Although it has now entered upon its third century, no attempt has ever been made to reconstruct it on a grand scale. Whenever any part of it was found insufficient for its purpose, new rooms were built for the special object in view, and thus it has been growing from the beginning by a process as natural and simple as that of the growth of a tree. Even now, the money value of its structure is less than that of several other public observatories, although it eclipses them all in the results of its work. Haeckel lays it down as a general law of research that the amount of original investigation actually prosecuted by a scientific institution is inversely proportional to its magnitude. Although this may be regarded as a humorous exaggeration, it teaches what the history of science shows to be a valuable lesson.

A glance at the number and work of the astronomical observatories of the present time will show how great a waste of means has been suffered in their erection and management. The last volume of the "American Ephemeris" contains a list of nearly 150 observatories, supposed to be, or to have recently been, in a state of "astronomical activity." The number omitted because they have lain inactive it is impossible to estimate; but it is not unlikely that, in this country at least, they are as numerous as those retained. It is safe to say that nearly everything of considerable value which has been done by all these establishments could have been better done by two or three well-organized observatories in each of the principal civilized countries. Indeed, if we leave out of account local benefits, such as the distribution of time, the instruction of students, and the entertainment of the public, it will be found that nearly all the astronomical researches of really permanent value have been made at a very small number of these institutions. The most useful branch of astronomy has hitherto been that which, treating of the positions and motions of the heavenly bodies, is practically applied to the determination of geographical positions on land and at sea. The Greenwich Observatory has, during the past century, been so far the largest contributor in this direction as to give rise to the remark that, if this branch of astronomy were entirely lost, it could be reconstructed from the Greenwich observations alone. During the past twenty years, the four observatories at Greenwich, Pulkowa, Paris and Washington have been so far the largest contributors to what we may call geometrical astronomy that, in this particular direction, the work of the hundred others, in the northern hemisphere at least, can be regarded only as subsidiary.

This remark, it will be understood, applies only to that special branch of astronomy which treats of the positions and motions of the heavenly bodies. The other great branch of the science treats of the aspect and physical constitution of these bodies. It dates from the invention of the telescope, because, without this instrument and its accessories, no detailed study of the heavenly bodies is possible. The field open to the telescope has, during the last twenty years, been immensely widened by the introduction of the spectroscope, the ultimate results of which it is scarcely possible to appreciate. Photography has recently been introduced as an accessory to both instruments; but this is not so much an independent instrument of research as a means of recording the results of the spectroscope and telescope. To this branch of the science a great number of observatories, public and private, have duly contributed, but, as we shall presently see, the ratio of results to means is far less than it would have been had their work all been done on a well-organized system.

Nearly all great public observatories have hitherto been constructed for the purpose of pursuing the first branch of the science,—that which concerns itself, so to speak, with the geometry of the heavens. This was naturally the practice before the spectroscope opened up so new and rich a field. Even now, there is one sound reason for adhering to this practice—namely, that physical investigations, however made, must be the work of individuals, rather than of establishments. There is no need of a great and expensive institution for the prosecution of spectroscopic observations. The man of genius with imperfect instruments will outdo the man of routine in the greatest building, with the most perfect appliances that wealth can supply. The combination of qualities which insures success in such endeavors is so rare that it is never safe to count upon securing it. Hence, even now, a great observatory for the prosecution of physical research would be a somewhat hazardous experiment, unless the work it was to do were well mapped out beforehand.

Considering the great mass of observatories devoted to geometrical astronomy, the first thing to strike the professional student of their work is their want of means for a really useful and long-continued activity; and this notwithstanding that their instrumental equipment may be all that could be required. The reason is that their founders have not sufficiently taken into account the fact that the support of astronomers and the publication of observations is necessary to the usefulness of such an

establishment, and requires a much larger endowment than the mere outfit of the building. Let us take, for instance, that omnipresent and most useful instrument, the meridian circle. Four or five of these instruments, of moderate size, located in good climates, properly manned, under skillful superintendence, working in co-operation with each other, would do everything necessary for the department of research to which they are applicable, and a great deal more than is to be expected from all the meridian circles of the world, under the conditions in which they are actually placed. They could, within the first five years, make several independent determinations of the fundamental data of astronomy, including the positions and motions of several hundred of the brighter fixed stars. In five years more, they could extend their activity so as to fix the position of every star in the heavens visible to the naked eye; and, during the ten years following, could prepare such a catalogue of telescopic stars as there is no prospect of our seeing during the next half-century.

There are probably not less than twenty meridian circles in this country alone, most of them antiquated, it is true, yet, so far as average size and cost are concerned, amply sufficient for the work in question. How many there may be in other countries it is impossible to estimate, but probably fifty or upward, and the number is everywhere constantly increasing. Should we seek out what they are doing, we should probably find half of them rusting in idleness upon their pivots. With others, some industrious professor or student would be found making, unaided, a series of observations to be left among the records of the establishment, or immured in the pages of the "Astronomische Nachrichten," with small chance in either case of ever being used. We may be sure that the solitary observer will soon find something else to do, and leave the instrument once more in idleness. Others we should find employed in the occasional instruction of students, a costly instrument being used where a rough and cheap one, which the student could take to pieces and investigate at pleasure, would answer a far better purpose. Yet others we should find used in distributing time to the neighboring cities or states, or regulating chronometers for the shipping of a port. I dare not guess how many we should find engaged in work really requiring an instrument of the finest class, and gaining results which are to contribute to the astronomy of the future, but in our own country there would hardly be more than three.

The general cause of this state of things lies upon the surface. It is as true in astronomy as in any other department of human affairs that the best results can be attained only by a careful adaptation of means to ends. Failures have arisen, not from the intervention of any active opposing agency, but because observatories have been founded without a clear conception of the object to be attained, and therefore without the best adaptation of means to ends. To build an observatory before knowing what it is going to do is much like designing a machine shop and putting in a large collection of improved tools and machinery before concluding what the shop is to make, and what are the conditions of the market open to its products. Some hints on the considerations which should come into play in the erection of any new observatory may not be out of place, as pointing out the remedy for the evils we have described.

Heretofore, the practice has usually been first to decide upon the observatory, and to plan the building; next, to provide instruments; and lastly to select an astronomer, and, with his advice, to decide what direction the activities of the establishment should take. This order of proceeding should be reversed. The first thing to be done is to decide what the observatory shall be built to do. The future astronomer would, of course, have a controlling voice in this decision, and should, therefore, be selected in advance. One thing which it is especially important to decide is to which of the two great divisions of astronomical research attention shall principally be directed. If the prosecution of geometrical astronomy is kept in view, the conditions of advance in that department of the science must be kept in mind. The public is too apt to associate astronomy with looking through a telescope. That some of the greatest astronomers of modern times, such as Kepler, Newton, Hansen, Laplace and Leverrier, scarcely ever looked through a telescope as astronomers, is not generally understood. For two thousand years, astronomy has furnished the great geometers of the world with many of their profoundest problems, and thus has advanced hand in hand with mathematics. It borrows its fundamental data from observation, but the elaboration and development of its results taxes the powers of the mathematical investigator. The work of making the necessary observations is so much easier than that of developing the mathematical theories to which they give rise, that the latter is comparatively neglected alongside the former. It is lamentable to see what a collection of unused observations are found in the pages of scientific periodicals, to say nothing of those which have remained unpublished in the records of observatories. Under these circumstances, it is not worth while to found any more observatories for the prosecution of geometrical astronomy, except under special conditions. Among these conditions we may enumerate the following:

First. The institution should have such an endowment as to secure the continuous services of two or three observers, and to publish at least the results of their observations in a condensed form.

Second. The instrument should be of the finest class, but not necessarily of large size. This is not a difficult condition to fulfill, since such instruments are not very costly. One reason for observing it is that it is only within the last few years that the highest perfection has been attained in the construction of instruments of measurement.

If these two conditions can be really fulfilled, it is very desirable to add a few more to the great number of meridian circles now in existence, for the simple reason that it is easy to exceed them in perfection. It is, however, to be remarked that a good climate is a scientific prerequisite for the success of an observatory of any kind. The value of observations is decidedly lessened by the breaks in their continuity due to the intervention of clouds. It is therefore extremely desirable that, so far as possible, new observatories should hereafter be erected under sunny skies.

If an observatory is to be devoted to physical research, a more modest outfit, both in the way of endowment and of instrumental means, may be sufficient to serve an excellent purpose. Instead of being a great co-operative work, requiring the continuous labor of several persons, physical research may be divided up into sections almost as small as we please, each of which may be worked by an individual astronomer with any instrument suited to the purpose in view. To the success of such an observatory, a clear sky is even more necessary than to one engaged in measurement. Whether a great telescope will be necessary, will depend principally upon what is to be done. The consideration which is really of the first importance is the astronomer. The man who is really wanted will do more with the most inexpensive instruments than another one with the most costly ones. As already remarked, physical research is mainly the work of the individual, and what we want is to secure the services of the ablest man and then supply him with such means of research as are necessary to the problems he has in view. New questions are arising so frequently, and the field of physical research is now so wide, that it is impossible to lay down any general rules for a physical observatory, except that means should be furnished for supplying the investigator with any instrument he may want.

A third class of observatories are those intended for instruction in astronomy. The requirements, in this direction are so different from those necessary to research that it is impossible to combine the highest efficiency in both directions with the use of the same instruments. The number of observatories especially designed for pure instruction are very few in number. The instruments necessary for the purpose are of the simplest kind; indeed, so far as mere training is concerned, the engineer's level, transit, and theodolite can be made to serve most of the purposes of the astronomical student. What the latter really wants is that training of the eye and the mind which will enable him to understand the theories of instruments, the methods of eliminating the errors to which they are subject, and the mathematical principles involved in their application. In this, as in nearly every department of professional education, we may lay it down as a rule that the wants of a liberal and of a professional education are, so far as the foundation is concerned, identical. We are too prone to lead the student into the minute details of a subject without that previous training in first broad principles which, though it may not immediately tell on his progress as a student, will be felt throughout his life to whatever field of work he may devote himself. Such a transit instrument as Hipparchus might have made,—a wooden level mounted on an axis and supplied with slits to serve the purpose of sights,—properly mounted in the meridian, could well be made to take the place of the transit instrument for purposes of instruction. Scarcely any higher skill than that of a cabinet-maker would be required in its construction. The object at which the student should then aim would be, with the aid of this instrument, to determine the error of his clock or watch within a few seconds. If he is really acquainted with the principles of the subject, and has his eyes properly trained, he will have no difficulty in soon learning to do this.—(*North American Review*).

MICHIGAN FLORA.

By Charles F. Wheeler and Erwin F. Smith, Hubbardston, Michigan.

The following interesting sketch forms the preface to a catalogue of the Phænogamous and vascular Cryptogamous Plants of Michigan, Indigenous, Naturalized and Adventive, which can be obtained of W. S. George & Co., of Lansing, Michigan:

The climate of the Upper Peninsula of Michigan is colder than that of the Lower Peninsula, the surface is considerably broken, especially in the western part, and the flora is in many respects decidedly northern, resembling in part that of British America, and in other respects like that of N. New England and Canada. Pines, firs, cedar, larch, junipers, elms, poplars, black ash, basswood, maples, and birches, are the principal trees. *Pinus strobus*, the prevailing species southward, is here largely supplanted by its more northern and less valuable congener, *P. resinosa*, whose tall, slim trunks are, however, in good demand for driving piles. Under-shrubs, like *Rubus Nutkanus* and *Taxus baccata*, var. *Canadensis*, are common, and indicate a tendency toward northern types that we find more strongly developed in the herbaceous plants. Among the latter we note as found rarely, or not at all, in the Lower Peninsula, but frequently northward, and often having a high northern range, such plants as *Anemone parviflora*, *Viola Selkirkii*, *Potentilla frigida*, *Stellaria borealis*, *Saxifraga Aizoon*, *S. tricuspidata*, *Pinguicula vulgaris*, *Castillea pallida*, *Halenia deflexa*, *Physalis grandiflora*, *Tofieldia*, *palustris*, *Salix adenophylla*, *Eriophorum alpinum*, *Aspidium fragrans*, etc., etc.

The influence of climate on vegetation may be summed up in a few words. The climate of the Lower Peninsula is not as severe of as that of the Upper, nor so even, but is subject to frequent, sudden, and extreme changes of temperature—as great a variation during the winter season as 53° Fahr. in less than 24 hours having been recorded. Such rapid changes more or less affect vegetation, especially the tender branches of cultivated trees, which are sometimes seriously injured. In one or two instances a like effect on our forest trees has been noticed. The annual range of temperature is about 116°, and the annual mean 46°. Of rain-fall, including what falls in form of snow, we have, yearly, about thirty inches. Our snow-fall is much less, for the same latitude, than that of New York and New England. In the center of the peninsula, we seldom have more than a few inches at a time.

The proximity of the Great Lakes exerts a marked influence in equalizing the temperature and the effects are marked upon our flora.

Trees like *Liriodendron Tulipifera*, *Asimina triloba*, *Cercis Canadensis*, *Gleditschia triacanthos*, *Cornus florida*, *Nyssa multiflora* and *Morus rubra*, which belong to Ohio and Central Illinois, have crept northward, favored by the mild influence of the lake winds, through the central and western part of the Lower Peninsula, often beyond the middle, and the same is true of smaller and less noticeable plants.

As might be expected from the uniform surface of the peninsula, the flora is much alike throughout. Probably three-fourths of our species are common to all sections, though by no means equally distributed; some being very abundant in one district and rare in another at no great distance. In most cases such change is due to soil rather than to difference in elevation, temperature, or atmospheric moisture.

The Lower Peninsula is covered with a deep drift of alternating sands, clays and gravels, and the flora of any section depends chiefly on which of these happens to lie uppermost. With reference to its flora, the Peninsula may be roughly divided into two great divisions—the hard-wood and the soft-wood lands; one representing the Appalachian flora, and the other, the Canadian.

The hardwood country lies south of latitude 43°, and consists of very fertile sand, clay, or loam, mostly cleared of the original forest, and largely cultivated.

The sandy or stony drift of many river valleys in this section supports a heavy growth of oak, frequently interspersed with walnut and hickory, while the margins of the streams, and the neighboring swamps, abound in soft maples, swamp and chestnut oak, white and black ash, elm, hackberry, sycamore, butternut and similar trees. Willows, dogwoods, viburnums, and buttonbush, are common shrubs in the swamps; and hazel, hawthorn, wild cherry and plum, June berry, witch-hazel, etc., are abundant on the dryer ground.

On the uplands, and away from streams, clay, loam, and a peculiar blackmuck soil, supersede the sands and gravels of the valley. The prevailing timber here is beech and maple and oak forest in about equal proportions. Beech and maple (*Acer saccharinum* and var. *nigrum*) generally grow together, forming magnificent forests of great extent. The best wheat farms are usually found on uplands near streams, where the oak timber gradually shades into beech and maple. Plains of fertile sand covered with a low, or scattering growth of oak (oak openings) are frequent, and always very desirable for farming purposes. Four species of oak are usually found on such plains—*Q. alba*, *macrocarpa*, *coccinea* and *tinctoria*.

Marshes densely covered with tamarack are common in this part of the State, and nourish in their thick shade such plants as *Drosera rotundifolia*, *Sarracenia purpurea*, *Rhus venenata*, *Ribes rubrum*, *Chiogenes hispidula*, *Salix candida*, *Smilacina trifolia*, *Pogonia ophioglossoides* and *Calopogon pulchellum*. Arborvitæ, red cedar and black spruce are comparatively rare.

A similar tract of soil and timber occurs in the upper end of the Peninsula, north of a line drawn from Thunder Bay west to the head of Grand Traverse Bay. This is commonly known as the "Traverse Region," and has a flora much like that we have just described, with the exception that some of the southern species disappear, and northern ones begin to take their place, or if found growing further south, here first become frequent.

The littoral flora of Little Traverse Bay is rich in interesting species, among which may be mentioned a small form of *Cakile Americana, Lathyrus maritimus, Potentilla Anserina, Tanacetum Huronense, Artemisia Canadensis, Cnicus Pitcheri, Juncus Balticus, Triticum violaceum, T. dasycarpum*, a peculiar form of *Bromus ciliatus, Calamagrostis longifolia, C. arenaria, and Equisetum variegatum.* The flora of the low dunes at the head of the Bay comprises, among others, the following species: *Juniperus Sabina*, var. *procumbens, Prunus pumila* and *Cornus stolonifera*, half buried in the drifting sand, *Hypericum Kalmianum, Salix glaucophylla*, and varieties, *Lilium Philadelphicum*, etc. In a moist depression were found *Arabis lyrata, Coreopsis lanceolata, Arctostaphylos Uva-ursi, Primula farinosa, Lithospermum hirtum, Triglochin maritimum*, var. *eltaum, Carex aurea, C. Œderi*, etc., etc. In thickets near the shore were found *Abies balsamea, Picea alba, Sheperdia Canadensis, and Rubus Nutkanus.* Deep forests of hemlock and yellow birch (*B. lutea*) mixed with a fine, tall growth of striped maple (*A. Pennsylvanicum*) are frequent, having underneath a tangled growth of *Taxus baccata*, var. *Canadensis*, and under all a carpet of *Lycopodium annotinum.* Alternating with these are sandy plains covered with a dense growth of *Vacciniums*, yielding a great abundance of fruit. Sugar maples and basswood are also abundant in this region, and reach an immense size. In fact, finer groves of maple it would be difficult to find in any part of the State.

The pine country proper lies between the two tracts we have thus described, and embraces about 15,000 square miles. It is composed largely of sand hills and plains, either scantily furnished with vegetation, or densely covered with pine forest. Argillaceous tracts wooded with beech and maple also occur, like oases in a desert; and swamps abound, with the usual lowland timber. Forests of hemlock spruce are frequent, and there are occasional ridges of oak. Birch (*B. lutea*) also begins to be a common forest tree, and attains a large size. The usual timber of the barrens is Jack Pine (*P. Banksiana*). Climatic and other influences have combined to produce groves composed entirely of this species of large size and of great beauty, for, instead of being "a straggling shrub, or low tree" (Gray), it rises, often 50–60 feet, straight and symmetrical. All through this region *Pinus strobus* is the prevailing species and furnishes most of the lumber, but *P. resinosa* is frequent as far south as Clare county, and occurs sparingly in the northern part of Isabella county, which appears to be its southern limit.

Such is the general character of the sylva down to about latitude 43°, but in the western part of the State, owing perhaps to moister climate, or to favorable soil, hemlock spruce is more abundant, and reaches much farther south, nearly or quite to the Indiana line, and the same is true of white pine.

Portions of the counties of Clare, Missaukee and Roscommon represent an undulating plateau, which is 700–800 feet above the level of the great lakes, and has an interesting flora, as yet little studied. This region was examined in June, 1876, and revealed a number of northern plants. In the southern part of Clare county were found *Ledum latifolium, Kalmia glauca, Physalis grandiflora* (not before found south of the Upper Peninsula), *Corydalis glauca*, and *Geranium Carolinianum*—the two latter species growing luxuriantly in the deep woods, after fires. In the shade of the Jack Pines grew *Prunus pumila, Potentilla tridentata* (not before observed in Lower Peninsula), *Krigia Virginica, Arctostaphylos Uva-ursi, Linaria Canadensis, Kœleria cristata, Carex Houghtonii*, etc., etc. Near Houghton Lake were found *Adlumia cirrhosa, Ribes lacustre, Dracocephalum parviflorum, Streptopus roseus*, and *S. amplexifolius*; and in Muskegon river, near its source, *Potamogeton lucens. Pinus resinosa* was noticed frequently, growing with common pine, and near the center of Clare county it became more abundant, forming groups. Single individuals stretch upwards 150–160 feet, their clean, copper-colored boles often rising 100 feet to the first limbs.

The flora of the deep pine woods is interesting, though rather monotonous. Very little undergrowth is found, and their gloomy recesses nourish only such plants as love thick shade. Here the club-mosses (*Lycopodiums*) find a congenial home, and flourish luxuriantly, while *Clintonia borealis* covers the ground. The great round-leaved orchid (*Habenaria orbiculata*), with its tall, greenish spike and twin leaves close to the earth, is also frequent and striking. We shall also meet *Mitchella repens, Smilacina bifolia, Trillium grandiflorum*, perhaps, and a few ferns, particularly *Asplenium Filix-fœmina*, and *Phegopteris Dryopteris.* Other species occur, of course, but not so abundantly. In more open places, and on ridges, we meet *Rhus aromatica* and *Comptonia* along with wintergreen (*Gaultheria*) and trailing arbutus (*Epigea*), and are often fortunate enough to find the wax-white, fragrant flower of *Moneses uniflora* or *Polygala paucifolia*, hiding its shining leaves under a wealth of showy pink blossoms.

The floral treasures of the pine region lie, however, in its swamps and lake borders rather than in the deep woods. Therein grows *Linnea borealis* in all its delicate beauty, carpeting the ground, and close at hand, the odd, brown-purple flower of *Cypripedium acaule* and the small yellow blossom of its water-loving relative *C. parviflorum.* In such swamps, or within a stone's throw of them, may be found many other plants of equal interest, such as *Medeola Virginica, Ledum latifolium, Andromeda polifolia, Kalmia glauca, Lonicera oblongifolia, Cardamine pratensis, Gerardia aspera, Mitella nuda, Eriophorum vaginatum*, etc. On lake margins we shall find *Lysimachia* and the blue *Pontederia* and more rarely, *Nesæa* and *Eleocharis quadrangulata.* The lake itself, most likely, will be full of *Nymphæa, Nuphar, Utricularius*, and a world of *Potamagetons* and similar water weeds. Shrubby *Vacciniums* line the bluffs, and here and there gleam the white trunks of paper birches against the dark background of pines.

In the thick-pine country, where the lumberman's axe has let in the sunlight, new plants spring up freely. Here, *Prunus Pennsylvanica* and poplars are frequent, and the blackberry is omnipresent. *Aralia hispida* and *Physalis pubescens* are also peculiar to such land, and in August *Gnaphalium decurrens* may be seen whitening thousand of acres.

One seldom beholds a drearier sight than a dead and deserted lumber region. The valuable trees were all felled years ago, and the lumberman moved on to fresh spoils, leaving behind an inextricably confused mass of tree tops, broken logs, and uprooted trunks. Blackberry canes spring up everywhere, forming a tangled thicket, and a few scattering poplars, birches, and cherries serve for arboreal life, above which tower the dead pines, bleached in the weather and blackened by fire, destitute of limbs, and looking at a distance not unlike the masts of some great harbor. Thousands of such acres, repellant alike to botanist and settler, can be seen in any of our northern counties.

In certain districts considerable beech is found associated with the pine. The soil of such tracts is usually of better quality, and can be rendered productive without much labor. It may be noticed that in such cases the pine also grows thriftier and makes better lumber.

Sections of this and the Traverse region of Michigan are still sparsely settled, or not at all, and have been visited rarely by botanists. Consequently, we may expect many editions to our flora, as well as corrections, when this region is as thoroughly known as the south half of the State now is; our ignorance, rather than nature's parsimony, explaining why we have so few species credited to us. The most promising field for the botanist evidently lies in the Houghton Lake region and northward, and in the upper Peninsula, many parts of the interior of which are botanically unknown.

Our flora, as here presented, contains in all 113 families (orders), and 1,634 species. The composites claim the largest number of species, 182—about one-ninth of all. Sedges follow with 176 species; grasses, 139; rosaceæ, 61; ferns, 56; leguminosæ, 55; figworts, 46; mints, 40; mustard and crowfoot, 39 each; heath family, 35; and umbelliferæ, 27. We have 165 trees and shrubs, about 20 of which are valuable timber trees. At least 40 of our trees and shrubs are worthy of cultivation for ornament. Sugar maples and elms are commonly planted, while the tulip tree, basswood, Kentucky coffee tree, black walnut, and butternut, among deciduous trees, and hemlock, white pine, black spruce, arbor vitæ, and red cedar, among evergreens, deserve more attention. About 20 species of woody and herbaceous native climbers are frequent, and some are worthy of cultivation, (see State Pomological Report of '79 for a list.) Ninety medicinal plants are admitted into the U. S. Pharmacopœia, 45 belonging to the primary list, and an equal number to the secondary, while a number of others deserve attention at the hands of Pharmacists.

It may be stated in conclusion that, in the preparation of this catalogue, we have spared no pains to make it thoroughly reliable, a majority of the species enumerated having passed through our hands, and the remainder being admitted only on good authority. We have preferred to make a *useful* rather than a a *large* catalogue, and, on this ground, we have rejected a number of species, some of which may yet make good their claim to be considered as part of our flora. We cannot hope to have escaped all errors, and crave charitable judgment for any such the kind reader may discover, trusting that they may be found errors of omission rather than of commission.

In our arrangement of orders, we have preferred, as more convenient, to follow the 5th edition of Gray's Manual rather than later works. The vexatious subject of synonomy has received considerable attention, and will, we believe, be found brought down nearly to date. Further observations will be published from time to time in the form of addenda, towards increase of which we solicit correspondence and contributions from all parts of the State.

IONIA, MICH., January 20, 1881.

DISRUPTION OF PLANETARY MASSES FROM THE PRIMEVAL NEBULA.

V.

By EDGAR L. LARKIN.

It has been shown in this series that the gaseous sphere could not have parted with any form of ring known to geometers. All varieties of segmental rings were examined, and their displacement found impossible by known laws of mechanics. The nebula subsided from space to the dimensions of the orbit of Neptune, else its assumed rotation could not have been equal to the orbital velocity of that planet.

Indeed, it must have revolved faster, for matter along the line of the centre of gravity of the ring moved with the rate that Neptune now has. Then the outside of the ring moved faster and the inside slower than the Neptunian velocity. But the inside was required to move with greater rapidity than any other point to exceed attraction and disrupt the mass. From this consideration alone the doctrine of ring detachment is subverted.

We are now to demonstrate that no particle whatever can be detached from a revolving sphere whether gaseous, fluid or solid, by any force known to man. Tangental force in no case overcomes radial, being unable from known physical laws, which teach that not an atom ever left a rotating cosmical mass. We have made calculation of the maximum effect of tangental force on matter on the equator of the sphere when coincident with the orbit of Neptune, radius being 2,780,000,000 miles. And if the solar parallax is modified, bringing Neptune somewhat nearer, the figures will not be in material error. It is a law of mechanics that if matter is thrown off the periphery of a revolving sphere by force evolved by rotation, the detached portion always, when maximum power is exerted, traverses a line tangent to the curvature at the point of departure. If a revolving globe should burst, the pieces would be projected along tangental lines and never rise higher. But what is a tangent to the Neptunian orbit, and what is its departure from the curvature of that mighty sphere whence Neptune's mass is said to have been detached? It is apostulate of the Hypothesis that the nebula was a sphere, else it could not have parted with matter in the form of a ring. We adopt the idea that it was round, and for the purposes of trigonometry imagine the surface to have been as level as still water. We are in search of the departure of the tangent from the curve at different distances along the equator, to learn how far tangental force was able to project matter above the periphery.

The length of 1″ of arc on the equator of the nebula was 13,478 miles, and we made selection of 8″ of arc or 107,824 miles to find the amount of its deflection from the tangent. The curvature cannot be detected by tables of logarithmic functions carried to the sixth decimal place—thus:

$$\begin{array}{lr} \log.\ \sin.\ 1' = & 6.463726 \\ \log.\quad 60 = & 1.778151 \\ \hline \log.\ \sin.\ 1'' = & 4.685575 \\ \log.\quad 8 = & .903090 \\ \hline \log.\ \sin.\ 8'' = & 5.588665 \\ \text{and} & \\ \log.\ \tan.\ 1'' = & 4.685575 \\ \log.\quad 8 = & .903090 \\ \hline \log.\ \tan.\ 8'' = & 5.588665 \end{array}$$

That is, the logarithmic sine and tangent of 8″ are the same; hence the arc cannot be told from a straight line by ordinary tables. This being the case, radii drawn to the centre from each extremity would be equal in length, and it follows that any particle of matter on the equator of the primeval sphere, after having traversed more than a hundred thousand miles under the influence of tangental force, was no further from the centre than when it started, making the formation of a ring, or detachment of an atom, alike impossible.

Not deeming it true that an arc of such length had no curvature, and not having logarithmic tables for exact computation of functions near their limits, we were obliged to use the cumbersome method of natural sines, cosines and tangents, carrying the calculation to the twentieth decimal place to secure accuracy.

To find the cosines of such minute arcs use was made of the formula—$\text{Cos.} = 1 - \frac{1}{2}\ \text{sin.}^2$, and for secants—$\text{Sec. } A = \frac{1}{\text{cos.}} A.$

Applying these formulæ to the arc of 8″ it was found that the secant was only 1.94 miles longer than the radius. That is, the curvature of the sphere at any point distant 107,824 miles from another, made a point of tangency, is less than two miles! Let us watch the career of an atom destined to be cast off the equator to

become a part of the Neptunian ring. Conceive the sphere at rest; let some unknown law cause it to rotate, with constantly accelerating velocity, until finally equatorial atoms are moving so fast that tangental force just counteracts gravity. The particles will be balanced and without weight. Increase rotation, and the atoms will move on a tangent instead of the surface of the sphere.

But they had to move 50,000 miles before it could be determined whether they were traversing the periphery or tangent, and over 50,000 more miles in order to attain an elevation of two miles! To do this the maximum force was required, as it alone was able to project matter to the tangent.

Nothing in nature can exceed the feebleness of this maximum tangental force. An atom on the equator required 8h. 54m. to traverse 107,000 miles, and then it was not quite two miles further from the centre. Yet this gentle force cast off a ring whose mass was 102 sextillion tons, if the Hypothesis is true.

No theory ever advocated concerning the development of the planets has so little in its favor as that of ring detachment. Below is a table showing the increase of distance of equatorial atoms from the centre of the sphere after having traversed different arcs from the point where they became balanced between the opposing forces, centripetal and tangental.

The first column gives the arcs, the second their length in miles, and the third shows the gain in distance from the centre of the nebula, after reaching the extremity of arcs, providing the matter touched the tangents.

Arcs.	Length in Miles.	Altitudes of Matter in Miles.
8″	107,824	1.94
10″	134,780	2.78
15″	202,170	7.
25″	336,950	20.
1′	808,671	117.
10′	8,086,710	11,759.
1°	48,520,266	417,061

But no atom could rise above the periphery, for the entire periphery itself would rise. Thus, let a particle become subject to tangental force and fly along a tangent. Let the force be enormous, sufficient to hurl equatorial matter along a tangent of 1° or 48,520,666 miles, and it will then be 417.061 miles more distant from the centre. The next atom behind would follow and all others on the same line around the sphere. The next inner particle would become elevated, and the next until the space 417,061 miles filled with gas, the result of the process being that the equatorial diameter of the nebula increased 834.122 miles. But this diminished rotation allowed gravity to regain dominion and bring down the protuberance to a level as before. This mutation must obtain in all rotating masses so long as they remain gas or liquid, the areolar velocity being a constant. During the ascent and fall of the equatorial matter it is seen that no particle wandered away, but every one returned at the command of gravity. When a mass solidifies its rotary velocity cannot accelerate, and since matter is unable to part from a fluid sphere it cannot possibly leave a solid. Hence no cosmical mass, whatever its size, density or rate of revolution, ever detached an atom by force generated by rotary motion. Suppose the nebula received an impulse that imparted inconceivable velocity of revolution, causing peripheral matter to rush on a tangent of 20°, flattening the mass into the shape of a bi-convex lens, then rotation must have almost ceased, when gravity reasserted mastery. Let one imagine himself to have been placed on the equator of the nebula, assuming the gas visible, which was not the case. An ordinary tree could then be seen with a telescope at a distance of 50.000 miles! The top of a common terrestrial mountain would have been in sight at a distance of more than 100,000 miles! The observer would have found himself in the midst of a mighty plain, and would have been able to see mountains a hundred thousand miles in every direction, so slight was the curvature. At a distance of 1° or 48,000,000 miles the depression below a tangent was only 417,000 miles. The diameter of the sun is 852,000 miles; therefore, if it were placed on the circumference of the primeval sphere, its semi-diameter could be seen at that enormous distance. Reverse nature's laws, making it possible that tangental force can disrupt a revolving mass, then with the sphere's known rotation of 3.36 miles per second (admitting the Hypothesis true) could a ring have been abandoned? Could the rotary motion even cause currents to flow from the latitudes to the equator, or even produce an equatorial elevation in so vast a level capable of detection by some distant micrometer? We answer no, because Neptune, with the same velocity, keeps on its orbit. We fail to see why the theory of ring displacement was ever entertained, since no analogy in nature suggests it.

NEW WINDSOR OBS., *Aug.* 8, 1881.

MICROSCOPICAL TECHNOLOGY.

DR. CARL SEILER'S METHODS.

MOUNTING.

For mounting, both resinous and aqueous solutions may be used, which each possess advantages over the other, and for this reason a controversy has been going on for some time, between eminent microscopists, in regard to the advantages of glycerine, on the one hand, representing the aqueous mounting media and balsam, on the other, representing the resinous class. The truth is, that both should be used, as occasion requires. Glycerine, or its equivalents, should be used when it is desired to bring out delicate striæ, lines, hair-like projections, such as cilia on the epithelium of the respiratory tract, processes of the ganglionic nerve cells, and so forth, and for delicate vegetable preparations. Balsam should be used when clearness and transparency of the object, and brilliancy as well as durability of the staining is desired.

In order to clearly understand this the student will do well to mount two preparations of the same tissue, the one in balsam, or other resinous medium, and the other in glycerine or its equivalent, and then compare the results. He will find that the one medium is better suited for a particular preparation than the other.

Balsam. Among all resinous substances Canada balsam is the best for mounting purposes, provided it has been properly prepared. To do this, take a clear sample of balsam and evaporate it in a water bath, to dryness, that is, until, when hot, all odor of turpentine has disappeared, and, when cold, it is hard and brittle, like resin. This will take several days; and great care should be exercised in keeping the water bath full of water, for as soon as the temperature in the balsam is raised above 212° F. it turns brown, and is then unfit for use.

When thus evaporated the balsam is again heated in the water bath and enough of Squibb's absolute alcohol is added to dissolve it and make the solution of the consistency of thin syrup. It is now allowed to cool and poured into a spirit lamp, the wick having been removed, in which it is kept for use, the glass cap of the lamp protecting it from dust and preventing the evaporation of the alcohol. If, after using for some time, the solution becomes too thick, it should be warmed by placing the spirit lamp in warm water and adding to it some warm absolute alcohol. If the alcohol used in dissolving the balsam or in diluting the solution is not strong enough, a white precipitate will form, which may be redissolved by the application of heat, but will reappear when exposed to the air, in a thin layer on the

slide, and the solution thus becomes useless for mounting.

Having cleaned his slides and covers, and having his balsam solution prepared, the student may now proceed to mount the objects in the following way: Place one of the stained sections which have been kept in alcohol in a small shallow dish containing some absolute alcohol, and allow it to remain there for some minutes, so as to remove all traces of water which may remain in it from the staining fluid and which have not been removed by the washing in the weaker alcohols. Then float it on the surface of some oil of cloves, also contained in a shallow glass or porcelain dish, until it has become transparent, when it should be removed from the oil, spread out on a glass slide and covered with a thin cover glass which has been taken from the bottle filled with alcohol and wiped dry with a soft rag. The specimen is now ready to be examined under the microscope, in order to see whether it will pay to permanently mount it in balsam. If found good the cover glass is carefully removed and all superfluous oil remaining on the section and on the slide is taken up with the edge of a piece of blotting paper, the object covered with a drop of the balsam solution, a fresh, dry cover is placed upon it, taking care to exclude any air bubbles, and pressure is made upon the cover to press out all superfluous balsam. In order to prevent the formation of air bubbles in the specimen the cover should be held by the forceps, near the edge, the opposite edge should be carefully placed into the balsam and the cover gradually lowered over the section until it lies flat upon it. If, after pressing the cover down, it is found that the balsam does not extend to the edge of the cover all round, a small drop of balsam should be placed near the edge, at the point where the balsam under the cover joins the empty space, when it will run in by capillary attraction. The slide is then laid aside to allow the balsam to dry spontaneously, which will take place in from four to six weeks, or it may be placed in a drying oven, the temperature of which is not raised above 130° F., when it will be ready for finishing in a much shorter time. An excellent apparatus of this kind is sold by dealers in microscopical appliances. It consists of a box of copper containing movable trays, surrounded by another larger box, also of copper, so that a space remains between the two boxes, which, when the oven is used, is filled with water through an opening at the top. A thermometer is inserted through this opening, and a lamp is placed under the outer box, which raises the temperature of the water up to any desired degree, and thereby warms the air in the inner box. A current of air is established through the inner box by ventilators, both at the top and bottom. Specimens which have been double stained with indigo should not be exposed to either heat or sunlight, as they will fade under these circumstances.

The fact that the oil of cloves or other volatile oils which may be used in its stead shrink many of the more delicate tissues, and the difficulty attending the removal of large thin sections from one solution to the other, as well as the danger of tearing while they are spread upon the slide, has led the author to discard the oil of cloves as a clearing agent, and to adopt a plan of mounting in balsam, which avoids all these dangers and which has the advantage that the slides may be finished immediately.

MOUNTING IN BALSAM.

After one of a number of sections which have been stained together has been examined in oil of cloves, and has been found to be good, the others may be inferred to be also good and worth mounting. One of them is placed in absolute alcohol, and after it has remained therein for some time, is floated upon a cover glass, which need not be wiped dry after taking it from the bottle of alcohol in which the covers are kept, held in a pair of forceps whose ends have been bent so as to stand at right angles to the shafts, and to close on top of each other. The cover with the section on it is then lifted out of the alcohol, when the specimen will be found to be evenly spread out, needing but little unfolding at the edges, which sometimes fold over; the lower surface is to be wiped dry and a drop of the alcoholic solution of balsam is placed on the section, which, on the cover, is set aside in a place free from dust, to clear up and allow the balsam to get dry. After fifteen or twenty minutes another drop of balsam should be placed upon it, in order to prevent the drying of the tissue. After twelve hours the balsam has dried sufficiently on the cover so that the specimen can be mounted, in the following manner: Take the cover up with a pair of forceps and place a drop of crude benzole* on the balsam and quickly place the cover, with the balsam down, on a clean slide, as near the centre as possible, and taking care to avoid air bubbles. Then warm the slide over a spirit lamp, place on a turn-table and quickly centre the cover so that its edge does not seem to shake when the slide is rapidly revolved. Next run a ring of cement around the edge, as will be described presently, and then press gently upon the cover, to cause the section to lie flat, and to press out the surplus of balsam, which, with a little management of the pressure, will run into the ring of cement. Another ring of cement may then be applied, when the slide is ready to be labeled and put away.

The cement for balsam mounting which is most satisfactory was devised by Mr. T. W. Starr, of Philadelphia, and is prepared as follows:

Clear Canada balsam,	370 grains.
Deodorized benzine,	140 grains.
Spirits turpentine,	120 grains.
Gum dammar,	185 grains.

Mix the balsam and benzine well together in a bottle, then add the turpentine and shake until mixed; finally, add the gum dammar, in selected pieces, and shake frequently till dissolved. If necessary, the solution should be filtered through absorbent cotton, previously moistened with turpentine. A portion of this is to be placed in a small glass-capped vial, to the cap of which is attached a small *sable* brush, which will come to a point, the ordinary camel's hair brush not being suitable for ringing, as it spreads too much. If the solution is too thick to flow readily it should be diluted with spirits of turpentine until the proper consistency is obtained. This fluid is also an excellent mounting medium when the object has previously been cleared in oil of cloves or turpentine. For ringing, this cement may be colored by adding to it a few drops of alcoholic solution of aniline of any shade desired, or it may be mixed with white zinc, when the resulting ring will appear as if made of porcelain.

The specimen to be ringed is placed upon the turn-table, and if any balsam has soiled the slide or the cover it must be removed by scraping with a sharp knife and afterward wiping with a soft linen rag wet with benzole. As a matter of course, the balsam should be hard, so that the cover will not be displaced by the scraping and wiping. If the cover should not be in the centre, and a self-centering turn-table is used, the slide is to be warmed until the balsam becomes soft, when the cover may be centered on the turn-table. Having thus prepared the slide, the brush in the cement bottle is removed and the surplus scraped off, so that it is almost dry; with the left hand the turn-table is spun round rapidly and the point of the brush applied to the edge of the cover for a moment only, holding the brush slanting in

*The refined benzole or benzine, which is frequently sold for benzole, is to volatile for our purposes.

the right hand, and that hand resting upon the stand of the table. The brush is then moistened a little more with the cement and again applied to the edge of the cover, without, however, allowing the hair of the brush to touch the glass; the small drop at the point of the brush only should be in contact with the glass and be carried around by the rapid spinning of the turn-table. The slide is then set aside so as to allow the ring to become thick by evaporation of the benzine and turpentine, when the applications of cement may be repeated until the desired thickness is obtained.

If colored or white zinc cement is to be used it should not be applied until after the first rings of clear cement have become hard, as otherwise the colored cement will run in under the cover and be disseminated among the mounting medium. If white zinc cement has been used, it may be still further improved by running one or two fine lines of asphaltum varnish around it, but not before the cement has thoroughly hardened.

The making of a neat ring around the edge of the cover is an art which can only be acquired by practice and experience, and therefore a few hints in regard to the causes of failure will greatly help the beginner.

If the ring, when finished, shows irregularties both at its inner and outer edge, the cement used is too thick and should be diluted with turpentine. If the ring is too broad—wider than about one-thirty-second of an inch, unless intentionally widened—the brush has been pressed down too hard upon the glass, which causes it to spread, or too much of the cement has been applied at once. This is especially the cause when irregularities or bulging in the edges of the ring are noticed.

If the ring is filled with minute air bubbles, the brush has been kept too long in contact with the glass in making the first ring, or its point has been brought in contact with the first ring in making the second application, when only the minute drop should have touched the glass; or, finally, the solution may be too thick.

MOUNTING IN GLYCERINE.

When a preparation is to be mounted in glycerine, it should, after having been stained, be placed in dilute glycerine for twenty-four hours, and then for the same length of time in strong glycerine (Bower's), in order to make it transparent. The section is then carefully spread upon the slide; a clean cover, which has been wiped dry, is placed upon it and pressed down, to remove the excess of glycerine from under the cover, and a small spring-clip is applied, so as to hold the cover in position during the subsequent manipulation of washing. The excess of glycerine must now be removed as carefully as possible by washing it off with a stream of water, either from a syringe or from a tap, taking care not to displace the cover in doing so. The slide is then stood on edge to dry, the spring-clip still holding the cover, and when all the water has evaporated it is ready for ringing. In order to do this the spring-clip must be removed, the slide placed upon the turn-table, the cover centered and a ring of some water-proof cement applied, in the manner described above.

The best cements for this purpose are, first, the so-called Bell's cement, which may be obtained from any dealer in microscopical supplies, and the composition of which is a secret with the makers; and second, the author's gelatine cement, which is prepared as follows:

Take of—

Coxe's gelatine,	2 drachms.
Gum ammoniac,	10 grains,
Acetic acid, No. 8,	1 ounce.

Dissolve the gum ammoniac in the acetic acid and filter through absorbent cotton; then warm the acid and gum solution by placing the vessel containing it in a water bath and add the gelatine, stirring until it is dissolved, when the resulting solution should be filtered or strained through muslin. After a ring of this cement has been made around the edge of the cover, and has become set, it should be painted over with a solution of bichromate of potash in water (ten grains to the ounce) and exposed to either sunlight or ordinary daylight. The action of the light is to make the chromate of gelatine which has been formed insoluble and thus perfectly waterproof. After this gelatine cement ring has become hard, it should be covered with a ring of white zinc cement, when it will be found that none of the glycerine will leak out, even after the specimen has been kept for years.

If thicker pieces than thin sections, such as pieces of the mucous membrane of the intestine or bladder of animals, are to be mounted in glycerine or other watery medium, a cell must be employed. This consists of a ring made of either glass, rubber, metal or cement, and which is high enough to prevent the cover-glass from pressing upon the specimen when mounted. In order to do so, the ring, if it be of glass, rubber or metal, is first cemented upon the slide with marine glue or the gelatine cement, and is accurately centered with the turn-table. If the cell is to be made of cement a ring of the required diameter (which must be a little smaller than the diameter of the cover-glass) is spun upon a slide in the same manner as was described for applying the ring to the edge of the cover in finishing slides, and is built up to the required height by repeated applications of the brush. It should then be set aside to dry and harden. Any of the cements may be used for this purpose, provided they will stick well to the glass.

Just before mounting the top of the ring forming the cell must be moistened with gelatine cement; the specimen, which has been made transparent by soaking in glycerine, is then placed in the centre of the ring, enough glycerine is added to fill the cell and the cover is applied. If an air bubble is left under the cover the latter must be lifted up and more glycerine must be added; if, on the other hand, too much liquid has been used, the surplus must be washed off, as described above. A ring of gelatine or Bell's cement is next spun around the edge of the cover, in order to seal up the cell, and it is then finished with white zinc cement.

An excellent substitute for glycerine is Farrant's solution, which combines all the advantages of glycerine and some of those of balsam, inasmuch as it has nearly the same index of refraction as glycerine, and becomes hard like balsam, doing away with the necessity of a waterproof cement. The formula generally given in the text-books for this solution is not correct, and the author has found that the following formula is more satisfactory:—

Picked gum arabic,	4 drachms.
Camphor water,	4 fl drachms.
Glycerine,	2 fl drachms.

Dissolve and strain through muslin.

Specimens to be mounted in this medium must first be made transparent by soaking in strong glycerine, and may then be mounted as though the solution were a resinous mounting medium. Great care should be taken to exclude air bubbles, as they cannot afterward be gotten rid of. This medium is especially adapted for delicate animal membranes and soft vegetable tissues.

Some specimens, especially vegetables, such as seeds, pollen grains, sections of wood, etc., may often with advantage, be mounted dry, *i. e.*, without much previous preparation, and without any mounting medium, but they must then be examined as opaque objects, and must be viewed by reflected light.

If an object is to be thus mounted, a disk is painted with asphaltum varnish in the centre of the slide, which is spun around upon the turn-table while applying the brush with varnish. A disk of dead black paper may

be pasted upon the slide, instead of the disk of varnish, to serve as a dark background. A cell ring is then applied around the edge of the disk, and the object is fastened in the centre of the cell by means of mucilage or glue. This done, the cover is placed upon the ring and cemented down as described above.—(*Compendium of Microscopical Technology.*)

THE DEARBORN OBSERVATORY.

The annual report of the Board of Directors of the Chicago Astronomical Society, together with the report of the Director of the Dearborn Observatory, dated May, 1881, is now published.

The first report is brief, and states that the Society has entered into a contract with the city of Chicago for furnishing standard time to the City Hall. In order that this contract may be more satisfactorily fulfilled, the Directors of the Society have ordered from Messrs. Howard & Co., of Boston, two new clocks, which will cost about $1000. The cost of running wires and other equipments has been $574.

The friends of the Society have contributed the funds required to meet the immediate wants of the Observatory, but the Society reiterate the often-repeated call for a permanent endowment, which will not only enable it to continue its present course of action, but to enlarge its sphere of astronomical work and take an honorable place among the prominent astronomical Observatories of Europe and America. The Directors express a hope that the time has arrived when the public-spirited citizens of Chicago will contribute the amount to accomplish this object, and we heartily trust that the confidence expressed in this respect may receive a prompt confirmation. The Dearborn Observatory is built and equipped with one of the finest equatorials in the United States; the question of endowment is therefore one which calls for immediate action.

In the second report Professsr G. H. Hough states the nature and amount of the astronomical work carried on at the Dearborn Observatory during the past year by himself and Messrs. Elias Colbert and S. W. Burnham.

The planet Jupiter has received the attention of a large number of astronomers during the past two years, especially of amateurs, and much writing of a miscellaneous character has appeared on the subject. As the proper study of markings and spots on celestial objects require the use of a telescope of great optical power, combined with good definition, the following report of observations on the planet Jupiter, made with the Dearborn equatorial, which possesses these conditions, will be read with interest, especially as they do not confirm many observations made under less favorable circumstances.

The planet Jupiter was made a special study during the past year. The first observation was secured on May 6, 1880, and the last on January 30, 1881. During this period the various spots and markings on his disc were subjected to micrometer measurements whenever practicable. It is readily apparent to any one who has examined contemporaneous drawings or sketches made by different observers and telescopes, that they are generally unreliable, unless based on micrometer measurement, and frequently give rise to erroneous deductions with regard to the phenomena in question. We believe the time has passed when mere estimations or sketches are of value in any department of practical Astronomy. Jupiter presents such a variety of phenomena on his disc, at different times, that it has been accepted as an established fact that his surface is subject to sudden and rapid changes, which may be accomplished in a few days or even a few hours.

The observations made at the Dearborn Observatory during the past two years does not confirm this statement. On the contrary, all minor changes in the markings or spots have been slow and gradual, such as might be produced by the operation of measurable mechanical forces. In fact, the principal features have been permanent, no material change being detected by micrometer measurement.

The following is a summary of the observations on Jupiter:

GREAT RED SPOT.

Longitude,	37 nights	560 measures.
Latitude,	12 "	34 "
Length,	20 "	67 "
Breadth,	10 "	32 "
Position of maj. axis,	5 "	16 "
Total		709 "

EQUATORIAL BELT.

Observed on 26 nights—	
Position of the North Edge	87 measures.
Latitude " "	34 "
Width of the Belt	53 "
Total	174 "

EQUATORIAL WHITE SPOTS.

Observed on 18 nights—	
Longitude	240 measures.
Latitude	15 "
Total	255 "

POLAR SPOTS.

Observed on 22 nights—	
Longitude	144 measures.
Latitude	40 "
Total	184 "

Being a total of 1,379 micrometer measurements.

From the micrometer measurements for longitude of spots, the equatorial diameter of the planet is deduced on 50 different nights, and from the latitude measures, the polar diameter on 13 nights.

The following deductions have been drawn from these observations.

ROTATION OF JUPITER.

The period of the planet's rotation, as obtained by different observers, has varied between $9^h\ 49^m$ and $9^h\ 56^m$. The observations made on the great red spot during the opposition of 1879, gave for the rotation period about $9^h\ 55^m\ 34^s$; being 8 seconds greater than the previously accepted value.

The discussion of our longitude measures on the great red spot, made from September 25, 1879, to January 27, 1881, comprising a period of 490 days, gives for the mean value $9^h\ 55^m\ 35.2^s$,

When the individual observations are compared, however, with this value, there is found to be a well marked maximum displacement of the center of the spot amounting to $1''.4$ of arc, indicating that the center gradually oscillated to this extent in longitude, corresponding to an actual displacement on the surface of Jupiter 3,200 miles.

The observations are all well represented by making the rotation period depend on some function of the time.

The period $9^h\ 55^m\ 33.2^s + 0.18^s \sqrt{t}$ satisfies all the observations with a mean maximum error of $0''.5$ of arc. In which the zero epoch is September 25, 1879, and t is the number of days after that date.

This formula gives for the rotation at the date January 27, 1881, $9^h\ 55^m\ 37.2^s$, agreeing essentially with the value deduced directly from the observations made during the two months previous to that date.

The rotation period derived from the observation of polar spots was as follows;

	Latitude.	Longitude.	Interval Between Extreme Observations.	Rotation.
White Spot	+10″.46	3^h 00^m	2 months.	9^h 55^m 39.3^s
White "	−11″.62	3^h 57^m	2 "	31.0^s
White "	—11″.62	4^h 26^m	2 "	33.6^s
Black "	+10″.40	0^h $0c^m$	2 "	31.0^s
Black "	+ 9″.70	2^h 22^m	1 "	40.5^s
Mean of all	--------	-------	--------	9^h 55^m 35.1^s

The latitude is simply the measured distance north or south of the Jovian equator, reduced to the mean distance of the planet from the earth. The zero of longitude is the center of the great red spot.

The white spots were egg-shaped, about 1″ of arc in length, and were only visible under favorable atmospheric conditions.

The rotation period derived from the small spots indicates an average displacement during two months of 2″ of arc, or 4,600 miles, or an average drift in longitude of nearly 3 miles per hour.

ROTATION FROM EQUATORIAL SPOTS.

From July 8 to October 1, 1880, comprising a period of 85 days, the longitude of a white spot, between the equatorial belts, in latitude 2″.3, was observed on 10 nights. The rotation, as deduced from this spot, was 9^h 50^m 00.56^s, representing all the observations within 0″.3 of arc, showing that the motion, so far as we know, was absolutely uniform. From October 28, 1880, to January 30, 1881, during a period of 94 days, another white spot, in latitude 2″.8, and differing 20 deg. in longitude from the first, was observed on 8 nights.

The rotation was 9^h 50^m $09^s.8$, with uniform motion.

If the great red spot is supposed fixed, then the mean drift of the equatorial spots would be about 270 miles per hour in the direction of the planet's rotation, or the spot made a complete revolution around the planet in about 42 days.

The approximate diameter of the equatorial white spots was 1″.2 of arc, or 2,800 miles.

These observations leave the true period of the rotation of Jupiter in a very unsettled condition. The great red spot was frequently measured to ascertain whether it was subject to any marked change, in position, size or shape.

The following are the mean results for the two oppositions of 1879 and 1880, reduced to the mean distance of the planet from the earth:

	1879.	No. of Obs.	1880.	No. of Obs.
Length	12″.25*	9	11″.55	20
Breadth	3″.46	8	3″.54	10
Latitude	6″.95	8	7″.14	12

* Re-computed with the constants of 188 .

The position of the major axis of the spot was measured as follows, the number indicating the inclination of the axis of Jupiter's equator as compared with Marth's ephemeris.

1880, July 27 + 2°.3
" Aug. 6 + 2°.5
" Sept. 4 + 2°.9
" Dec. 3 + 2°.2
1881, Jan. 17 — 0°.8. Definition poor.

These numbers indicate a remarkable degree of permanency with regard to the size, shape and position of the spot, during the two oppositions. Our observations do not warrant the assumption of any considerable change since September 25, 1879.

The actual size of the object, as seen with our telescope, was as follows:

Length, 29,600 miles.
Breadth, 8,300 "

The smaller telescopes make the approximate length considerably less than the real value.

POLAR BELTS.

During the opposition of 1880 the polar belts were not as sharply defined as during 1879, with the exception of Nos. 2 and 3, the latter of which became very conspicuous. During the month of June, when the planet was at about mean distance, no trace of polar markings could be seen. And it was not until July 4, when the distance was 0.948, that the belts 2 and 3 were barely visible. Markings on the southern hemisphere were first seen on July 24, when the distance of the planet from the earth was 0.888.

The latitude of 2 and 3 was as follows:

	1879.	1880.
No. 2,	+ 9″.78	+ 9″.75
No. 3,	+ 5″.98	+ 5″.89

EQUATORIAL BELT.

The great equatorial belt remained without any material change in size or position, as the following measurements will show:

	1879.	1880.
Latitude N. Edge,	+ 2″.59	+ 2″.35
Width	6″.77	7″.04

During both years the position of the north-edge was parallel to Jupiter's equator, as given in Marth's ephemeris.

PHENOMENA.

When a satellite crosses the disc of the planet it usually disappears in our telescope, when one-fourth to one-third across the disk, and reappears at an equal distance from the preceding limb, proving that the center of the disc is more luminous than the satellite.

In the case of the first satellite, it is sometimes seen to transit as a grayish spot, and remains visible when on the middle of the disc; such a phenomenon was observed on December 10, 1880.

On July 3, 1880, the second satellite during transit passed almost directly over the center of the great red spot, when it appeared sensibly as bright as when off the disc.

On November 1, 1880, I had the good fortuue to witness the transit of the shadow of the second satellite over the center of the red spot, and, at the same time, the transit of the shadow of the first satellite over the disc of the planet.

The shadow of the satellite, when fully projected on the red spot, was distinctly visible, but not quite as black as the shadow on the disc, proving that the red spot, although much less luminous than the disc, was yet much more luminous than the shadow.

THEORY OF JUPITER.

The generally accepted theory is, that the planet Jupiter is surrounded by a dense atmosphere, that the belts are the solid portions of the planet, and that the minor spots are clouds floating in the atmosphere. It is difficult, if not impossible, to reconcile the known phenomena with any theory yet proposed. But whether there are a sufficient number of well determined facts to form a better one, is doubtful.

Accurate observations are needed on the markings seen at different times on his disc; not sketches and general statements, but suitable micrometer measurements, from which may be deduced the motions and changes taking place on the surface. And until this method is pursued there is but little hope of solving the problem of his physical constitution.

It has occurred to me, however, that the known phenomena might be explained in the following hypothesis, viz: the surface of the planet is covered with a liquid

semi-incandescent mass; that the belts, the great red spot and other dark markings, are composed of matter of lower temperature. The egg-shaped, polar white spots are openings in the semi-fluid crust. This hypothesis would account for the slow and gradual changes occurring on the surface, which does not seem reasonable on the simple atmospheric theory.

Over the liquid surface is an atmosphere in which is formed the equatorial white spots which are of the nature of cloud.

In conclusion the director expresses what we can well believe to be his sincere regrets at the loss of the valuable services of Mr. S. W. Burnham, who has accepted a position in the Washburne Observatory, at Madison, Wis. During the past year Mr. Burnham, as heretofore, had the use of the great equatorial for double-star observations, and reported the discovery since May, 1880, of about fifty new double-stars, all of which were measured at least three times. About one-half of the number are close double, not exceeding 1″.5 in distance. Among the more prominent stars are 9 persei, 5 persei, κ pegasi, γ foracis and 60 arietis. He also made about 600 measures on previously-known double-stars.

DR. COPELAND and Mr. Dreyer have been compelled to change the title of *Urania*, as it appears that name was appropriated by some *astrological* serial. In future, then, *Urania*, the astronomical serial, will bear the title *Copernicus*.

IT is rumored that Prof. Huxley will be asked to allow his name to be entered for the Linacre professor of physiology vacant by the death of Prof. Rolleston.

COMET (*b*) 1881.

The following observations of the Great Comet of 1881, made at Australian Observatories, have been kindly furnished for publication by Professor Wm. Harkness, U. S. N., to whom they were communicated by Mr. Todd, Superintendent of the Adelaide Observatory.

DATE.		h.	m.	R. A. h.	m.	s.	Dec. South. °	′	″	Station.	* Of Comparison.
May	22,	—	—	4	58	—	35	30	—	Windsor	B. A. C. 1573
"	23,	—	—	4	59	—	35	14	—	Melbourne	Lacaille 1685
"	25,	—	—	4	59	46.	34	13	30.	Sydney	----------
"	26,	6	17	5	0	16.62	33	40	44.9	Melbourne	----------
"	27,	18	10	5	1	3.07	32	31	2.	Windsor	Lacaille 1785
"	28,	8	0	5	1	25.	32	22	7.	Adelaide	Columbæ
"	28,	18	0	5	1	35.67	32	3	48.	Melbourne	B. A. C. 1564
"	29,	5	39	5	1	48.52	31	42	42.	"	" 1615
"	29,	7	20	5	1	51.7	31	39	39.	Adelaide	" 1564
"	30,	7	33	5	2	21.8	30	51	2.	"	" 1615
"	30,			5	2	26.12	30	50	49.	"	----------
"	31,	7	8	5	2	54.6	30	0	1.	"	----------
"	31,	18	23	5	3	12.38	29	34	14.	Melbourne	----------
June	1,	5	25	5	3	26.26	29	6	40.	"	----------
"	1,	6	48	5	3	32.8	29	2	58.	Adelaide	Washington 2173
"	3,	6	4	5	4	37.6	26	51	36.	Melbourne	----------
"	5,	6	10	—	—	—	24	5	—	Adelaide	----------
"	12,	6	0	5	11	38.4	8	11	39.	"	Rigel
"	12,	18	0	5	12	13.4	6	26	21.	"	τ. Orionis

Station		°	′	″			h.	m.	s.	
Windsor	Lat.	33	36	29	S.,	Long.	10	3	21.8	E. of Greenwich.
Sydney	"	33	51	41	"	"	10	4	50.8	"
Melbourne	"	37	49	53	"	"	9	39	54.8	"
Adelaide	"	34	55	34	"	"	9	14	21.3	"

WASHINGTON, Aug. 9, 1881. W. C. W.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING AUG. 6, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

JULY AND AUGUST.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 31	30.094	30.164	9 a. m.	30.044	9 p. m.	67.6	66.3	73	4 p. m.	69	12 p. m.	63	1 a. m.	63	1 a. m.	123.
Monday, 1	30.060	30.096	9 a. m.	30.036	9 p. m.	74.3	70.3	80	3 p. m.	73	3 p. m.	68	5 a. m.	67	5 a. m.	140.
Tuesday, 2	30.014	30.058	9 a. m.	29.966	6 p. m.	75.0	71.0	81	4 p. m.	74	5 p. m.	70	2 a. m.	69	2 a. m.	141.
Wednesday, 3	29.975	30.006	7 a. m.	29.942	4 p. m.	76.6	71.0	85	4 p. m.	75	7 p. m.	67	6 a. m.	67	6 a. m.	141.
Thursday, 4	29.970	29.996	9 a. m.	29.940	6 p. m.	81.7	74.0	91	4 p. m.	79	4 p. m.	70	5 a. m.	69	5 a. m.	142.
Friday, 5	29.930	29.976	9 a. m.	29.898	6 p. m.	82.3	75.6	91	3 p. m.	79	2 p. m.	75	5 a. m.	73	5 a. m.	141.
Saturday, 6	29.864	29.914	7 a. m.	29.804	7 p. m.	83.0	76.3	91	2 p. m.	80	2 p. m.	78	12 p. m.	73	12 p. m.	139.

Mean for the week.......... 29.986 inches.
Maximum for the week at 9 am., July 31st.......... 30.164 "
Minimum " at 7 pm., Aug. 6th.......... 29.804 "
Range.......... .360 "

Dry. Wet.
Mean for the week.......... 77.2 degrees .... 72.0 degrees.
Maximum for the week at 2 pm. 6th 91. " at 2 pm 6th, 80. "
Minimum " " 1 am. 31st 63. " at 1 am 31st, 63. "
Range " " .... 28. " .... 17. "

JULY AND AUGUST.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0 / 10
Sunday, 31	n. e.	e.	s. s. e.	132	1½	4.00 am	.596	.635	.658	100	90	90	10	9 cu.	10	1.30pm	4 pm.	2.30	.03	0
Monday, 1	w. s. w.	s.	s.	119	2	4.00 pm	.644	.717	.706	85	70	90	10	2 cir. cu. s	3 cu.	-----	-----	-----	--	0
Tuesday, 2	s. s. e.	s.	s.	118	2½	3.30 pm	.706	.717	.693	90	70	85	10	3 cir. cu.	8 cu.	-----	-----	-----	--	0
Wednesday, 3	n. w.	n. n. e.	s. w.	61	¼	2.00 pm	.648	.663	.744	95	57	77	3 cir. cu.	1 cir.	0	-----	-----	-----	--	0
Thursday, 4	n. n. w.	n. w.	s. s. w.	57	¾	5.00 pm	.641	.765	.816	76	56	74	0	3 cir. cu.	0	-----	-----	-----	--	1
Friday, 5	w. n. w.	s. s. e.	s. w.	82	¾	3.40 pm	.757	.855	.787	82	62	74	2 cir.	0	0	9.15pm	10 am.	0.45	.07	0
Saturday, 6	s. s. w.	s. s. w.	s. w.	176	5¼	2.20 pm	.772	.874	.814	78	60	82	2 cir. cu.	1 c. s.	6 cu.	-----	-----	-----	--	3

Distance traveled during the week.......... 745 miles.
Maximum force.......... 5¼ lbs.

Total amount of water for the week.......... .10 inch.

DANIEL DRAPER, Ph. D.
Director Meteorological Observatory of the Department of Public Parks, New York.

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SATURDAY, AUGUST 20, 1881.

The International Electrical Exhibition at Paris was opened with much *éclat* on the 10th instant by the President of the French Republic.

The brief telegraphic dispatches describing the event, all state that Edison's exhibit was the chief centre of attraction, and that great interest was shown for the forthcoming exhibition of certain novelties which he had sent. These appeared to prove that the energies of the great electrician were far from exhausted on this subject, and that his fertile brain is as active as ever.

We are promised a very detailed report of this exhibition, so defer particulars until it arrives. England and Germany occupy the largest space of the foreign countries represented, America and Belgium coming next in order. All the departments on the day of opening were incomplete, the Americans complaining much of the dilatory behavior of the French workmen, who seemed to have no idea of the value of time.

We presume that the object of exhibitions of this character is to stimulate those engaged in electrical investigations, and to form landmarks in the history of electrical progress. In that light the Exhibition has many advantages, but Edison appears to have suffered from his generous permission to permit all comers to inspect the progress of his inventions. Many misinterpreted what they saw, and came to false conclusions, while men of no mental endowment who were mere clever mechanics, assiduously appropriated the ideas of the man of brains, and have since produced barefaced copies. These men have so far proceeded unchecked, but the time appears to have arrived when Edison has decided to enforce with vigor all those patent rights which he has secured after so many years of patient study and unremitting toil, involving the outlay of an immense amount of money.

The seizure of the "*Maxim*" electric lamps at the Paris International Exhibition appears to have been directed in consequence of such a decision, and we can assure Mr. Edison that the public will heartily sympathize with him in his attempt to enforce his just rights.

We are informed by cable that Sir George Biddell Airy has retired from the office of Astronomer Royal, and his successor appointed.

Sir George was born on the 27th of July, 1801, and was elected a Fellow of Trinity College in 1824. He commenced his career as a scientific teacher in 1826, when he was elected Lucasian Professor. In 1828 he was elected Plumian Professor, and entrusted with the management of the Observatory at Cambridge which had been just then erected and supplied with one of its instruments. On taking charge of the new Observatory he commenced a series of observations, but his able services there will be best remembered by the admirable methods he introduced in the calculations and observations, by which their utility was greatly increased.

Professor Airy had also the satisfaction of superintending the mounting of the Equatorial, the Mural Circle and the Northumberland Telescope (the last entirely from his own plans), at the Cambridge University.

In the autumn of 1835 the office of Astronomer Royal became vacant by the resignation of Mr. John Pond, and at the request of Lord Auckland, Airy received the appointment for this distinguished office, which he has since filled with so much benefit to science and honor to his country, for a period which has covered nearly half a century.

In 1833 he received the gold medal of the Royal Astronomical Society "for his discovery of the long Inequality of Venus and the Earth;" and again in 1846, for his "Reduction of the Observation of Planets made at the Royal Observatory, Greenwich, from 1750 to 1830."

We have the pleasure of directing the attention of our subscribers to a very interesting work by A. B. Hervey, A. M., on "*Sea Mosses*," being both a collector's guide and an introduction to the Study of Marine Algæ. It is published by S. E. Cassino, of Boston. In another part of this issue will be found an extended extract from this book, giving Mr. Hervey's methods of collecting and preserving specimens, and the article will, doubtless, be read with interest at this season, when so many are at the seashore, with full opportunities for commencing the study of this department of Cryptogamic Botany.

SEA MOSSES—TIME AND PLACE FOR COLLECTING.*

Most collecting on our Atlantic coast will be done during the summer and early autumn months. But I must remind those of you who live by the sea, or have it accessible at all times, that many things of the greatest beauty and interest will be missed if you do not go to the shore early. Our finest *Callithamnion C. Americanus* can be had in its rarest beauty early in March and even in February. The finest varieties of our *Rhodomela subfusca* are only to be found in the early spring months. This is true of many other plants. You will be surprised, also, to see what quantities of things you can find as late as November and December. Indeed, if you are to know these plants thoroughly, you must collect them at all seasons of the year. Then you will know when they come and when they go, and when they are in their greatest perfection. Those living and collecting on the Pacific coast are not fenced away by an icy wall, as we are on our shores during two or three months of our hard, inclement winters; so they can collect the year around. Dr. Anderson assures me that most of them are more beautiful and of more luxuriant growth during the Summer than during the Winter months. In general, there are three principal places for collecting "Sea Mosses" by the shore.

First, from the mass of material which the sea throws up upon the beaches, and leaves behind it when the tide goes out. This will be your main resource for getting the plants that grow in deep water. By many causes they will be loosened from their holdings in the depths, and will then float up to the surface and margin of the sea, and will be cast on shore. By carefully turning over these masses, which will be found along almost every sandy or pebbly beach, you will be able to get plants which could otherwise be found only by dredging in deep water. And, by careful search, too, among this material you will find all the deep water forms.

Second, upon the rocks and in the tide pools when the tide is out.

You can collect living plants in their native homes here only. Of course, no Algæ grow upon the sandy beaches. You must, therefore, seek all such as grow between the tide-marks upon rocky shores. Put on a pair of stout rubber boots, and go two or three hours before low tide, and search in every place, following the tide down to its farthest retreat. Many of the best things are found close down by low water mark, and some a little below that. These latter can be got best by taking advantage of the extreme low run of tide which comes about "new" and "full moon." The advantage of going before low tide and following the retreating water down is that you are not so apt to get a drenching, by the unexpected advance of a great wave, as when the tide is coming in; for, if you are close by the water's edge when the tide is rising, busily intent upon getting your floral treasures, you will very likely find yourself soaked with brine, for

"The breaking waves dash high
On a stern and rock-bound coast."

In hunting through the tidal region for plants, hunt everywhere, and collect everything found growing, and when collected, like Captain Cuttle, "make a note of it." If you cannot remember without, carry a small memorandum book and enter in it the habitat of each particular kind as you collect it. The tide pools, that is, the little basins in the rocks out of which the water is never emptied, are the places where the choicest collecting may be had. And the nearer they are to the low tide limits the more likely they will be to have abundance of vegetable life in them. But do not fail to look, also, under the overhanging curtain of "Rockweed" which shadows the perpendicular sides of the cliffs and great boulders. You will often find some beautiful plants there, as, for instance, the *Ptilota elegans*, the *Cladophora rupestris* and other smaller "mosses."

Third, by standing on some low, projecting reef, by the side of which the tide currents rush in and out, you will see many of the more delicate, deep water forms, all spread out beautifully and displayed in all their native grace, carried past, back and forth, in the water. Many of these, like the *Polysiphoniæ*, are seldom thrown on shore in good condition, or if they are, do not long remain so. This, therefore, is by far the best place to take many of these plants. To do this you must be provided with some simple instrument for reaching down into the water, and seize them as they go floating by. I have found nothing more convenient for this than a wire skimmer, which can be got at any house-furnishing tin shop, tied with a stout string to a light, strong stick, five or six feet long. The water passes through the meshes of this with little resistance, but the Alga, with its delicate branches thrown out widely in every direction, is very readily caught by it. It will also serve to a limited extent as an implement for detaching plants from their holdings which grow in deep tide pools, or in the sea, not too far below low water mark. For the rest of your

COLLECTING APPARATUS

you may have as little or as much as is convenient. A simple basket or box, with a few newspapers in it to wrap up and keep somewhat separate the different sorts of your collectings, will do very well. If it is convenient, have a case made with a half-dozen or less wide-mouthed bottles set in it, each provided with a cork. The case should also have a compartment for storing coarse plants, newspapers, paper bags, or whatever you may use for keeping different species, or the plants from different localities separate. Then, as your plants are collected, they may be roughly sorted and put in different bottles. But two or three bottles should be reserved for the most delicate and fragile forms. And as there are several of them which rapidly perish on being exposed to the air, the bottles should be kept partly filled with sea-water. The more delicate *Polysiphonias*, the *Calithamnions*, *Dasyas*, and some others, will need this protection. I have found a quart fruit-jar very handy. I get the kind that I can fasten a string around the neck, so as to carry it suspended in one hand, which leaves the other always free to gather in the plants with. A jar, whose cover goes on and off with the least possible trouble, is the one to be selected. The only disadvantage in using a receptacle of this sort for your collection is that in climbing over the wet and mossy rocks, your feet may chance to slip, and you get a tumble; then, in your efforts to save yourself, you will forget all about your fragile glass jar, and will smash it into a thousand pieces, upon the hard stones, and perhaps lose your whole collection. But two or three of these jars carefully packed in a basket, so as not to be easily broken, would perhaps, furnish as handy a collecting apparatus as you could extemporize at the sea shore.

MOUNTING AND PRESERVING.

For "floating out" your "Sea Mosses," as it is called, you should have a pair of pliers, a pair of scissors, a stick like a common cedar "pen stalk," with a needle driven into the end of it, or, in lack of that, any stick carefully sharpened; two or three large, white dishes, like "wash bowls," botanists drying paper, or common blotting-paper, pieces of cotton cloth, old cotton is the best; and the necessary cards or paper for mounting the plants on.

You will use the pliers in handling your plants in the water. The scissors you will need for trimming off the superfluous branches of plants which are too bushy to look well, when spread upon the paper, and to cut away parasites. The needle should be driven point first, a considerable distance into the stick, so as to make it firm, and

* From *Sea Mosses*—by A. B. Hervey—S. E. Cassino, Boston.

allow you to use the blunt end of it in arranging the finer details of your plant on the paper. For drying paper, of course, you can use common newspapers, by putting many thicknesses together; and a great many, no doubt, will do that. But sheets of blotting paper will be found much more satisfactory; twenty-five of them cut into quarters would probably be all you would use, and those you could easily take in your trunks. What will be found cheaper and still more serviceable, if you are going to mount a large number of plants at once, is a quantity of botanist's "drying paper." It can be had of the "Naturalists' Agency," 32 Hawley street, Boston, Mass., for, I believe, $1.25 per 100 sheets; probably also of other sellers of Naturalists' supplies in all the large cities on both sides of the continent. It is a coarse, spongy, brown felt paper, cut into sheets 12 x 18 inches, and has a fine capacity for absorbing moisture. For convenience, the cotton cloths should be made the same size as the drying paper used. Some collectors, who do not care to mount a great number of specimens at once, but want to have them very smooth and fine, when dry, use no drying paper at all, but in the place of it, have thin smooth pieces of deal, got out a foot or so square and one-quarter or one-third of an inch thick; upon these they spread one or more layers of cotton and lay the plant on them and put as many more over it; the cotton absorbs the moisture and the boards keep the pressure even and the papers and the plants straight and smooth throughout. For "mounting paper" each one must use his own taste. Many prefer cards cut of uniform size; they can be had at almost any paper store or job printing office made to order. Four and a half by six and a half inches, is a neat and convenient size. But if you want to mount several hundred or several thousand spceimens, in the course of a season, so as to have some to give to your friends, and to make up a number of books or albums, to sell at Church or Charity fairs, then perhaps the expense will be an item worth considering. In that case you will find it cheaper to buy a few quires of good 26 or 28 pound demy paper, unruled of course. The paper is in unfolded sheets 16 x 21 and will cut into convenient sizes for mounting any plants ordinarily collected. By halving it you have sheets 8 x 21, or 10½ x 16 inches. By quartering, the sheets are 8 x 10½ inches; halving these you get an octavo sheet 5¼ x 8 inches, which is quite large enough for the majority of plants. One half of this will give a sheet 4 x 5¼ inches which will be the size most used; while the smallest plants look best on the half of these sheets, 2½ x 4 inches.

With your large white dishes filled near to the brim with sea water, or, if you are away from the ocean with water made artificially salt, take a few of your plants from the collecting case, and put them in one of the dishes. Here, handling them with your pliers, shake them out and clean them of any adhering sand or shells, trim away parasites and superfluous branches and generally make them ready for "floating out." Thence, transfer them to the other dish. Then take your card or your paper, selecting a piece large enough to give the plant ample room, and leave a margin of white all around, and having dipped it in the water put it quite under the floating plant, holding the paper with your left hand and managing it with your right. Now float the plant out over the paper and draw the root or the base of it up near the end of the paper next your hand, so that you can hold it down on the paper with the thumb of the left hand, the rest of the hand being under the paper in the water. Now, slowly lift the paper up to the surface and draw it out of the water in such a way that the water will flow off from it in two or three directions. This will spread the plant out somewhat evenly over the paper. But in many cases you will need to arrange the plants in their most natural and graceful position, and also take care that they do not get massed upon each other, and make unsightly heaps while others are left bare. They should be carefully arranged so as to make the most beautiful picture possible. In some fine and delicate plants, too much care cannot be bestowed, in having the remote branchlets all naturally disposed and spread out. This final work of arranging details, you will do with your needle while you hold the paper very near to the surface of the water with your left hand, so near, indeed, that there will be just water enough, and no more, above it to float the delicate parts which you are manipulating. Oftentimes it will be found convenient, after the paper with the plant on it has been removed from the water, to re-immerse a part of it at a time and re-arrange the several parts separately. But all this can easily be done, more easily than I can tell how to do it. A very little practice will give you the "knack" perfectly. And, indeed, these plants are by no means refractory, or hard to manage. They will do anything you can reasonably want them to, while you humor them by keeping them in their native element. In fact, you will commonly need to do no more with them, than to just help them to do what they are willing and disposed to do themselves. For if you will let them take on your paper the form and outline which they have by nature in the water, there will be nothing left to desire, for their color, form and movement all combine there to make them the loveliest and most graceful things that grow. When you have put the last finishing touches upon the "floating" process and your "Sea Moss" is adjusted on your paper so as to be "a thing of beauty and a joy forever," then you want to lay the paper upon some inclined surface—any smooth board will do—to drain away the superfluous water. Thence it is to be transferred, in a few moments, to the press, for drying.

This is made in the following manner: Laying down one of the above described sheets of blotting paper, botanists' "drying paper" or boards of muslin-covered deal, you lay your paper with the plant on it upon this, the plant up. Cover the board or drying paper all over with "floated" specimens in the same way. Over all, and lying directly upon the plants spread your piece of muslin. Upon this put another sheet of the paper, or board, and upon this again a layer of plants, then a piece of the muslin, more paper, plants, muslin, and so on until you have disposed of all your collection, or so much of it as you care to mount. Upon the last layer of plants put a final sheet of paper, and over all a stout board as large as the drying paper. Upon this lay some heavy weights—stones will be as handy as anything at the seaside. I should put on, I think, about fifty pounds of them if I were using botanist's drying paper, which has a good deal of give in it. With the use of boards, unless there are a good many thicknesses of muslin, it would not do to weight it so heavily, or some of the plants would be crushed beyond recognition. I use the drying paper and always have two boards, one for the bottom and one for the top of my press. Then when I have made the pile complete, I can put it aside in some convenient corner out of the way, and set the stones to work bearing down upon it, a business for which they seem to have some conspicuous and weighty gifts.

Some botanists recommend that the drying paper be removed in the course of five or six hours, and the cloths and papers again in twenty-four hours. This will, perhaps, be the best if anyone has plenty of time. But my practice has always been to let them lie twenty-four hours and then give them a change of both cloths and papers, being careful in removing the cloths so as not to lift the plants from the mounting paper.

The second time in the press they should be subject to a harder pressure, seventy-five or one hundred pounds of stone being not too much. In twenty-four hours more most of them will be quite dry and ready to be put into your herbarium, album, or whatever you use for the final disposition of them. Those that are not perfectly dry should be put back in the press with dry papers and cloths for another day's stay.

When the plant is perfectly dry and removed from the press you should, before putting it away and forgetting these facts, write on the back of the paper the exact date and place of collecting.

People often ask me what I use to make the plants stick so firmly to the paper, supposing, evidently, that it is necessary to have some kind of gum or mucilage for that purpose. I have to answer that I have for most of them to use nothing whatever; that there is sufficient gelatinous mat'er in the body of the plant to make it perfectly adhere to the paper without other aid. And the reason for putting the muslin over the plants in the process of pressing and drying is, that they may not stick to the drying paper which is above them, the muslin not adhering to the plants at all except in some few cases.

But a considerable number of "Sea Mosses" do not adhere to paper well. They either have not gelatinous matter enough in them or will not give it out to glue their bodies to the paper. Various devices are resorted to in these cases. Sometimes the plant, after being dried in the press in the usual way, is simply strapped down with slips of gummed paper. Sometimes they are fastened down with some kind of adhesive substance, after being dried, gum tragacanth being the best for this. Others take them and float them out a second time in skimmed milk and after wiping off the milk from the paper, from the plants, except directly under the plants, put them in the press to dry again, when, it is said, they stay. I have never tried this method. A friend of mine who is famous for the artistic way in which she always "lays out" her "Sea Mosses" tells me that for these forms which lack what the Phrenolcgist might call "adhesiveness" she prepares from the "Irish Moss" *Chrondus crispus* a semi-fluid paste, into which she dips them before putting them on paper, and then carefully removes all of it from the paper and plant, except what is between the two and then put them in the press. By this means they are made to stick "like paper on the wall."

In preparing the coarser "Rockweed" and "Kelp" for the herbarium, another method will have to be persued. These will almost all turn very dark, or quite black, in the process of drying. I am accustomed to treat them by the following method: Taking them home, I spread them out in some shady place and let them lie for a few hours, perhaps twenty-four, perhaps less or more, until most of the water in them has evaporated, but not till they have become hard, stiff and brittle. Then I put them between sheets of drying paper and lay them in the press, and keep them there until the process of drying is complete. A little practice will be the only way by which you will be able to tell if they have been dried long enough in the open air. If you find them inclined to mould while kept in the press, you may be sure they are not dry enough, throw them away and get some new ones.

It is sometimes desirable to keep the treasures we have gathered from the sea unmounted, that we may carry them away to await a more convenient season for floating them out, or that we may send them to some friend or correspondent on the other side of the continent, or beyond the seas. It is, therefore, fortunate that all but the more delicate and perishable of these plants, may be dried rough, rolled up and kept any length of time, transported around the world, and then, when put in water again, will come out in half an hour, as fresh and bright and supple and graceful as they were when taken from their briny home. The friend just referred to assured me that even the *Callithamnia*, *Dasyæ* and the most delicate *Polysiphoniæ*, and such like plants, may be so treated by first shaking the wa'er out of them, and then thoroughly mingling them with dry seasand, and drying them rough in the usual way. She says the sand will adhere to the most delicate fibres and ramuli of the plant in such a way as to keep them separate and prevent their getting glued together, then, when they are afterwards soaked out, the sand will be disengaged, and the plant will be left as good as ever it was. Perhaps I ought to suggest that "soaking out" should always be done with salt water, unless you know you have only those plants which fresh water will not hurt. When I have had specimens of the "Rockweed," or "Kelp," sent to me "rough dried," I have found it best to prepare them for mounting, not by immersing them in water, and so get a great quantity of moisture into them, which would have to be expelled afterwards, with no little trouble, but by wrapping them about with wet towels; from these they would imbibe enough dampness to be manageable, and not enough to make them troublesome.

Before taking leave of this part of my subject I must permit myself to add a word to a point which botanists commonly think too little about, viz., the display of taste in the mounting of their plants. To the mere botanist a plant is a *specimen* of a given genus and species, interesting only for that fact. If it is a full grown typical form with fruit, all the better. Now, all are not botanists. Most of those who will read these pages will have an interest in these plants, to which the scientific interest will be secondary. I want to say then to them, look for the best things; get the whole plant when you can, but get and preserve the most perfect and beautiful plants. It is the rule with botanists to put but one species on each paper or card. I certainly advise disregarding this rule, unless you are mounting for scientific purposes altogether or chiefly. With numberless shades of red, which one group of "Sea Mosses" will give you, with the various kinds of green the other two will present, you will have an opportunity to display all the taste and skill you are master of. For in combining several different colors and forms on the same paper you may often produce the most brilliant results. A little practice will soon make you able to handle two or three plants at the same time in "floating them out" almost as readily as you can manage one. Then, again, you will find it possible with some of the more slender plants to work interesting and beautiful "designs" in the same way. Initial letters, even monograms, may not be beyond your reach with a little care and practice. Let the "Sea Mosses" contribute to the cultivation of every faculty and all possible means of pleasure for you.

For preserving your treasures after they are neatly mounted, pressed and dried, you have two courses open to you. You can take care of them as the botanist does by arranging them systematically in a herbarium, with covers of stout manilla paper, folded 10½ x 16 inches for each genus, and the species separated by white sheets of thinner cover, or you can provide yourself with blank books, made for the purpose, having the leaves cut to fit the sizes of paper or card which you mount your plants on, so as to slip the corners of the cards into the cuts. It is well in that case to provide a book with leaves large enough to hold two or four cards each. By following the directions here given I cannot doubt you will soon become a successful collector, and an expert in mounting and preserving "Sea Mosses."

ACCORDING to M. Chappuis (*Bull. Soc. Chim.*) the phosphorescence of phosphorus vapor by ozone. Phosphorus is not luminous in pure oxygen at 15°, and at the ordinary pressure, introduction of a trace of ozone causes luminosity; those substances which hinder the luminosity of phosphorus, *e. g.* turpentine oil, are substances which destroy ozone. If a little turpentine oil is brought along with phosphorus into a tube containing pure oxygen, and a small quantity of ozone is then passed in, the phosphorus exhibits its luminosity for a few moments only; M. Chappuis supposes that this is due to the combustion of phosphorus vapor by the ozone, and that the transiency of the phenomenon is explained by the rapid removal of the ozone by the turpentine oil.

THE ROTATORY POWER OF COMMERCIAL GLUCOSE AND GRAPE SUGAR. A METHOD OF DETERMINING THE AMOUNT OF REDUCING SUBSTANCE PRESENT BY THE POLARISCOPE.

BY PROF. W. H. WILEY.

From the Journal of the American Chemical Society, Vol. II.

In "the trade" the name of grape sugar is applied only to the solid product obtained from corn starch. On the other hand, the term "glucose" is given to the thick syrup made from the same material.

I shall use these words in their commercial sense.

INSTRUMENTS EMPLOYED.

I used in the following investigations two polariscopes made by Franz Schmidt and Haensch, Berlin. The readings of these instruments, after correction for displacement, agreed well together.

The one was the instrument ordinarily used, in which the purple ray is employed, and the quartz half moons give blue and red tints.

Both of these instruments are graduated to read 100 divisions, each equal to one per cent sugar with a solution containing 26.048 grms. pure cane sugar in 100 c.c. In addition to this scale the half shadow has another which gives the actual angular rotation.

This is especially convenient when the specific rotatory power of a substance is to be determined. The angular rotation, however, can be calculated for the former instrument.

For if we take the specific rotatory power of cane sugar at 73.8°, we have the following equation:

$$73.8 = \frac{a \times 100}{2 \times 26.048} \text{ whence } a = 38^\circ.45.$$

Each division on the cane sugar scale is therefore equal to 0°.3845 angular measure.

This quantity corresponds to the transition tint. It is different for the differently colored rays. In the half shadow polariscope, an instrument particularly adapted to persons afflicted with any degree of color-blindness, the mono-chromatic light coming from the sodium-Bunsen lamp passes through a crystal of acid potassium chromate. The ray thus produced is less rotatable than the "transition tint."

When the instrument gives 100 divisions on the sugar scale, it shows an angular rotation of only $34^\circ 42' = 34^\circ.7$. Our division, therefore, of the sugar scale, is equal to 0°.347 angular measure.

To determine the specific rotatory power of cane sugar for the sodium-acid potassium chromate ray, we use the following equation:

$$\text{Sp. rot. pr.} = \frac{34^\circ.7 \times 100}{2 \times 26.048} = 66^\circ.6.$$

To determine the specific rotatory power for any other substance which has been determined for the transition tint, we multiply by the factor 0.9024.

Thus, if we take the specific rotatory power for any other substance which has been determined for the transition tint, we multiply by the factor 0.9024.

Thus, if we take the specific rotatory power of dextrine for the transition tint at 139°, for the half shadow tint it will be $139^\circ \times 0.9024 = 125^\circ.4$.

These data rest upon the accepted formula:

$$(1)\ \theta = \frac{a}{\varepsilon.\ \delta.\ \lambda}$$

$$(2)\ \theta = \frac{a \times v}{\lambda \times w}$$

Here a = angular rotation.
θ = specific rotatory power.
ε = amount of substance in one grm. of the solution.
δ = specific gravity of solution.
λ = length of observation tube.
w = weight of substance in grms.

MATERIAL.

The glucose studied in the following examinations, was made by the Peoria Grape Sugar Company. I am under obligations to Mr. Allen, the superintendent, for many favors in connection with my work. The grape sugars were made in Buffalo.

ROTATORY POWER.

The average value of θ for the "half shadow ray" is nearly 85°. For the purple ray it is nearly 94°. It however varies extremely in different samples.

The following table will show the range of these variations.

TABLE I.

Showing variations of θ in different specimens of glucose and grape sugar, together with the specific gravities of the same.

No.	θ	Sp. Gr.	No.	θ	Sp. Gr.
1	91.50	1.406	11	89.36	1.416
2	91.50	1.407	12	87.73	1.422
3	98.10	1.440	13	89.77	1.417
4	79.93	1.414	14	70.84	1.463
5	75.47	1.414	15	69.40	1.463
6	83.97	1.417	16	87.67	1.412
7	82.75	1.416	17	109.99	1.427
8	86.41	1.415	18	93.17	1.431
9	84.11	1.416	19	89.75	1.409
10	87.19	1.417	20	91.31	1.421

From a study of this table it is seen that within small limits θ is independent of the specific gravity of the solution. Nos. 14 and 15 were grape sugar, and the specific gravity is much higher here than in the glucose, while the value of θ is much less.

Where the increase in density, however, is considerable, as in 3 and 17, there is also a marked increase in the value of θ, although this increase is not proportional to the increment of specific gravity. In masses of homogeneous nature and structure we should expect *a priori* that θ would always be proportional to the density of the body, *i. e.*, to the amount of optically active matter in a unit number of grammes.

It is thus seen without further argument that commercial glucoses are not optically homogeneous, even when made in the same factory and by processes which do not sensibly vary.

A further study of these optical reactions convinced me that the rotatory power of commercial glucose increased as the percentage of reducing substance diminished.

The following table shows the value of θ and the corresponding percentage of reducing matter as obtained by Fehling's solution.

TABLE II.

No.	θ	% Glucose.	No.	θ	% Glucose.
1	91.50	53.20	11	89.63	53.50
2	91.50	52.36	12	87.73	56.49
3	98.10	54.60	13	89.77	52.36
4	79.93	61.73	14	70.84	69.93
5	75.47	62.50	15	69.40	69.30
6	83.97	59.35	16	87.67	56.34
7	82.75	61.40	17	109.99	39.22
8	86.41	58.80	18	93.17	57.14
9	84.11	58.55	19	89.75	54.37
10	89.19	55.60	20	91.31	56.81

It will be seen by the above table that as the per cent

of reducing matter increases, the value of θ diminishes, and *vice versa*. Nos. 14 and 17 show extreme cases of this law. Nos. 3, 18 and 20, because of their high specific gravities, should not be included in the above generalization. Having thus established the law that the per cent of reducing matter is in general inversely as the value of θ, it is next proposed to investigate the relation between these two quantities, and determine whether it is constant or variable.

From Nos. 1 and 2, of table II, it is seen that for a value of $\theta = 91.50$, the percentage of reducing matter is 52.78. Let us say for convenience in calculation that $\theta = 91.50$ corresponds to reducing substance = 53 per cent. Let us consider next, some of the cases which the value of θ differs widely from 91.50. No. 17 of above table affords an example.

The difference is, 109.99—91.50 = 18.49. The difference in the per cent of reducing substance is 53—39.22 = 13.78. Thus an excess of the value of θ of 18.49 corresponds to a deficit of 13.78 in the percentage of reducing matter.

Therefore a variation of each degree in the value of θ is equivalent to 0.745 in the percentage of a reducing matter. By similar calculations with the other data furnished by the table, I have found, not including Nos. 3, 18 and 20, marked by the high specific gravity, that this number lies between 75 and 78.

I will give the calculation for the first of these numbers, and compare them with the numbers obtained by analysis.

TABLE III.

NUMBER.	Variation of Θ	% Glucose by 0.75 Factor.	% Glucose by Fehling's Sol.
1	0.00	53.00	53.20
2	0.00	53.00	52.36
3	—12.08	62.06	61.73
4	—16.03	65.03	62.50
5	— 7.53	58.64	59.35
6	— 8.75	59.57	61.40
7	— 5.07	56.81	58.80
8	— 7.44	58.58	58.55
9	— 4.30	56.21	56.60
10	— 2.14	54.62	53.50
11	— 3.77	55.82	56.49
12	— 1.73	54.29	56.18
13	—20.66	58.32	69.93
14	—22.10	69.56	69.30
15	— 3.83	55.88	56.09
16	+18.49	39.14	39.22
17	— 1.75	54.32	54.05
18	— 0.19	53.14	56.81
19	+ 6.60	47.15	54.60
20	+ 1.66	51.75	57.14

In the above table, Nos. 18, 19 and 20 are the three specimens with high specific gravity. We will, therefore, exclude them from the discussion. In the other numbers the percentage of reducing matter, as calculated from the reading of the polariscope, falls short of the amount obtained by the alkaline copper test ten times, and by an average of 1.018. It exceeds that amount seven times with an average of 0.75. We thus see that the polariscope will enable us to compute the reducing matter present in a glucose with a probable error of less than one per cent. This is quite accurate enough for practical purposes.

Perhaps a larger number of determinations should be made before constructing a formula for determining the amount of reducing in substance a "straight" glucose. The following formulæ, however, are given provisionally, subject to some slight correction derived from more extended data.

We may have three cases:

1. The value of Θ = 91.50
2. " " < 91.50
3. " " > 91.50

In the first case the percentage of reducing substance in a glucose, if not far from 1.410 sp. gr., will be nearly 53.

In the second case the required percentage may be found by the following formula, in which a = difference between the value of θ and 91.50, and g = per cent reducing required—

$$g = 53 + 0.75\,a \text{ or } 0.78\,a.$$

In the third case we have

$$g = 53 - 0.75\,a \text{ or } 0.78\,a.$$

In order to make the principle of more general application, I have modified the calculations so as to apply the formula directly to the cane sugar scale of the instrument.

To this end, for instruments using 26.048 grammes for 100 divisions, it is convenient to use only 10 grms. of the glucose. If 26.048 grms. are employed, the neutral point is thrown entirely beyond the limit of the scale. Ten grammes is the quantity which has been employed in the following table.

The average reading of the sugar scale for ten grammes is about 50.

In the following table will be found the results of the experiments.

TABLE IV.

NUMBER.	Scale.	% Reducing Substance by Calculation.	% Same by Cu. Test.	Difference.
1	52.65	53.43	53.20	+0.23
2	52.65	53.43	52.36	+1.07
3	46.07	61.66	61.73	—0.07
4	43.05	64.90	62.50	+2.40
5	48.04	58.75	59.35	—0.60
6	47.70	59.63	61.40	—1.77
7	49.80	57.00	58.80	—1.80
8	48.45	58.56	58.55	+0.01
9	50.26	56.45	55.60	+0.85
10	51.50	54.88	53.50	+1.30
11	50.57	56.04	56.49	—0.45
12	51.74	54.58	56.18	—1.60
13	40.83	68.21	69.93	—1.72
14	40.00	69.25	69.80	—0.05
15	50.53	56.09	56.34	—0.27
16	63.80	39.50	39.22	+0.28
17	51.73	54.37	54.05	+0.32
18	52.63	53.46	56.81	
19	56.53	48.59	54.60	
20	53.70	52.10	57.14	

The above calculations were made from the following data.

Starting with Nos. 1 and 2, and discarding small fractions, we find that 53 divisions of the cane sugar scale correspond to 53 per cent of reducing matter nearly.

By a method of calculation entirely similar to that employed in determining the reducing matter from the fluctuations of Θ, I have found that a variation of one degree in the sugar scale corresponds to an inverse variation of nearly 1.25 per cent of reducing substance.

We may have as in the previous calculation three cases.

1st. The reading of the scale = 53
2d. " " " " > 53
3d. " " " " < 53

In the first case 53 = 58 per cent nearly.

In the second case, placing a for the reading of the scale, we have

$$g = 53 - (a - 53)\ 1.25$$

In the third case we have

$$g = 53 + (53 - a)\ 1.25$$

In seven of the first seventeen cases the percentage of reducing substance calculated by the above formula, exceeded that given by the copper test, and by a mean amount of 0.539.

In ten of them it fell short, and by a mean amount of 0.938. This method, therefore, can be relied upon to give results which do not vary from the copper test except by a small amount.

Not much more in the way of accuracy can be claimed for the copper test itself.

In Nos. 18, 19 and 20 we have again the cases where the high specific gravities vitiate the results of the calculation.

CORRECTION FOR SPECIFIC GRAVITY.

I next proceeded to find out a method for correcting the reading of the polariscope for variations, caused by changes in the specific gravity of the specimens. First I determined the percentage of water in glucose of different specific gravities; following are the results:

I.

Sp. gr. = 1.440
Weight taken = 5.515. in Pt. dish.
Loss = 0.35, at 170°, 2 hours.
Per cent H_2O = 0.35 ÷ 5.515 = 6,37.

II.

Sp. gr. = 1.431
Weight taken = 5.86
Loss = 0.53, 170°, 2 hours.
Per cent H_2O = 0.53 ÷ 5.89 = 9.05.

III.

Sp. gr. = 1.409
Weight taken = 4.038
Loss = 0.622, 170°, 3 hours.
Per cent H_2O = 15.40

IV.

Sp. gr. = 1.416
Weight taken = 4.425
Loss = 0.525, 170°, 2 hours.
Per cent H_2O = 11.93

V.

Sp. gr. = 1.417
Weight taken = 8.639
Loss = 1.091, 170°, 3 hours.
Per cent H_2O = 12.70

VI.

SOLID GRAPE SUGAR.

Sp. gr. = 1.463
Weight taken = 7.215, 170°, 3 hours.
Loss = 0.61
Per cent H_2O = 9.29

These data are scarcely sufficient to establish a rule for correction for variations in specific gravity, but it appears from them that the formulæ will not vary much from the following:

The rule, 53 divisions = 53 per cent, seems applicable to samples in which the percentage of H_2O is 12 to 14, and of which the sp. gr. is from 1.409 to 1.414. For each variation of 0.001 in the specific gravity, the percentage of H_2O varies about 0.3.

Thus if we take the two extreme cases, viz.: 6.37 and 15.14 per cent of H_2O, we find the corresponding specific gravities to be 1.440 and 1.409, a difference of 0.031.

The difference in the percentage of water is 9.03. The quotient of 0.0903 ÷ 0.031 = 3 nearly.

Let us apply these data to the correction of Nos. 18, 19 and 20 in table IV. I give below these numbers and also their corrections.

TABLE V.

NUMBER.	Scale.	% Reducing Substance by Calculation.	Same Corrected.	Same by Cu. Sol.
18	52.63	53.46	55.83	56.81
19	56.53	48.59	55.17	54.60
20	53.70	52.10	56.55	51.14

The above corrections were based on the supposition that 53 divisions of the scale correspond to 53 per cent reducing matter, when the sp. gr. = 1.409, and the percentage of water 15.

We may therefore construct the following provisional formulæ for estimating the correction to be applied to the reading of the scale when the sp. gr. of the specimen varies much from 1.409.

Let a = reading of scale.
" a' = corrected reading.
" ε = sp. gr. of the sample.

Then $a' = a - 3a(\varepsilon - 1.409)$, when the sp. gr. is greater than 1.409, and $a' = a + 3a(1.409 - \varepsilon)$, when ε is less than 1.409.

I next propose to undertake some investigations to show the nature and number of the optically active principles present in glucose.

THE UNITY OF NATURE.

BY THE DUKE OF ARGYLL.

X.

THE ORIGIN OF RELIGION CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(Concluded.)

IN the beginning of this chapter I have observed how little we think of the assumptions which are involved in putting such questions as that respecting the origin of Religion. And here we have come to a point in our investigations at which it is very needful to remember again what some of these assumptions are. In order to do so let us look back for a moment and see where we stand.

We have found the clearest evidence that there is a special tendency in religious conceptions to run into developments of corruption and decay. We have seen the best reasons to believe that the religion of savages, like their other peculiarities, is the result of this kind of evolution. We have found in the most ancient records of the Aryan language proof that the indications of religious thought are higher, simpler, and purer as we go back in time, until at last, in the very oldest compositions of human speech which have come down to us, we find the Divine Being spoken of in the sublime language which forms the opening of the Lord's Prayer. The date in absolute chronology of the oldest Vedic literature does not seem to be known. Professor Max Müller, however, considers that it may possibly take us back 5000 years.[1] This is probably an extreme estimate, and Professor Monier Williams seems to refer the most ancient Vedic hymns to a period not much more remote than 1500 B. C.[2] But whatever that date may be, or the corresponding date of any other very ancient literature, such as the Chinese, or that of the oldest Egyptian papyri, when we go beyond these dates we enter upon a period when we are absolutely without any historical evidence whatever, not only as to the history of Religion, but as to the

[1] Hibbert Lectures, p. 216.
[2] "Hinduism," p. 19.

history and condition of Mankind. We do not know even approximately the time during which he has existed. We do not know the place or the surroundings of his birth. We do not know the steps by which his 'knowledge "grew from more to more." All we can see with certainty is that the earliest inventions of Mankind are the most wonderful that the race has ever made. The first beginnings of human speech must have had their origin in powers of the highest order. The first use of fire and the discovery of the methods by which it can be kindled; the domestication of wild animals; and above all the processes by which the various cereals were first developed out of some wild grasses—these are all discoveries with which in ingenuity and in importance no subsequent discoveries may compare. They are all unknown to history—all lost in the light of an effulgent dawn. In speculating, therefore, on the origin of these things, we must make one or other of two assumptions—either that Man always had the same mental faculties and the same fundamental intellectual constitution that he has now, or that there was a time when these faculties had not yet risen to the level of Humanity, and when his mental constitution was essentially inferior.

On the first of these assumptions we proceed on the safe ground of inquiry from the known to the unknown. We handle a familiar thing; we dissect a known structure; we think of a known agency. We speculate only on the matter of its first behavior. Even in this process we must take a good deal for granted—we must imagine a good deal that is not easily conceivable. I we try to present to our own minds any distinct image of the first Man, whether we supposed him to havef been specially created or gradually developed, we shall soon find that we are talking about a Being and about a condition of things of which science tells us nothing, and of which the imagination even cannot form any definite conception. The temptation to think of that Being as a mere savage is very great, and this theory underlies nine-tenths of all speculations on the subject. But, to say the very least, this may not be true, and valid reasons have been adduced to show that it is in the highest degree improbable. That the first Man should have been born with all the developments of savagery is as impossible as that he should have been born with all the developments of civilization. The next most natural resource we have is to think of the first Man as something like a child. But no man has ever seen a child which never had a parent, or some one to represent a parent. We can form no picture in our mind's eye of the mental condition of the first Man, if we suppose him to have had no communication with, and no instruction from, some Intelligence other than his own. A child that has never known anything, and has never seen example, is a creature of which we have no knowledge, and of which therefore we can form no definite conception. Our power of conceiving things is, of course, no measure of their possibility. But it may be well to observe where the impossibilities of conception are, or may be, of our own making. It is at least possible that the first Man may not have been born or created in the condition which we find to be so inconceivable. He may have been a child, but having, what all other children have, some intimations of Authority and some acquaintance with its Source. At all events, let it be clearly seen that the denial of this possibility is an assumption; and an assumption too which establishes an absolute and radical distinction between childhood as we know it, and the inconceivable conditions of a childhood which was either without Parents, or with Parents who were comparatively beasts. Professor Max Müller has fancied our earliest forefathers as creatures who at first had to be "roused and awakened from mere staring and stolid wonderment," by certain objects "which set them for the first time musing, pondering, and thinking on the visions floating before their eyes." This is a picture evidently framed on the assumption of a Fatherless childhood—of a Being born into the world with all the innate powers of Man, but absolutely deprived of all direct communication with any Mind or Will analogous to his own. No such assumption is admissible as representing any reasonable probability. But at least such imaginings as these about our first parents have reference to their external conditions only, and do not raise the additional difficulties involved in the supposition that the first Man was half a beast.

Very different is the case upon the other of the two assumptions which have been indicated above. On the assumption that there was a time when Man was different in his own proper nature from that nature as we know it now—when he was merely an animal not yet developed into a Man—on this assumption another element of the unknown is introduced, which is an element of absolute confusion. It is impossible to found any reasoning upon data which are not only unknown, but are in themselves unintelligible and inconceivable. Now it seems as if many of those who speculate on the origin of Religion have not clearly made up their minds whether they are proceeding on the first of these assumptions or on the second; that is to say, on the assumption that Man has always been, in respect to faculty, what he now is, or on the assumption that he was once a beast. Perhaps, indeed, it would be strictly true to say that many of those who speculate on the origin of Religion proceed upon the last of these assumptions without avowing it, or even without distinctly recognizing it themselves. It may be well, therefore, to point out here that on this assumption the question cannot be discussed at all. We must begin with Man as Man, when his development or his creation had made him what he is; not indeed as regards the acquisitions of experience or the treasures of knowledge, but what he is in faculty and in power, in the structure and habit of his mind, in the instincts of his intellectual and moral nature.

But, as we have also seen at the beginning of this chapter, there are two other assumptions between which we must choose. Besides assuming something as to the condition and the powers of the first Man, we must also make one or other of two assumptions as to the existence or non-existence of a Being to whom his mind stands in close relation. One is the assumption that there is no God; and then the problem is, how Man came to invent one. The other is that there is a God; and then the question is, whether He first formed, and how long He left, His creature without any intuition or revelation of Himself?

It is really curious to observe in many speculations on the origin of Religion how unconscious the writers are that they are making any assumption at all on this subject. And yet in many cases the assumption distinctly is that, as an objective reality, God does not exist, and that the conception of such a Being is built up gradually out of wonderings and guessings about "the Infinite" and "the Invisible."

On this assumption I confess that it does not appear to me to be possible to give any satisfactory explanation of the origin of Religion. As a matter of fact, we see that the tendency to believe in divine or superhuman Beings is a universal tendency in the human mind. As a matter of fact, also, we see that the conceptions which gather round this belief—the ideas which grow up and are developed from one consequence to another respecting the character of these superhuman Personalities and the relations to mankind—are beyond all comparison the most powerful agencies in molding human nature for evil or for good. There is no question whatever about the fact that the most terrible and destructive customs of barbarian and of savage life are customs more or less directly connected with the growth of religious superstitions. It was the perception of this fact which inspired the intense hatred of Religion, as it was known to

him, which breathes in the memorable poem of Lucretius. In all literature there is no single line more true than the famous line—"Tantum religio potuit suadere malorum." Nor is it less certain, on the other hand, that the highest type of human virtue is that which has been exhibited in some of those whose whole inspiration and rule of life has been founded on religious faith. Religious conceptions have been historically the centre of all authority, and have given their strength to all ideas of moral obligation. Accordingly, we see that the same hatred which inspired Lucretius against Religion because of its power for evil, now inspires other men against it because of its power for good. Those who wish to sever all the bonds which bind human society together, the State, the Church, the Family, and whose spirits are in fierce rebellion against all Law, human or divine, are and must be bitter enemies of Religion. The idea must be unendurable to them of a Ruler who cannot be defied, of a Throne which cannot be overturned, of a Kingdom which endureth throughout all generations. The belief in any Divine Personality as the source of the inexorable laws of Nature is a belief which enforces, as nothing else can enforce, the idea of obligation and the duty of obedience.

It is not possible, in the light of the unity of Nature, to reconcile this close and obvious relation between religious conceptions and the highest conditions of human life with the supposition that these conceptions are nothing but a dream. The power exercised over the mind and conduct of Mankind, by the belief in some Divine Personality with whom they have to do, is a power of having all the marks that indicate an integral part of the system under which we live. But if we are to assume that this belief does not represent a fact, and that its origin is any other than a simple and natural perception of that fact, then this negation must be the groundwork of all speculations on the subject, and must be involved, more or less directly, in every argument we use. But even on this assumption it is not a reasonable explanation of the fundamental postulates of all Religion—namely, the existence of superhuman Beings—to suppose that the idea of personality has been evolved out of that which is impersonal; the idea of Will out of that which has no Intelligence; the idea of life out of that which does not contain it.

On the other hand, if we make the only alternative assumption—namely, that there is a God, that is to say, a Supreme Being, who is the Author of creation,—then the origin of man's perception of this fact ceases to have any mystery other than that which attaches to the origin of every one of the elementary perceptions of his mind and spirit. Not a few of these perceptions tell him of realities which are as invisible as the Godhead. Of his own passions his perception is immediate—of his own love, of his own anger, of his own possession of just authority. The sense of owing obedience may well be as immediate as the sense of a right to claim it. Moreover, seeing the transcendent power of this perception upon his conduct, and, through his conduct, upon his fate, it becomes antecedently probable, in accordance with the analogies of Nature and of all other created Beings, that from the very first, and as part of the outfit of his nature, some knowledge was imparted to him of the existence of his Creator, and of the duty which he owed to Him.

Of the methods by which this knowledge was imparted to him, we are as ignorant as of the methods by which other innate perceptions were implanted in him. But no special difficulty is involved in the origin of a perception which stands in such close relation to the unity of Nature. It has been demanded, indeed, as a postulate in this discussion, that we should discard all notions of antecedent probability—that we should take nothing for granted, except that Man started on his course furnished with what are called his senses, and with nothing more. And this demand may be acceded to, provided it be well understood what our senses are. If by this word we are to understand nothing more than the gates and avenues of approach through which we derive an impression of external objects—our sight, and touch, and smell, and taste, and hearing—then, indeed, it is the most violent of all assumptions that they are the only faculties by which knowledge is acquired. There is no need to put any disparagement on these senses, or to undervalue the work they do. Quite the contrary. It has been shown in a former chapter how securely we may rest on the wonder and on the truthfulness of these faculties as a pledge and guarantee of the truthfulness of other faculties which are conversant with higher things. When we think of the mechanism of the eye, and of the inconceivable minuteness of the ethereal movements which that organ enables us to separate and to discriminate at a glance, we get hold of an idea having an intense interest and a supreme importance. If adjustments so fine and so true as these have been elaborated out of the unities of Nature, whether suddenly by what we imagine as Creation, or slowly by what we call Development, then may we have the firmest confidence that the same law of natural adjustment has prevailed in all the other faculties of the perceiving and conceiving mind. The whole structure of of that mind is, as it were, revealed to be a structure which is in the nature of a growth—a structure whose very property and function it is to take in and assimilate the truths of Nature—and that in an ascending order, according to the rank of those truths in the system and constitution of the Universe. In this connection of thought too great stress cannot be laid on the wonderful language of the senses. In the light of it the whole mind and spirit of Man becomes one great mysterious retina for reflecting the images of Eternal Truth. Our moral and intellectual preceptions of things which, in their very nature, are invisible, come home to us as invested with a new authority. It is the authority of an adjusted structure—the mental organization of which has been molded by what we call natural causes—these being the causes on which the unity of the world depends.

And when we come to consider how this molding, and the molding of the human body, deviates from that of the lower animals, we discover in the nature of this deviation a law which cannot be mistaken. That law points to the higher power and to the higher value in his economy of faculties which lie behind the senses. The human frame diverges from the frame of the brutes, so far as the mere bodily senses are concerned, in the direction of greater helplessness and weakness. Man's sight is less piercing than the eagle's. His hearing is less acute than the owl's or the bat's. His sense of smell may be said hardly to exist at all when it is compared with the exquisite susceptibilities of the deer, of the weasel, or of the fox. The whole principle and plan of structure in the beasts which are supposed to be nearest to him in form, is a principle and a plan which is almost the converse of that on which his structure has been organized. The so-called man-like Apes are highly specialized; Man on the contrary is as highly generalized. They are framed to live almost entirely on trees, and to be dependent on arboreal products, which only a very limited area in the globe can supply. Man is framed to be independent of all local conditions, except indeed those extreme conditions which are incompatible with the maintenance of organic life in any form. If it be true, therefore, that he is descended from some "arboreal animal with pointed ears," he has been modified during the steps of that descent on the principle of depending less on senses such as the lower animals possess, and more and more on what may be called the senses of his mind. The unclothed and unprotected condition of the human body, the total abscence of any organic weapon of defense, the want of teeth adapted even for prehension, and the same want of power for similar purposes in the hands and fingers—these are all changes and departures from the mere animal type which stand in obvious relation to the mental

powers of Man. Apart from these, they are changes which would have placed the new creature at a hopeless disadvantage in the struggle for existence. It is not easy to imagine—indeed, we may safely say that it is impossible to conceive--the condition of things during any intermediate steps in such a process. It seems as if there could be no safety until it had been completed—until the enfeebled physical organization had been supported and reinforced by the new capacities for knowledge and design. This, however, is not the point on which we are dwelling now. We are now speculating on the origin of Man. We are considering him only as he is, and as he must have been since he was Man at all. And in that structure as it is, we see that the bodily senses have a smaller relative importance than in the beasts. To the beasts theses sense tell them all they know. To us they speak but little compared with all that our spirit of interpretation gathers from them. But that spirit of interpretation is in the nature of a sense. In the lower animals every external stimulus moves to some appropriate action. In Man it moves to some appropriate thought. This is an enormous difference; but the principle is the same. We can see that, so far as the mechanism is visible, the plan or the principle of that mechanism is alike. The more clearly we understand that this organic mechanism has been a growth and a development, the more certain we may be that in its structure it is self-adapted, and that in its working it is true. And the same principle applies to those other faculties of our mental constitution which have no outward organ to indicate the machinery through which their operations are conducted. In them the spirit of interpretation is in communication with the realities which lie behind phenomena—with energies which are kindred with its own. And so we come to understand that the processes of Development or of Creation, whatever they may have been, which culminated in the production of a Being such as Man, are processes wholly governed and directed by a law of adjustment between the higher truths which it concerns him most to know, and the evolution of faculties by which alone he could be enabled to apprehend them. There is no difficulty in conceiving these processes carried to the most perfect consummation, as we do see them actually carried to very high degrees of excellence in the case of a few men of extraordinary genius, or of extraordinary virtue. In science the most profound conclusions have been sometimes reached without any process of conscious reasoning. It is clearly the law of our nature, however, that the triumphs of intellect are to be gained only by laborious thought, and by the gains of one generation being made the starting-point for the acquisition of the next. This is the general law. But it is a law which itself assumes certain primary intuitions of the mind as the starting-point of all. If these were wrong, nothing could be right. The whole processes of reasoning would be vitiated from the first. The first man must have had these as perfectly as we now have them, else the earliest steps of reason could never have been taken, the earliest rewards of discovery could never have been secured. But there is this great difference between the moral and the intellectual nature of Man, that whereas in the work of reasoning the perceptions which are primary and intuitive require to be worked out and elaborately applied, in morals the perceptions which are primary are all in all. It is true that here also the applications may be infinite, and the doctrines of Utility have their legitimate application in enforcing, by the sense of obligation, whatever course of conduct Reason may determine to be the most fitting and the best. The sense of obligation in itself is, like the sense of logical sequence, elementary, and, like it, is part and parcel of our mental constitution. But unlike the mere sense of logical sequence, the sense of moral obligation has one necessary and primary application which from the earliest moment of Man's existence may well have been all-sufficient. Obedience to the will of legitimate Authority is, as we have seen in a former chapter, the first duty and the first idea of duty in the mind of every child. If ever there was a man who had no earthly father, or if ever there was a man whose father was, as compared with himself, a beast, it would seem a natural and almost a necessary supposition that, along with his own new and wonderful power of self-consciousness, there should have been associated a consciousness also of the presence and the power of that Creative Energy to which his own development was due. It is not possible for us to conceive what form the consciousness would take. "No man hath seen God at any time." This absolute declaration of one of the Apostles of the Christian Church proves that they accepted, as metaphorical, the literal terms in which the first communications between Man and his Creator are narrated in the Jewish Scriptures. It is not necessary to suppose that the Almighty was seen by His first human creature walking in bodily form in a garden "in the cool of the day." The strong impressions of a spiritual Presence and of spiritual communications which have been the turning-point in the lives of men living in the bustle of a busy and corrupted world, may well have been even more vivid and more immediate when the first "Being worthy to be called a man" stood in this world alone. The light which shone on Paul of Tarsus on the way to Damascus may have been such a light as shone on the father of our race; or the communication may have been what metaphysicians call purely subjective, such as in all ages of the world do sometimes "flash upon that inward eye which is the bliss of solitude." But none the less may they have been direct and overpowering. The earliest and simplest conception of the Divine Nature might well also be the best. And although we are forbidden to suppose the embodiment and visibility of the Godhead, we are not driven to the alternative of concluding that there never could have been anything which is to us unusual in the intimations of His presence. Yet this is another of the unobserved assumptions which are perpetually made—the assumption of an uniformity in Nature which does not exist. That "all things have continued as they are since the beginning" is conceivable. But that all things should have continued as they were since before the beginning is a contradiction in terms. In primeval times many things had then just been done of which we have no knowledge now. When the form of Man had been fashioned and completed for the first time, like and yet unlike to the bodies of the beasts; when all their organs had been lifted to a higher significance in his; when his hands had been liberated from walking and from climbing, and had been elaborated into an instrument of the most subtle and various use; when his feet had been adapted for holding him in the erect position; when his breathing apparatus had been set to musical chords of widest compass and the most exquisite tones; when all his senses had become ministers to a mind endowed with wonder and with reverence, and with reason and with love—then a work had been accomplished such as the world had not known before, and such as has never been repeated since. All the conditions under which that work was carried forward must have been happy conditions—conditions, that is to say, in perfect harmony with its progress and its end. They must have been favorable, first, to the production and then to the use of those higher faculties which separated the new creature from the beasts. They must have been in a corresponding degree adverse to the incompatible with the prevalence of conditions tending to reversion or to degradation in any form. That long and gradual ascent, if we assume it to have been so,—or, as it may have been, that sudden transfiguration,—must have taken place in a congenial air and amid surroundings which lent themselves to so great a change. On every conceivable theory, therefore, of the origin of Man, all this seems a necessity of thought.

But perhaps it seems on the Theory of Development even more a necessity than on any other. It is of the essence of that theory that all things should have worked together for the good of the Being that was to be. On the lowest interpretation, this "toil co-öperant to an end" is always the necessary result of forces ever weaving and ever interwoven. On the higher interpretation it is the same. Only, some Worker is ever behind the work. But under either interpretation the conclusion is the same. That the first man should have been a savage, with instincts and dispositions perverted as they are never perverted among the beasts, is a supposition impossible and inconceivable. Like every other creature, he must have been in harmony with his origin and his end—with the path which had led him to where he stood, with the work which made him what he was. It may well have been part of that work—nay, it seems almost a necessary part of it—to give to this new and wonderful Being some knowledge of his whence and whither—some open vision, some sense and faculty divine.

With arguments so deeply founded on the analogies of Nature in favor of the conclusion that the first Man, though a child in acquired knowledge, must from the first have had instincts and intuitions in harmony with his origin and with his destiny, we must demand the clearest proof from those who assume that he could have had no conception of a Divine Being, and that this was an idea which could only be acquired in time from staring at things too big for him to measure, and from wondering at things too distant for him to reach. Not even his powers could extract from such things that which they do not contain. But in his own Personality, fresh from the hand of Nature,—in his own spirit just issuing from the fountains of its birth,—in his own Will, willing according to the law of its creation,—in his own desire of knowledge,—in his own sense of obligation,—in his own wonder and reverence and awe,—he had all the elements to enable him at once to apprehend, though not to comprehend, the Infinite Being who was the Author of his own.

It is, then, with that intense interest which must ever belong to new evidence in support of fundamental truths that we find these conclusions, founded as they are on the analogies of Nature, confirmed and not disparaged by such facts as can be gathered from other sources of information. Scholars who have begun their search into the origin of Religion in the full acceptance of what may be called the savage theory of the origin of Man—who, captivated by a plausible generalization, had taken it for granted that the farther we go back in time, the more certainly do we find all Religion assuming one or other of the gross and idolatrous forms which have been indiscriminately grouped under the designation of Fetishism—have been driven from this belief by discovering to their surprise that facts do not support the theory. They have found, on the contrary, that up to the farthest limits which are reached by records which are properly historical, and far beyond those limits to the remotest distance which is attained by evidence founded on the analysis of human speeeh, the religious conceptions of men are seen as we go back in time to have been not coarser and coarser, but simpler, purer, higher—so that the very oldest conceptions of the Divine Being of which we have any certain evidence are the simplest and best of all.

In particular, and as a fact of typical significance, we find very clear indications that everywhere Idolatry and Fetishism appear to have been corruptions, whilst the higher and more spiritual conceptions of Religion which lie behind do generally even now survive among idolatrous tribes as vague surmises or as matters of speculative belief. Nowhere even now, it is confessed, is mere Fetishism the whole of the Religion of any people. Everywhere, in so far as the history of it is known, it has been the work of evolution, the development of tendencies which are deviations from older paths. And not less significant is the fact that everywhere in the imagination and traditions of Mankind there is preserved the memory and the belief in a past better than the present. "It is a constant saying," we are told, "among African tribes that formerly heaven was nearer to man than it is now; that the highest God, the Creator Himself, gave formerly lessons of wisdom to human beings; but that afterwards He withdrew from them, and dwells now far from them in heaven." All the Indian races have the same tradition; and it is not easy to conceive how a belief so universal could have risen unless as a survival. It has all the marks of being a memory and not an imagination. It would reconcile the origin of Man with that law which has been elsewhere universal in creation—the law under which every ceature has been produced not only with appropriate powers, but with appropriate instincts and intuitive perceptions for the guidance of these powers in their exercise and use. Many will remember the splendid lines in which Dante has defined this law, and has declared the impossibility of Man having been exempt therefrom:—

> Nell' ordine ch'io dico sono accline
> Tutte nature per diverse sorti
> Più al principio loro, e men vicine;
> Onde si muovono a diversi porti
> Per lo gran mar dell' essere; e ciascuna
> Con istinto a lei dato che la porti.
> * * * * *
> Nè pur le creature, che son fuore
> D'intelligenzia, quest'arco saetta,
> Ma quelle c'hanno intelletto ed amore.[3]

The only mystery which would remain is the mystery which arises out of the fact that somehow those instincts have in Man not only been liable to fail, but that they seem to have acquired apparently an ineradicable tendency to become perverted. But this is a lesser mystery than the mystery which would attach to the original birth or creation of any breature in the condition of a human savage. It is a lesser mystery because it is of the essence of a Being whose Will is comparatively free that he should be able to deviate from his appointed path. The origin of evil may appear to us to be a great mystery. But this at least may be said in mitigation of the difficulty, that without the possibility of evil there could be no possibility of any virtue. Among the lower animals obedience has always been a necessity. In Man it was raised to the dignity of a duty. It is in this great change that we can see and understand how it is that the very elevation of his nature is inseparable from the possibility of a Fall. The mystery, then, which attaches to his condition now is shifted from his endowments and his gifts to the use he made of them. The question of the origin of Religion is merged and lost in the question of the origin of Man. And that other question, how his Religion came to be corrupted, becomes intelligible on the supposition of wilful disobedience with all its consequences having become "inherited and organized in the race." This is the formula of expression which has been invented or accepted by those who do not believe in original instincts or intuitions, even when these are in harmony with the order and with the reasonableness of Nature. It may well therefore be accepted in a case where we have to account for tendencies and propensities which have no such character—which are exceptions to the unity of Nature, and at variance with all that is intelligible in its order, or reasonable in its law.

If all explanation essentially consists in the reduction of phenomena into the terms of human thought and into the analogies of human experience, this is the explanation which can alone reconcile the unquestionable corruption of human character with the analogies of Creation.

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[3] "Paradiso," canto i. 110-120.

For the present I must bring these papers to a close. If the conclusions to which they point are true, then we have in them some foundation-stones strong enough to bear the weight of an immense, and, indeed, of an immeasurable, superstructure. If the Unity of Nature is not a unity which consists in mere sameness of material, or in mere identity of composition, or in mere uniformity of structure, but a unity which the mind recognizes as the result of operations similar to its own; if man, not in his body only, but in the highest as well as in the lowest attributes of his spirit, is inside this Unity and part of it; if all his powers are, like the instincts of the beasts, founded on a perfect harmony between his faculties and the realities of creation; if the limits of his knowledge do not affect its certainty; if its accepted truthfulness in the lower fields of thought arises out of correspondences and adjustments which are applicable to all the operations of his intellect, and all the energies of his spirit; if the moral character of Man, as it exists now, is the one great anomaly in Nature—the one great exception to its order and to the perfect harmony of its laws; if the corruption of this moral character stands in immediate and necessary connection with rebellion against the Authority on which that order rests; if all ignorance and error and misconception respecting the nature of that Authority and of its commands has been and must be the cause of increasing deviation, disturbance, and perversion, then, indeed, we have a view of things which is full of light. Dark as the difficulties which remain may be, they are not of a kind to undermine all certitude, to discomfit all conviction, and to dissolve all hope. On the contrary, some of these difficulties are seen to be purely artificial and imaginary, whilst many others are exposed to the suspicion of belonging to the same class and category. In some cases our misgivings are shown to be unreasonable, whilst in many other cases, to say the least, doubt is thrown on Doubt. Let destructive criticism do its work. But let that work be itself subjected to the same rigid analysis which it professes to employ. Under the analysis, unless I am much mistaken, the destroyer will be destroyed. That which pretends to be the universal solvant of all knowledge and of all belief, will be found to be destitute of any power to convict of falsehood the universal instinct of Man, that by a careful and conscientious use of the appropriate means he can, and does, attain to a substantial knowledge of the Truth.

ELEMENTS OF COMET (*b*), 1881.

(Communicated by Rear Admiral JOHN RODGERS, Superintendent U. S. Naval Observatory.)

The following elements have been computed by Prof. Frisby, U. S. N., from observations made with the Transit Circle at the Naval Observatory:

Time of perihelion passage, June 16, .37001.

π	=	265°	31′	15.″4
☊	=	270	58	27
log q	=	9.866748		
ι	=	63	25	55.7

MIDDLE PLACE.

C — O

$\delta\lambda\cos\beta$ — 13.″4

$\delta\beta$ + 62.1

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING AUG. 13, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

AUGUST.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 7	29.773	29.810	0 a. m.	29.722	2 p. m	73.6	70.6	79	2 p. m.	73	2 p. m.	67	12 p. m.	67	12 p. m.	123.
Monday, 8	29.889	29.910	12 p. m.	29.796	0 a. m.	70.0	65.3	78	5 p. m.	69	7 p. m.	61	5 a. m.	60	5 a. m.	140.
Tuesday, 9	29.794	29.910	0 a. m.	29.632	12 p. m.	74.0	67.7	81	3 p. m.	71	6 p. m.	62	5 a. m.	61	6 a. m.	141.
Wednesday, 10	29.616	29.710	12 p. m.	29.578	5 a. m.	77.3	70.0	86	2 p. m.	74	5 p. m.	64	12 p. m.	62	12 p. m.	141.
Thursday, 11	29.832	29.878	10 a. m.	29.710	0 a. m.	69.7	63.3	78	4 p. m.	67	6 p. m.	59	5 a. m.	58	5 a. m.	139.
Friday, 12	29.803	29.872	7 a. m.	29.700	12 p. m.	74.6	67.6	81	2 p. m.	71	2 p. m.	62	5 a. m.	61	5 a. m.	138.
Saturday, 13	29.560	29.700	0 a. m.	29.498	6 p. m.	81.3	73.7	96	4 p. m.	81	6 p. m.	70	5 a. m.	66	5 a. m.	146.

Mean for the week .... 29.752 inches.
Maximum for the week at 12 p. m., August 8th .... 29.910 "
Minimum " at 7 p. m., August 6th .... 29.498 "
Range .... .412 "

	Dry.		Wet.	
Mean for the week	74.3	degrees	68.3	degrees.
Maximum for the week,	at 4 pm. 13th 96.	"	at 6 pm 13th, 81.	"
Minimum "	" 5 am. 11th 59.	"	at 5 am 11th, 58.	"
Range "	" 37.	"	23.	"

AUGUST.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 1 / 10
Sunday, 7	s. w.	s. w.	s. w.	187	6¾	4.30 am	.693	.730	.708	85	74	100	8 cu.	8 cir. cu.	5 cir. cu.	3.45 am 2.15 pm	9 a m. 10 p m	5.15 7.45	.11 .63	10
Monday, 8	n.	n. n w.	s. e.	111	1¼	11.00 pm	.516	.554	.622	83	64	85	1 cir.	5 cu.	0	-----	-----	-----	--	0
Tuesday, 9	w. s. w	s. w.	s. s. w.	179	4	2.50 pm	.509	.612	.666	74	62	77	5 cir. cu. s	6 cir. cu.	7 cu.	10 p m	10½ pm	0.30	.01	4
Wednesday, 10	w. s. w.	n. n. w.	n. n. w.	246	5¼	1.15 pm	.666	.596	.644	77	48	85	0	3 cu.	0	-----	-----	-----	--	8
Thursday, 11	n. n. w.	n. n. e.	s. s. e.	112	1½	9.10 am	.465	.449	.586	78	52	80	0	0	0	-----	-----	-----	--	0
Friday, 12	w.	s. s. w.	s. s. w.	137	2	5.40 pm	.476	.624	.666	69	59	77	2 cir. s.	7 cir.	1 cu.	-----	-----	-----	--	1
Saturday, 13	w. s w.	s. w.	n. n. e.	230	3¾	4.00 pm	.608	.768	.829	80	51	78	7 cu.	4 cu.	5 cu.	-----	-----	-----	--	6

Distance traveled during the week .... 1,202 miles.
Maximum force .... 6¾ lbs.

Total amount of water for the week .... .75 inch.
Duration of rain .... 13 hours, 30 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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JOHN MICHELS, Editor.

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SATURDAY, AUGUST 27, 1881.

THE CINCINNATI MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The thirtieth meeting of this Society was held at Cincinnati, on the 17th of August and following days, and adjourned on the 23rd.

The meeting opened with some discouraging features, due principally to the marked absence of many of the most prominent members of the Association. Among the absentees we noticed retiring President Lewis E. Morgan, of Rochester; Professor Spencer F. Baird, Professor O. C. Marsh, of Yale; Professor Asaph Hall, of Washington; Professor W. B. Rogers, of Boston; Professor Burt G. Wilder, of Cornell; Professor Simon Newcomb, of Washington; Professor George F. Barker, of Philadelphia; Professor Alexander Graham Bell, of Washington, and Professor Alexander Agassiz, of Cambridge.

It is agreeable to record that in spite of these desertions, which in most cases were unavoidable or due to sickness, the Cincinnati meeting has been in many respects most successful, showing that the "*esprit de corps*" of the great body of the Association is at a high standard and requires no fostering spirit to maintain its vitality.

The following registration shows the attendance of members at the annual meeting since 1869:

1869—Salem	244
1870—Troy	188
1871—Indianapolis	196
1872—Dubuque	164
1873—Portland	195
1874—Hartford	224
1875—Detroit	165
1876—Buffalo	215
1877—Nashville	176
1878—St. Louis	134
1879—Saratoga	256
1880—Boston	997

At the recent Cincinnati meeting the attendance of members was about 550, which compares most favorably with all previous years, excepting the last at Boston, which from various circumstances was a phenomenal year of success.

The growing popularity of the Association, or the increased interest of the masses in scientific matters, is shown by the registration of 400 new names on the roll of the Association, the majority of whom resided in Ohio, Indiana and Kentucky.

An agreeable feature of this meeting was the exhibition of scientific apparatus by those engaged in such manufactures.

Messrs. Beck, Bausch and Lomb, Bullock, Queen & Co., and Sexton for Gundlach showed exhibits, which made it evident that microscopists can command all they desire from the optician, provided the one essential of dollars and cents are within their reach, and even in this respect improvements have been made, which have greatly reduced the expenses of microscopists.

The following gentlemen acted as officers of the association on this occasion.

CINCINNATI, 1881.

President.—George J. Brush, of New Haven, Connecticut.

Vice President, Section A.—William Harkness, of Washington, D. C.

Vice President, Section B.—E. T. Cox, of San Francisco, Cal.

Chairman of Permanent Subsection of Chemistry.—G. C. Caldwell, of Ithaca, N. Y.

Chairman of Permanent Subsection of Microscopy.—A. B. Hervey, of Taunton, Mass.

Chairman of Permanent Subsection of Anthropology.—Garrick Mallery, of Washington, D. C.

Chairman of Permanent Subsection of Entomology.—John G. Morris, of Baltimore, Md.

Permanent Secretary.—F. W. Putnam, of Cambridge, Mass.

General Secretary.—C. V. Riley, of Washington, D. C.

Secretary of Section A.—E. T. Tappan, of Gambier, O.

Secretary of Section B.—Charles S. Minot, of Boston, Mass.

Secretary of Permanent Subsection of Chemistry.—Alfred Springer, Cincinnati, O.

Secretary of Permanent Subsection of Microscopy.—W. H. Seaman, of Washington, D. C.

Secretary of Permanent Subsection of Anthropology.—J. G. Henderson, of Winchester, Ill.

Secretary of Permanent Subsection of Entomology.—B. Pickman Mann, of Cambridge, Mass.

Treasurer.—William S. Vaux, of Philadelphia, Pa.

Nearly two hundred papers, described in the following list, were entered to be read.

TITLES OF PAPERS.

1. Magnetic survey of Missouri.—*Francis E. Nipher.*
2. On the effect of prolonged stress upon the strain in timber.—*R. H. Thurston.*
3. Numerical elements of the orbits of the seven Electrical Vortices, to whose motions atmospheric storms are principally due, with the processes by which they have been derived, and examples given of the application of the formula by which their positions on the surface of the earth can be computed for any given time.—*Thomas Bassnett.*
4. Universal Energy of light.—*Pliny Earl Chase.*
5. A new system of interest, discounts, etc.—*James W. Robinson.*
6. The constitution of the "Atom" of science.—*Mrs. A. B. Blackwell.*
7. Cañons, as I have seen them, with some thoughts as to their origin.—*Wm. Bross.*
8. The unification of geological nomenclature.—*Richard Owen.*
9. Recent discoveries, measurements, and temperature observations made in Mammoth Cave, Ky.—*H. C. Hovey.*
10. A remarkable case of retention of heat by the Earth.—*H. C. Hovey.*
11. Coal dust as an element of danger of mining; shown by the late explosion in the Albion Mines, in Nova Scotia.—*H. C. Hovey.*
12. The successful administration of nitrous-oxide for dental and surgical operations.—*C. P. Howland.*
13. An Iso-picraminic acid.—*Charles W. Dabney, Jr.*
14. Development of Sugar in Maize and Sorghums.—*Peter Collier.*
15. A revision of the anatomy of the ethmoid bone in the mammalia.—*Harrison Allen.*
16. The life unit in plants.—*Byron D. Halsted.*
17. On Bopyrus manhattensis from the gill-cavity of Palæmonetes vulgaris Stimpson.—*Carl F. Gissler.*
18. The uncivilized mind in the presence of higher phases of civilization.—*Otis T. Mason.*
19. The stone images and idols of the mound-builders.—*Wm. McAdams.*
20. Some remarkable relics from mounds in Illinois.—*Wm. McAdams.*
21. Stone implement showing glacier marks.—*Wm. McAdams.*
22. The occurrence of Cretaceous fossils near mouth of Illinois river.—*Wm. McAdams.*
23. Mound-builders' skeleton.—*Watson C. Holbrook.*
24. Stone implements in the drift.—*Watson C. Holbrook.*
25. Prehistoric hieroglyphics.—*Watson C. Holbrook.*
26. A contribution to Croll's theory of secular climatal changes.—*W. J. McGee.*
27. Influence of forests upon streams.—*David D. Thompson.*
28. Alchemy, the cradle of Chemistry. Illustrated by lantern views.—*H. Carrington Bolton.*
29. The great primordial force.—*H. R. Rogers.*
30. "Mixed" or "New Process" sugars. With methods and results of analysis.—*H. W. Wiley.*
31. Amylose. Its nature and method of manufacture. Its optical properties.—*H. W. Wiley.*
32. Relation of reducing power as measured by Fehling's solution to the rotatory power of commercial glucose and grape sugar.—Second paper.—*H. W. Wiley.*
33. On a new material suitable for stop-cocks and stoppers for reagent bottles,—*H. W. Wiley.*
34. The stereoscope, and vision by optic divergence.—*W. Le Conte Stevens.*
35. The nitrogenous constituents of grasses.—*Clifford Richardson.*
36. Mineralogical Notes.—*Benjamin Silliman.*
37. On the influence of the structure of the nerve-fibres upon the production and conduction of nerve-force.—*H. D. Schmidt.*
38. The recurrence of faunas in the Devonian Rocks of New York.—*H. S. Williams.*
39. Note on some Fish remains from the Upper Devonian of New York.—*H. S. Williams.*
40. Note on specimens of Ptilophyton and associated fossils collected by Dr. H. S. Williams, in the Chemung Shales of Ithaca, N. Y.—*J. W. Dawson.*
41. Composition and quality of American Wines.—*Henry B. Parsons.*
42. On Dibromiodacrylic and Chlorbromiodacrylic Acids.—*C. F. Mabery* and *Rachel Lloyd.*
43. On Chlortribrompropionic Acid.—*C. F. Mabery* and *H. C. Weber.*
44. Alhazen's Problem: its history and bibliography, together with various solutions of it.—*Marcus Baker.*
45. Is the law of repetition the Dynamic Law underlying the Science of Chemistry?—*Miss Virginia K. Bowers.*
46. A study of blood during a protracted fast.—*Lester Curtis.*
47. A contribution to the study of Bacterial Organisms, and commonly found on exposed mucus surfaces, and in the alimentary canal of healthy individuals.—*Geo. M. Sternberg.*
48. Suggestions for improvement in the manufacture of glass, and new methods for the construction of large telescopic lenses.—*G. W. Holley.*
49. On the electrical resistance and co-efficient of expansion of incandescent platinum.—*E. L. Nichols.*
50. On recent deep-sea soundings in the Gulf of Mexico and Caribbean Sea, by the U. S. Coast Survey.—*J. E. Hilgard.*
51. Symmetrical method of Elimination in Simple Equations, by the use of some of the principles of Determinants.—*Jas. D. Warner.*
52. A Musical Local-telegraph alphabet.—*William Boyd.*
53. An Improved Sonometer.—*W. Le Conte Stevens.*
54. A new and improved freezing Microtome.—*Thomas Taylor.*
55. A new and Improved Freezing apparatus for use in Surgical and Dental Practice, being a substitute for the ether spray.—*Thomas Taylor.*
56. Bacteria and Micrococci, and their relations to plant culture.—*Thomas Taylor.*
57. Electricity, Magnetism, Gravitation. Their phenomena considered as the manifestation of one force.—*S. S. Parsons.*
58. The Berea Grit of Ohio.—*Edward Orton.*
59. The Gold-bearing drift of Indiana.—*Geo. Sutton.*
60. On the amount of Glacial erosion in Ohio, Indiana and Illinois, with some deductions therefrom.—*E. W. Claypole.*
61. On the Discovery of an Archemediform Tenestellid in the Upper Silurian Rocks of Ohio.—*E. W. Claypole.*
62. Life-history of the Buckeye Stem-borer. *Sericoris instrutana* Clem.—*E. W. Claypole.*
63. Some needed reforms in the use of Botanical Terms.—*Charles R. Ridler.*
64. Digital differentiation.—*A. J. Howe.*
65. The excavation of the Grand Cañon of the Colorado River.—*C. E. Dutton.*
66. On the cause of the Arid climates of the Far West.—*C. E. Dutton.*
67. Evolution and its place in Geology.—*Edward S. Edmunds.*
68. A Short study of the Features of the Region of Lower Great Lakes, during the Great River Age; or notes on the Origin of the Great Lakes of North America.—*J. W. Spencer.*
69. On the inhabitants of N. E. Siberia, commonly called Chukchis and Namollo.—*W. H. Dall.*
70. A Lawgiver of the Stone Age.—*Horatio Hale.*
71. *Ilex cassine*, the Black Drink of the Southern Indians.—*John G. Henderson.*
72. Was the antelope hunted by the Indians on the prairies of Illinois?—*John G. Henderson.*
73. Agriculture and Agricultural Implements of the Ancient Inhabitants of the Mississippi Valley.—*John G. Henderson.*
74. Houses of the Ancient Inhabitants of the Mississippi Valley.—*John G. Henderson.*
75. Comparative Differences in the Iroquis Group of Dialects.—*Mrs. Erminnie A. Smith.*
76. Typical thin sections of the rocks of the cupriferous series in Minnesota.—*N. H. Winchell.*
77. The limited biological importance of synthetic achievements in Organic Chemistry.—*Albert B. Prescott.*
78. On a mesal cusp of the deciduous mandibular canine of the domestic cat, Felis domestica.—*Burt G. Wilder.*

79. Remarks on the Classification and Distribution of Producti.—*S. H. Trowbridge.*
80. Note on a comparison of Newcomb's Tables of Uranus and Neptune, with those of the same planets by Le Verrier.—*D. P. Todd.*
81. The Saroscope: Register of eclipses traced from Eden's prime, 3939 B. C.—*A. W. Brown.*
82. Pentachloramyl formate.—*Alfred Springer.*
83. On the features of Equivalence to Chemical Elements, shown by electricity and heat.—*Samuel J. Wallace.*
84. On a sign of logical connection in Equations.—*Samuel J. Wallace.*
85. On an abbreviation in writing a long series of figures, and its use in calculations.—*Samuel J. Wallace.*
86. Retarded Development in Insects.—*C. V. Riley.*
87. New Insects Injurious to American Agriculture.—*C. V. Riley.*
88. The Egg-case of Hydrophilus triangularis.—*C. V. Riley.*
89. On the Oviposition of Prodoxus decipieus.—*C. V. Riley.*
90. The Cocoon of Gyrinus—*C. V. Riley.*
91. Ozark Highlands.—*G. C. Swallow.*
92.—On the disposition of color—markings of domestic animals.—*Wm. H. Brewer.*
93. On the ancient Japanese bronze belts.—*Edw. S. Morse.*
94. On changes in Mya and Lunatia since the deposition of the New England Shell-heaps.—*Edw. S. Morse.*
95. On worked shells in New England Shell-heaps.—*Edw. S. Morse.*
96. Natural and industrial history of the White Pine in Michigan.—*Wm. Hosea Ballou.*
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98 On a convenient method of expressing micrometrically the relation between English and metric units of length on the same scale.—*Wm. A. Rogers* and *Geo. F. Ballou.*
99. Evidence of atomic motion within liquid molecules, as based upon the speed of chemical action.—*R. B. Warder.*
100. On a new method of applying water power of small head to effect the direct compression of air to any required high pressure.—*H. T. Eddy.*
101. A preliminary investigation of two causes of lateral deviation of spherical projectiles based on the kinetic theory of gases.—*H. T. Eddy.*
102. Phenomena of growth in plants.—*D. P. Penhallow.*
103. On the life duration of the Heterocera (moths).—*J. A. Lintner.*
104. On the action of Pilocarpin in changing the color of the human hair.—*D. W. Prentiss.*
105. On a simple method of measuring faint spectra.—*Wm. Harkness.*
106. On the methods of determining the solar parallax, with special reference to the coming transit of Venus. —*Wm. Harkness.*
107. The sources of the nitrogen of plants.—*W. O. Atwater.*
108. The chemistry of fish and invertebrates.—*W. O. Atwater.*
109. The quantitative estimation of nitrogen.—*W. O. Atwater.*
110. The quantitative estimation of Chlorine.—*W. O. Atwater.*
111. Historic Notes on Cosmic Physiology.—*T. Sterry Hunt.*
112. Upon the use of the Induction Balance as a means of determining the location of leaden bullets imbedded in the human body.—*Alexander Graham Bell.*
113. Upon a new form of electric probe.—*Alexander Graham Bell.*
114. The best method of mounting whole chick embryos.—*Charles Sedgwick Minot.*
115. Note on whether man is the highest animal.—*Charles Sedgwick Minot.*
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117. The motion of roots in germinating Indian Corn—*W. J. Beal.*
118. Exhibition of some archæological specimens from Missouri.—*S. H. Trowbridge.*
119. Animal myths of the Iroquois.—*Mrs. Erminnie A Smith.*
120 A remarkable invasion of northern New York by a Pyralid Insect (*Crambus vulgivagellus*).—*J. A. Lintner.*
121. On the wave-lengths of the principal lines of the Solar Spectrum.—*T. C. Mendenhall.*
122. How does the bee extend its tongue.—*A. J. Cook.*
123. The Syrian Bees.—*A. J. Cook.*
124 Carbolic acid as a preventive of Insect Ravages.—*A. J. Cook.*
125. A new self-registering Mirror Barometer.—*John R. Paddock.*
126. On the length of life of Butterflies.—*W. H. Edwards.*
127. On certain habits of Heliconia charitonia.—*W. H. Edwards.*
128. Notes on experimental Chemistry.—*Albert B. Prescott.*
129. Additional facts on the fertilization of Yucca.—*Thomas Meehan.*
130. On the Interpretation of of Pictographs by the application of Gesture-signs.—*W. J. Hoffman.*
131. On the great outburst in Comet *b* of 1881, observed at the Cincinnati Observatory.—*Ormond Stone.*
132. An alleged abnormal peculiarity iu the history of Argynnis myrina.—*W. H. Edwards.*
133. Some new forms of apparatus for the chemical laboratory.—*G. C. Caldwell.*
134. Time service, Carleton College Observatory.—*W. W. Payne.*
135. Note on the theory of the flight of elongated projectiles. —*H. T. Eddy.*
136. On the mechanical Principles involved in the flight of the boomerang.—*H. T. Eddy.*
137.—On a convenient form of slide case.—*Robt Brown, Jr.*
138. A filtration evaporation balance.—*H. Carmichael.*
139. The liquifaction of glass in contact with water at 250° c. —*H. Carmichael.*
140. A new Radiometer.—*H. Carmichael.*
141. A new differential Thermometer.—*H. Carmichael.*
142. On some relations of Birds and Insects.—*S. A. Forbes.*
143. Comparison of Maya dates with those of the Christian Era.—*Cyrus Thomas.*
144. A new theory of the formation of Hail.—*Leonard Waldo.*
145. Method of determining the value of the Solar Parallax from meridian observations of Mars.—*J. R. Eastman.*
146. Numbers of cometary orbit relative to perihelion distance.—*H. A. Newton.*
147. Phonetics of the Káyowe language.—*Albert S. Gatschet.*
148. The needle telephone, a new instrument by Dr. Goodman, of Louisville, Ky.—*J. Lawrence Smith.*
149. Hiddenite, a new American gem.—*J. Lawrence Smith.*
150. Iron with anomalous chemical properties.—*J. Lawrence Smith.*
151. Determination of Phosphorus in iron.—*J. Lawrence Smith.*
152. Nodular concretions in meteoric iron, bearing on the origin of same.—*J. Lawrence Smith.*
153. An anomalous magnetic property of a specimen of iron.—*J. Lawrence Smith.*
154. Regulator of filter pumps.—*J. Lawrence Smith.*
155. Ringing Fences.—*S. W. Robinson.*
156. Niagara River. Its cañon, depth and wear.—*Wm. Hosea Ballou.*
157. On the relations of the growth, size and age of animals. *Charles S. Minot.*
158. Suggestions of co-operation in furthering the study of entomology.—*B. Pickman Mann.*
159. On Standard Time.—*E. B. Elliott.*
160. The Electrophore and electric lighting.—*E. B. Elliott.*
161. An attachment for burettes avoiding the necessity of using glass stop-cocks.—*Fr. A. Roeder.*
162. On a new form of balances.—*Fr. A. Roeder.*
163. Natural Filtration of water for domestic use in cities. —*G. C. Swallow.*
164. Note on an experimental determination of the value of π.—*T. C. Mendenhall.*
165. Remarks upon and an exhibition of Japanese Magic mirrors.—*T. C. Mendenhall.*

166. Notice of a fern indigenous to California, but heretofore considered as an introduced hot-house species.—*Mrs. Leander Stone.*
167. Scheme for aiding the Euler's transformations of coordinates.—*J. D. Warner.*
168. The temporal process of the malar bone in the ancient human crania from Madisonville.—*Frank W. Langdon.*
169. Buffalo drives on the Rock river in Wisconsin.——*Stephen D. Peet.*
170. The Emblematical Mounds on the four lakes of Wisconsin.—*Stephen D. Peet.*
171. Fossil teeth of Mammals from the Drift of Illinois.—*Wm. McAdams.*
172. On comparison of yard and metre by means of reversible pendulum.—*C. S. Peirce.*
173. Exhibition of a curious stone relic.—*G. W. Holstein.*
174. Some Phenomena in the conjugation of the infusorium Actinophrys Sol.—*J. D. Cox.*
175. On the errors to which Self-registering clinical thermometers are liable.—*Leonard Waldo.*
176. Note on the chemical examination of maize residue from the manufacture of glucose.—*C. Gilbert Wheeler.*
177. The Temperature of North German Traps at the time of their extrusion.—*H. Carmichael.*
178. Recent existence of sword-fish, shark, and dolphin in the fresh water pond near Buffalo, N. Y.—*Wm. Zimmerman.*
179. Antiquity of Man in America.—*W. De Haas.*
180. Progress of Archæological Research.—*W. De Haas.*
181. The Mound Builders. An inquiry into their assumed southern origin.—*W. De Haas.*
182. Four years' observation with the Lysimeter, at Framington, Mass.—*E. Lewis Sturtevant.*

The next annual meeting of the Association will take place at the City of Montreal under the Presidency of Dr. J. W. Dawson, Principal of McGill College. The election of Dr. Dawson will be a welcome announcement in all scientific circles, and the meeting for 1882 will doubtless be one of the most memorable in the annals of the Association.

We commence this week with the publication of a series of the papers read at the Cincinnati meeting or abstracts prepared by the authors. Those who have not forwarded their communications are requested to do so as soon as convenient. We shall be willing to prepare suitable illustrations, if a request for the same is made at once, to afford time for their preparation.

REPORT ON THE GEOLOGY AND RESOURCES OF THE BLACK HILLS OF DAKOTA. By HENRY NEWTON, E. M., and WALTER P. JENNEY, E. M., Washington, D. C., 1880.

The report on the Black Hills issued six years after the death of its leading observers, to whose name at least it may prove an appropriate monument, comprises the geology, palæontology, mineral resources, lithology and related subjects of interest of that boss of rocks whose circular uplift commands the outstretched plains of central Dakota.

To the *fames sacra auri* may at least be attributed one important service in this connection, as it was a transient disturbance with the Indian settlers, caused by the appearance of gold hunters on their domain, that immediately led to the survey.

The Black Hills had been assigned to the Sioux, and this unauthorized irruption raised the question how far the United States Government might permit a violation of their contract with the Indians, and how much benefit in mineral wealth would accrue to the new explorers and settlers if their incursions were tolerated. To answer more especially this latter question, and to make substantial contributions to general knowledge, the United States Government instituted a survey of this interesting and unknown country, and to Messrs. Jenney, and Newton, was intrusted its management and direction, under the auspices of the Department of the Interior.

After six months spent in this wild and inhospitable region, members of the survey returned, richly provided with means for a more deliberate examination of its character, and scientific aspects in the laboratories and cabinets of the east.

A delay—one of the innumerable hitches incident to congressional apathy or pre-occupancy—in the appropriation of monies for the printing of their report, invited Mr. Newton to revisit the hills in the spring of 1877 to complete his observations, mend or extend his theories, and here he contracted typhoid fever, of which he died—a loss to science, to society and education.

The work begun under his vigorous and intelligent supervision naturally halted, and although many of its various parts were long since completed, it is only now that in a compiled form they appear in print.

Mr. Gilbert edited the work and undertook the difficult and thankless task of deciphering, compacting and evolving from the *disjecta membra* of Mr. Newton's notes, the part devoted to the discussion of the geology, physical and stratigraphical of these hills. It is not difficult to detect the mind and pen of the author of the "Geology of the Henry Mountains," and whether or not the essay would form an exact reproduction of Mr. Newton's views, it is itself a valuable monograph, instructive and suggestive.

The Black Hills cover an area of 850 square miles, rising from the level and uninhabited wastes about them to an altitude at their highest point of nearly 8000 feet, thickly covered with dense and primeval forests of pine, whose condensed shadows from afar hides all else, and for long marches distinguishes these highlands to the approaching traveller.

The Black Hills, briefly, are an uplift of conformable strata, displaying their consecutive beds in symmetrical succession, from a central axis or elevation, disintegrated and channelled, sculptured and modified by subærial and aqueous erasion. The simplicity and perfection of their stratigraphical structure render them comparatively easy of exposition, and make them a capital example of primary sedimentation, possibly to become classic in future illustrations of geological principles.

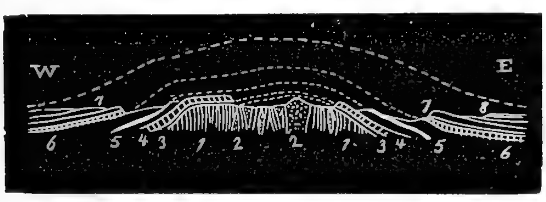
The formations, as they are crossed from the centre of the group outward to the circumference, and similarly disposed on every side—*i. e.*, sloping inward to the centre—are the archæan, Potsdam sandstone, carboniferous, shales and limestone, red beds—Trias, Jura—cretaceous and then beyond, upon the plains Tertiary. The central area is a diversified region abounding in park-like expanses, wild and rugged chasms, peaks, isolated pyramids, picturesque gorges, table-lands and a net-work of enfilading streams pouring outward east and west to swell the waters of the Cheyenne and Belle Fourche rivers. This is the archæan area or axis, upon whose flanks repose the higher strata, and in whose gulches and stream beds were found the traces of gold which first brought these hills to scientific notice. This axis lies generally north and south, is slightly arcuate, with its convexity pointing eastward, and is composed of schists, quartzites, gneiss rock, granite, trachytic intrusions and associated metamorphic slates. The granite and quartzites form salient ridges, and the trachyte sharp peaks in the landscape. Next out-cropping underneath the carboniferous is the Potsdam, unconformably bedded upon the upturned edges of archæan slates, carrying characteristic fossils and made up of basal conglomerate, sandstone locally altered around trachytic cones to quartzite, and calcareous beds. This rock has undergone extensive removal along with

the carboniferous strata which surmounts it; indeed, according to the results of the survey, has been removed from the entire uncovered archæan nucleus. Here a remarkable gap occurs; the next succeeding formation is the carboniferous, and we pass from the primordial at one step to the end of the palæozoic strata, with the striking omission of the Silurian and Devonian systems. We are then told that the carboniferous limestone overlies *conformably* this lower rock—a statement hardly credible—that its hard and resistant strata rise in conspicuous relief like an amphitheatric wall around the included and debased archæan area, with a talus of *debris* composed of its own and Potsdam fragments, piled upon its sides. Beyond the mural escarpments of the carboniferous, a trough-like valley encircles the latter formation like a moat, the bottom of which is formed of the Red Beds, probably Trias or Jura. These consist of marls and clays, sandstone or limestone bands, whose soft material has been easily and largely removed. Beyond this again, and rising from it in steep cliffs, the mechanical basis of the cretaceous is met—the Dakota Sandstone—forming the foot hills which encircle as a final group this geological unit. Beyond again stretches the plains of tertiary strata.

The history of the Black Hills, as written by this survey, is this: A low archæan area primarily, whose erasion and degradation has furnished the sands, and fragments of which the Potsdam sandstones, conglomerate, and quartzites have been formed, has been finally overlaid upon submergence with a regular but unconformable sheet of Potsdam rock. The dome thus made has been lifted from the water and for the long time from the Potsdam to the Lower Carboniferous remained dry land, not even subjected—an extraordinary statement—to considerable denudation. Then the carboniferous sea flowed over all and deposited its even floor of limestone over the Potsdam, which two, most regularly superimposed, now form the walls of the archæan inclosure, from which they have been removed by erasion.

The Triassic and Jurassic followed, surmounting the carboniferous with beds of marl and clays, and adding their accumulations to the rising mound of strata.

Lastly, the cretaceous sealed in the column of deposits so that the ideal dome assumed the form of the adjoined section after upheaval.



IDEAL CROSS-SECTION OF THE BLACK HILLS.

1. Archæan Schists.
. Granite.
3. Potsdam.
4. Carboniferous.
5. Red Beds.
6. Jura.
7. Cretaceous.
8. Tertiary.

Then the uplift occurred which brought these heavy beddings upward in a flat-topped oval displacement, a highland from which, by a process of enormous denudation, the cretaceous and the Jura and Red Beds have been pared away, their slanting beds and monoclinals now surrounding the hills. The carboniferous and Potsdam have also disappeared from the large area on the east side of the dome, where the archæan schists are exposed, and in time will retreat further and further, uncovering new portions of the azoic terrain. The carboniferous now forms the surface rock of the wide western plateau, and is deeply cut by a net-work of anastomosing cañons. A bird's-eye view of the whole presents the aspect of an overturned colossal pastry, with its bottom crust on one side badly gnawed away. In this place it would be impossible to discuss the serious questions which arise in reference to this exposition. Its guarantee is in the field work and observations of its authors, and it certainly presents a geological chapter of extreme interest.

Prof. Whitfield's important contributions in the palæontology form a striking feature in the report. Mr. Jenney reports, after a detailed examination of the mineral resources of the country, that "the Black Hills are pre-eminently a gold-producing region." Mr. Caswell contributes a chapter on the lithology of the Black Hills.

Very much of general scientific interest is found throughout this handsome volume, and the United States Government have, in its publication, added one more honor to its deserved eminence amongst nations re-organizing science, and added one more debt to the increasing sum due to it from all scientific students.

L. P. GRATACAP.

WASHINGTON, 1880.

THE GREAT PRIMORDIAL FORCE.*

By HENRY RAYMOND ROGERS, M. D., Dunkirk, N. Y.

The law of "*Conservation of Force*" having received the full and unqualified endorsement of all true scientists, is to-day the basis of all physical philosophy and the key to the explanation of all physical phenomena. No view of force can henceforth be accepted which is incompatible with it.

It may be said to be the product of the last half century, its origin being obscure and uncertain. Its earlier conceptions evinced but little promise of the grand future that awaited it, and its advancement, like that of all fundamental truths, has been exceedingly slow. It must be confessed that to-day, even, our knowledge of its provisions is but *rudimentary*. In the way of applying it to the explanation of the mysteries of nature's varied phenomena but little has yet been done. We are confident that whenever this immutable law shall be properly applied, a new era will have dawned upon physical science.

Another fundamental principle of recent discovery has been developed *pari passu* with that mentioned, and in importance is only secondary to it, viz:—the "Unity of Force,"—the correlation of all the forces. It has been demonstrated that all forms of force are resolvable into one another, it is therefore manifest that whatever name, or designation, we may give to these varied forms, but one essence pervades and animates them all. Instead of many independent forces, set forth in an irrational, contradictory, and mostly complicated philosophy, actually there exists *One Great Primordial Force*; simple in its character, competent to explain all physical phenomena, and in harmony with the nature of things. It is the force that rules the universe of matter,—innumerable star-suns and minutest atoms alike; and, for its realm, it has the vast bewildering space and all the cosmical bodies which occupy its depths.

This force is real and *substantial*. "Conservation of force proves as certainly as it proves anything, that all force is substantial. Nothing can be conserved, or preserved, unless it is something that exists, and it seems to be an axiomatic truth that nothing can exist unless it be a substance of some kind. If force in one form is convertible into force of another form, then all force in whatever form it may be exerted is substance, since it is impossible to conceive of the conversion of one thing into another thing, and neither thing be anything substantial. Our inability to take cognizance of the constituents, or corpuscles of a force, is no valid reason to a thoughtful

* Read before the A. A. A. S., Cincinnati, 1881.

mind why such force should not be regarded as a real substance—as literally and truly an entity, as is the air we breathe."

The two fundamental principles having been ascertained, our acquaintance with the essential nature of the primordial force depends upon the use we make of our positive knowledge of the specific operations of the several forms of force, as now, for the first time, we are enabled to understand and interpret them. Since we are just beginning to appreciate the great law of conservation of force and the great fact of the unity of all the forces, a better reading of physical phenomena is rendered possible. By these data we plainly read that this force is *electric* in its constitution. We claim that the electric theory is a factor in the explanation of all the phenomena.

We are able to perceive that a great celestial battery is in operation, to which the primordial force owes its perpetual development; a battery differing from our own magneto, and dynamo-electric batteries only in scale of operation. It has the same essential constitution, and *modus operandi.* Quite like the battery of man's construction, the celestial battery requires the co-operation of two elements, either of which, acting independently, is incapable of producing force, but acting in concert, the two develop all known forces of the universe. These two elements which have never yet been adequately associated in the interpretation of the phenomena of gravity, heat, light, etc, are *motion* and *magnetism;* the motions of the celestial spheres, and the magnetic constituent of each celestial sphere. True, the scientific world has vaguely conceived of the development of power through the agency of celestial motions, but it has failed to formulate any *exact philosophy* of the manner in which those motions should develop actual force. Such a philosophy is accessible.

It is known that the earth is a vast magnet, having a magnetic axis nearly corresponding to its geographical axis, and terminating in a *positive,* or northern pole, and in a *negative,* or southern pole. But the earth's inherent magnetic force must remain purely *potential,* or *static,* and without the ability to act, or to move, except through the co-operation of a separate and independent force, whereby this static magnetism may be converted into active, vital currents. From our experience with the electric battery, we know that motion, and change of polarity, are capable of producing vital, active force, from force inert and dormant. In the celestial, as in the terrestial battery, motion and change of polarity are prerequisite to the awakening of inert, dormant force, into vital, active force.

That electricity may be obtained from magnetism by motion, or that magnetism, conjoined with motion, may be made the source of electricity, is well understood. So, by their unceasing rotation the magnetic sun and earth furnish both the needed motions, and the incessant change of polarity, which are requisite to the completion of the celestial battery. Thus, through the medium of revolving celestial armatures, acting upon celestial magnets, *statics* are converted into *dynamics,* on a scale commensurate with the demands of the universe; and that dream of the ages, *perpetual motion,* is found a realized fact in nature.

All space being pure vacuum, distance is virtually annihilated, and all the spheres are brought relatively into close proximity, Mercury, 37,000,000 of miles from the sun, and Neptune 2,800,000,000 of miles, stand alike in their relation with the great central orb. This condition is thus favorable to such "*action at a distance,*" as is found *indispensable* to the inter-communications going on between the bodies which constitute the universe of matter. The fact of instantaneous action at a distance, and without a medium of transmission, though questioned, is demonstrated in the action of gravity.

Through the operation of the celestial battery a great current of force is incessantly in motion from the sun to the earth. Both the law of conservation, and the law of magnetic action, demand that a current of equal proportion shall as incessantly return from the earth, to the sun; —the one current cannot for a moment exist without the other. Were this return current interrupted, the light of the sun would instantaneously cease, all life would become extinct, and, the power of cohesion being suspended, the earth and everything thereon would return to their original elements, and vanish into space. In this action, and retro-action, we find a grand magnetic circuit between the earth and sun, through the operation of which the earth's portion of the primordial force is utilized. The same conditions which exist between the sun and earth, as described, exist also between every satellite and its primary, and between the star-suns of the universe.

SOLOMON was wiser in some respects than the scientists of the present day. He says that "The wind returneth again according to his *circuits.* All the rivers run into the sea, yet the sea is not full; unto the place from whence the rivers come, thither they return again." Science recognizes the immense movement of force from the sun to the earth,—but has she made sufficient account of the fact that the earth is not full? Has she recognized the fact that back to the sun again, in some form or other, the rivers of energy must return in ceaseless circuit? And yet, the law of conservation demands it, and the electrical theory satisfies the demand.

The various manifestations, or affections, of the primordial force which receive the appellation of the great physical forces have their fullest and clearest explanation through the interpretation given by the electrical theory.

GRAVITY CONSIDERED AS AN AFFECTION OF THE PRIMORDIAL FORCE.

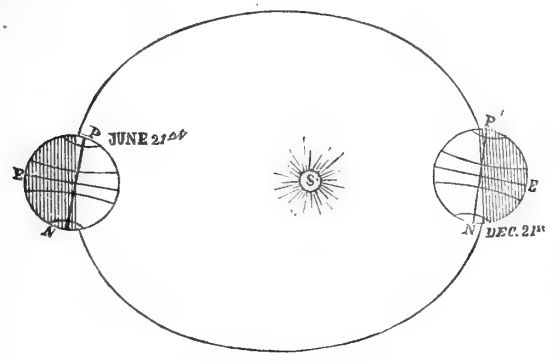
Science has strangely neglected to examine the field which gives best promise of an ultimately satisfactory explanation of this phenomena.

According to the present popular philosophy, "gravity acts directly as the mass, and inversely as the square of the distance." FARADAY says, "The received idea of gravity appears to me to ignore entirely the principle of the conservation of force, and, by the terms of its definition, if taken in an absolute sense, 'varying inversely as the square of the distance' to be in direct opposition to it."

If gravity acts directly as the mass, then surely, as the sun's mass is *constant,* and the earth's mass is also *constant,* the force of attraction should at all times remain invariable, and the earth's orbit should consequently be a true circle, instead of an ellipse. If gravity acts inversely as the square of the distance, then the earth at aphelion could not without the aid of some other force or interfering cause, be returned to perihelion, but the lessening attraction would permit it to move on indefinitely into space.

The electrical theory, however, suggests a more rational, clear, and satisfactory philosophy, and makes provision for the elliptical orbit of the members of the solar system. In order that the earth's orbit, for example, should be elliptical, the mutual attraction existing between the sun and the earth must necessarily increase and diminish with mathematical exactness and regularity. To such *regularly varying attraction,* and to no other cause, can the ellipticity be due. Attraction is greatest about the 21st of December when the two bodies are nearest each other, and least about the 21st of June, when they are relatively the most distant. Whether the attraction between the sun and earth shall be greater, or shall be less, depends altogether upon their relative position. Thus when the earth's south-pole presents nearest the sun, the attraction between the two bodies is the greatest, and when the earth's north-pole so presents,

attraction is least. Why should this variation in position produce such a variation in attraction? In the light of the electrical theory the following explanation is confidently advanced:



On the 21st of December the *positive* sun, (S.) and the *negative* south-pole of the earth's magnetic axis (N.) are in closest relation to each other, and the north-pole, (P.) is out of the field, therefore the opposing conditions, viz: the *positive* sun, and the *negative* portion of the earth, represented by the south-pole, acting in concert, produce attraction between the two bodies, according to the electrical law that *unlikes* attract each other. At that date the centre of the great electric sun-current strikes the earth at a point 23½ degrees south of the equatorial line, and from thence moves in the direction of its mass towards, and along the earth's natural magnetic axis; in this instance, towards the north. This northerly movement of the electric mass is *concurrent* with the earth's magnetic axis, and the force of attraction between the two bodies is thereby increased to its maximum.

On the 21st of June precisely opposite conditions exist: the *positive* sun, and the *positive* north-pole, are in closest relation to each other, and the south-pole is out of the field; attraction between the two bodies is consequently at that date lessened, and this in accordance with the electrical law that *likes* repel each other. And, too, the centre of the great sun-current strikes the earth at a point 23½ degrees north of the equator, and its mass moves in a southerly direction. The direction of this electrical movement is *contrary* to the earth's natural magnetic axis, and the force of attraction between the two bodies is further lessened; consequently at that date they are found at their greatest distance apart,—viz: several millions of miles more distant than on the 21st of December.

On the 20th of September, and the 20th of March, the sun is equidistant from the earth's two polar extremities, and the centre of the great sun-current strikes the earth at the equator in a direction at right-angles with the earth's magnetic axis. The electrical conditions are thus balanced, and the earth at those periods is equally distant from the sun.

The degree of ellipticity of each planetary orbit is due to the inclination of its axis: if the axis is at right angles with the plane of the ecliptic, the poles must be equidistant from the sun, and attraction and repulsion thus become equalized, and the orbit must necessarily be circular. Had the axis of the earth been perpendicular to the plane of the ecliptic, the sun would always have appeared to move in the equator, the days and nights would have been equal, and there could have been no change in the seasons.

The extent of ellipticity in any orbit is governed by the amount of axial divergence from such right-angle.

As a further demonstration of the ability of the electrical theory to account for the grand phenomena of the universe, we will apply it to the philosophy of the earth's axial motions. Electricity and Magnetism are regarded by scientists at the present day as virtually identical. The correlation of heat and magnetism is apparently as pronounced, as witness certain natural phenomena which bear directly upon this point. The tropical plant, the *phytolacca electrica*, is known to produce marked electrical effects; a touch of a twig gives to the hand as vivid a shock as that of a Rumkorff battery. At the distance of seven or eight paces, the influence of the plant is manifested through a compass needle, the closer the proximity, the more marked are the demonstrations; and if the compass is placed in the centre of the bush, the movement of attraction previously shown by the needle is changed into that of rapid rotation. The intensity of the phenomena varies with the time of day. At two o'clock P.M. it attains its maximum, and at night its magnetic powers are scarcely perceptible. It is thus demonstrated that at precisely the same hour, viz: two o'clock P.M. *heat* intensity, and *magnetic* intensity are *co-incident*. From this hour each diminishes, and from the morning until two o'clock P.M. each increases in the same proportion. The hour of the least magnetic effect, or the most *negative* condition, is shown by the following phenomenon to occur at a period of time opposite to that of its maximum, or *positive* condition, viz: two o'clock A.M. It is the experience of miners whose lives are passed in the depths of the earth, that between twelve and two o'clock in the night, if there is a loose stone or bit of earth in the mine it is sure to fall. Says a miner of many years experience: "About this time it seems that everything begins to stir, and soon after twelve o'clock, although the mine has been as still as the tomb before, you will hear particles of rock and dirt come tumbling down, and if there is a caving piece of ground in the mine it is sure to give way."

From these and such like familiar suggestions on the part of nature we may infer that the portion of the earth which is at any given time specially under the action of the great sun-current, becomes *electro-positive*, the maximum intensity occurring at two o'clock P.M. During the night the magnetic condition changes, and is at two o'clock A.M. most *electro-negative*. Thus at two o'clock P.M. the sun on the one hand, and that portion of the earth on the other hand, being in like electrical conditions, viz: *electro-positive*, mutually repel each other, and the consequent *push* moves the earth in revolution. The revolving earth turning eastward, is continually carrying its *negative* condition of the night into the field of the *positive* sun, a mutual attraction therefore takes place with its consequent *pull* upon that side; and thus is generated the process of an incessant attraction on the east side, and of repulsion on the west side, giving to the earth its axial motion.

Gravity may therefore legitimately be claimed as purely an electrical phenomenon.

The words of the grand old FARADAY now stand forth in lines of light, viz: Gravity, surely this force must be capable of an experimental relation to electricity, magnetism, and the other forces, so as to bind it up with them in reciprocal action, and equivalent effect.

SUN HEAT, *as an Affection of the Great ONE-FORCE.*

The demonstrations of our senses, as well as the teachings of all the ages, lead us to attribute to the sun the possession of a most dazzling brilliancy, an unlimited amount of heat. So it certainly appears. Yet the simple fact that the earth receives its heat through the agency of the sun, is not conclusive evidence that the sun is itself hot. On the contrary, it is well known that heat rapidly diminishes in the direction of the sun, and that at the altitude of considerably less than three miles lies the line of perpetual frost, the temperature of space progressively lowering beyond that point. The space therefore which separates the earth from the sun, 93,000,000 of miles in extent, is inconceivably cold; its intensity is variously estimated at from *minus* a few hundred degrees, (Fahr.) to

minus 18,000,000 of degrees. It seems incredible that scientists who possess the knowledge of these facts can entertain the thought, or attempt to maintain the theory, that heat comes from the sun, *as heat*, through such a distance, and such a medium. And, if not coming as heat, then the previously existing philosophies of the functions of the sun are *fundamentally erroneous*.

Science at the present time admits of four different explanations of the production of sun heat, viz: (1.)combustion of cosmical substances falling into the sun; (2.) arrest of motion of substances thus supposed to fall into the sun; (3.)contraction of the solar mass; and (4.) dissociation of compound bodies in the sun's substance. Each of the foregoing hypotheses stands in direct opposition to the inexorable law of conservation of force. Each recognizes the presence of a vast flood of heat, light, and force incessantly issuing from the great solar mass, and proceeding therefrom with inconceivable velocity to the earth. Yet neither of them makes provision for the retro-acting, or returning current, which under the law of conservation, becomes indispensable. Each assumes the actual and indispensable presence of heat at the sun, as an element in the solar economy. But inasmuch as heat cannot come from the sun *as heat*, there really exists at the sun no necessity for the enormous production there, such as these hypotheses demand. The prodigious destruction of material claimed to be involved in the production of heat at the sun, and the expenditure of an inconceivable amount of force in projecting the same in all directions, and to inconceivable distances into space, are uncalled for, and therefore irrational. Upon the electrical theory, no such extravagant and irrational processes are needful.

A true understanding of the great physical phenomena of our earth depends upon a correct knowledge of the constitution of its atmosphere. There is a more vital element than clouds, vapors, gases. This constituent is magnetic in its character, and may be designated as *static*, from its habit in equilibrium, and also in contradistinction from the vast *active* current which fills the space between the sun and the earth. In all scientific formulæ of its constitution, this principle as a real entity has been ignored. The fact that the atmosphere is a vast magnetic reservoir, that it is the most magnetic of all earthly bodies except iron, nickel, and cobalt, is well understood; yet there appears to have been no suspicion of the grandeur and importance of its functions in the earth's physical economy. As constituted, the atmosphere is peculiarly adapted to co-operate with the sun in this economy.

In fact it is the medium and instrument of all the sun's terrestrial operations. Prepared by such knowledge of the constitution of the atmosphere, we can better comprehend the philosophy of the action of the great sun-current so incessantly moving earthward.

By means of the dynamo-electric machine, it is demonstrated that motion or magnetism, (or both,) is converted into heat and light; so does not analogy suggest that the grand motions of the heavenly spheres are by the same principle converted into sunlight, and sun-heat; thus making unnecessary the measureless and ceaseless destruction of material that is demanded by the present theory? Our atmosphere supplies the conditions represented by the "carbon point," and the "platinum coil," in practical electricity. A current invisible, without manifestation, passes through space, as electricity through wires, until, meeting the resistance and favorable conditions of our atmosphere, there occur those wonderful and important phenomena, heat, and light. No particle of either heat or light need therefore come as such from the sun to the earth, the current being wholly invisible and cold in its passage. For a practical demonstration of such transmission of a current, we are much indebted to experiments made by Prof. MAHLON LOOMIS, of WASHINGTON, D. C. Without any visible means of transmission, he succeeded in sending the magnetic current from one mountain top to another twenty miles distant.

In the light tenuous atmosphere of the summits of lofty mountains, the human body often experiences the fiercest effects of sun-heat, and the pyrheliometer of POUILLET also records such effects for the reason that the body and the instrument become *objects of resistance* to the current, and a local heat is thus developed, which is far greater than that of the light atmosphere surrounding, which offers no such resistance. It is hardly necessary to add that the greater heat always manifested on the surface of the earth beneath is owing to the fact of a denser medium, and a consequent greater resistance.

The battery of mundane construction—our best aid and interpreter in the reading of universal phenomena—while it is the developer of heat, light, and power, is itself neither luminous, hot, nor magnetic. To explain the effects of the sun, therefore, there is not the least reason to infer that it is itself luminous, or even warm. Potential action generated in a dark, cold body may produce great heat, light, and attraction, at a distance from the seat of activity, and what is thus wrought artificially, in a small way, may surely be done naturally, and in a tremendous fashion, by the grand forces of the sun.

SUNLIGHT.

The same process develops sunlight. If lines be drawn from the sun to the earth, tangent to both, these lines will enclose a tapering space, the sun at the big end, the earth at the small end, and the space between a truncated cone; this space may be designated the solar cone or cone-space. Within this space incessant circulation is going on, and all the phenomena of gravity, heat, light, are produced through their reciprocal activity. The field of encounter between the forces of the sun and earth is our atmosphere, and in the collision light is generated. Being thus conditioned upon the atmosphere, light and heat cannot be found in space beyond the lines of the solar cone.

It is to be observed that light rapidly diminishes in the direction of the sun, even as we have seen to be the case with heat. Beyond the lower portion of the atmospheric mass, there is no dazzle; and the human eye in looking upon the great orb is not dazed. Thus the exceeding brilliancy which characterizes the sun's rays, so far from being a phenomenon located in the sun itself, as is the popular, and even the scientific conception, is actually confined to the lower strata of our atmosphere.

If light were transmitted to us from the sun in perfect intensity, the entire vault of heaven must appear as luminous as our sun.

The sun is therefore not the manufacturing place and distributing reservoir of *actual* light and heat; it is rather the source from whence the whole solar system is supplied with the *invisible*, *potential* light and heat, which become developed where it is required. The great central orb may therefore be regarded as like unto the earth, on its surface, and in its surroundings, viz: a dark, cool, habitable body.

OTHER RECOGNIZED FORMS OF FORCE.

All other recognized forms of force have their best explanation in the same theory.

It may be necessary to caution against a natural misconception which is fruitful in seeming objections against the theory advanced. Let it be understood then, that man's machinery must always work to disadvantage when made to illustrate the operations of nature. The machine itself is an inert thing, a dead weight interposed, the working of which requires the expenditure of force. In nature's operations, on the contrary, without labor or friction, one form of force under proper conditions transmutes itself into another form, there being no loss of force in the change. We must recognize the fact of an unimpaired energy in the universe, the fact that force is never lost nor wasted.

The Great Primordial force owes its genesis to the initial impulse which set all spheres in motion in vacuous space. To this universal principle, not only all physical force, but new life itself is due.

HUMBOLDT says: "It is indeed a brilliant effort, worthy of the human mind, to comprise in one organic whole the entire science of nature, from the law of gravity to the formative impulse in animated bodies."

That the earth, and the sun, and all the heavenly bodies, are possessed of the mysterious magnetic energy, and consequently exert a powerful magnetic influence over each other, has long ago been conceived by such men as HERSCHEL, HUMBOLDT, FARADAY; and is the faith of scientists to-day. But when we have arrived at such a conclusion, it is impossible for us to stop short, and not make the necessary deductions therefrom. Mighty magnets, when involved in mighty motions, must produce mighty currents and mighty effects. It is not for nothing that these powers and conditions exist. If we admit the premises, we must not ignore the conclusions that are necessitated. Provision must be made for the outcome of every admitted fact in science.

Therefore, it is with assurance that we urge the electric theory, and maintain that the burden of proof rests with those, who, admitting the elements of motion and magnetism, have yet made no provision whatever for their keeping.

Besides, there are two other principles already alluded to—the conservation of force, and the unity of all the forces—with respect to which it may be demanded, to what other result do they lead, and can they lead, in all reason and logic, than to the admission of the grand fact of a Great Primordial Force.

THE TIDAL EVOLUTION OF THE MOON.

On Saturday, June 4th, in the Museum Buildings, Trinity College, Dr. Ball, Astronomer Royal for Ireland, delivered an interesting and instructive lecture on recent discoveries in astronomical science. Dr. Ball said that from the variety of topics which might fairly be dealt with in his lecture he would select three, and in making this selection he had been mainly guided by the relative importance of different astronomical problems. He had also endeavored to exercise his choice so that his lecture should, as far as possible, refer to the various branches of astronomy. Having dealt with two branches of his subject, Dr. Ball described "Darwin's Theory of the Tidal Evolution of the Moon." It had, he said, been the triumph of modern gravitational astronomers to indicate the changes which must be going forward in a system devoid of rigidity. It was at all events easy to show that the tendency of these changes lay in one direction, and this was the most important point for consideration. Everyone was aware of the daily movements of the sea, which were called the tides. Most people were aware that the movements of the waters were caused by the attraction of the sun and the moon. Let them ponder therefore on the tides, as they seemed to give a clue to some of the profoundest of nature's secrets. He had heard that the port of Dublin was gradually being improved by the deepening of the bar. He had heard that the deepening of the bar had been attributed to the judicious action of the Port and Docks Board. But what the board had chiefly done was to call into requisition the scouring power of the tide, which, as he was informed, was gradually reducing or bearing away the bar. The tide was therfore accomplishing, at the bar of Dublin, the same kind of work as could be accomplished by men or by steam-engines. In other words, the tide was here doing a useful work that could otherwise only be done by the expenditure of energy. It was the same elsewhere. The tides were doing work useful or the reverse, and expending energy in so doing. Where did the energy come from? It could not be created. It could only come from the store of energy available for such purposes in the solar system. The reserve energy whence the tides drew the supplies they were daily consuming consisted partly in the daily rotation of the earth on its axis. The earth was like a mighty flywheel which would absorb a prodigious amount of energy in setting it in motion, and which would give out that energy before it would be brought to rest. The rotation of the earth on its axis was a vast but not inexhaustible storehouse of energy, on which the tides could draw for thousands of years. Energy also existed in the solar system in many other forms, some of which could also be rendered available for the tides. So far as was known, the total amount of energy could not be increased. The important question was—Can that total ever be diminished? The tides were diminishing it every day. The small oceanic tides were not the sole source of the expenditure. The solid body of the earth itself must be subject to tides; still more must the fluid or gaseous members of our system be subjected to tides. All tides involved friction, and all friction involved loss of energy. Here, then, was the great discrepancy between the theory of Lagrange and the actual condition of our system. Lagrange's calculations assumed that the total energy of the solar system was constant, but the actual fact was that the energy was slowly diminishing. The tracing of tidal evolution was chiefly due to the labors of Mr. G. H. Darwin, son of the celebrated naturalist. The influence of the tides had already been recognized as the cause of the same face of the moon being always bent on the earth. Whether the tides were merely oceanic, or whether they were actual bodily tides, the results remained much the same. At the present time the moon revolved around the earth in a month: the earth revolved on its axis in a day. The tides produced in the earth by the moon must act to reduce the rate of the earth's rotation. The effect of the tides on the earth was to lengthen the day. The day was gradually lengthening, but this change could not take place without a reactionary change on the moon. The change undergone by the moon was perhaps a little difficult to understand, as it depended on some by no means simple dynamical principles. The friction of the tides consumed the energy of the system. It turned a large portion of that energy into heat, which was then radiated off into space to be forever lost. But the friction of the tides could not alter the moment of momentum of the system. As the earth became gradually slower and slower in its rotation its moment of momentum decreased, yet for this to happen the moment of momentum of the moon should increase. It followed mathematically that as the tides gradually made the earth rotate more and more slowly, the moon must be getting farther and farther away from us. At the end of a million years from the present time the day will be more than one day of twenty-four is now; and in one million years hence the moon will move round the earth at a greater distance than she does now, and the length of the month will be correspondingly increased. In the far distant future therefore, we are to look for an increased length of month. The length of day will, however, increase much faster than the length of the month, until at length the duration of the day equals that of the month. When this time arrives the moon will have moved out to a distance half as great again as it is at present, and the length of the month will have increased to two months. Our day will then have increased from twenty-four hours up to nearly two months. and as the moon continues to to show the same face to us, we are destined to turn the same face on the moon. Were the earth and the moon the only bodies in the universe, such a state of things might go on forever. The sun, however, will produce tides in the earth which will again modify their movements. He had said that the moon was gradually receding farther and farther from the earth, and that the

length of the day was increasing—getting gradually longer and longer. But how long has this been going on? Yesterday was shorter than to-day. The day which Homer had was shorter than our day, but not indeed to any appreciable extent. There can be no doubt, however, that a million years ago the day was appreciably shorter than the day is at present. He wished to conduct them back to an exceedingly remote period, to a critical epoch in the history of the earth. That epoch must have been more than fifty millions of years ago, but how much more he could not tell. At that extremely remote time the day was greatly less than it is at present. It was only, indeed, a fraction of its present amount, being only from two to four hours long. He would trace back the moon to the same remote epoch to which he had conducted the earth. The tides in the earth are forcing the moon gradually away from us at present. The moon was therefore formerly nearer to us than it is now. Millions of years ago the orbit of the moon was much less than it is at present. The time of the moon's revolution was much smaller and the moon must have been quite close to the earth, and whirled round the latter in a period of from two to four hours equal to the period of the earth's revolution on its axis. Such, then, is the primeval condition of things to which the tracing of tidal evolution conducted. Antecedent to this critical epoch they could hardly go with any degree of certainty. After explaining Darwin's theory in reference as to the supposed rupture of the earth at a very remote period of time, and the consequent formation of the moon, the lecturer proceeded to speak of the surprise with which astronomers realized that the small interior satellite of Mars revolved on its axis in less than a third of the time —nearly 24 hours—which the primary occupied in revolving on its own axis. He also spoke of the tremendous forces in action at remote periods when tides rose to a height of a thousand or two thousand feet, scouring rocks and carrying enormous quantities of matter to the sea, and when that action caused so much comparatively rapid manufacture of strata.

MR. DARWIN ON DR. HAHN'S DISCOVERY OF FOSSIL ORGANISMS IN METEORITES.

Dr. Hahn's discovery, of which an elaborate account was given in No. 50 of SCIENCE, has stirred up a lively discussion of this highly interesting subject. Dr. Hahn has taken steps to enable Prof. von Quenstedt, the renowned Tübingen geologist, and all others who expressed the desire to examine his microscopic preparations. It is understood that all those who have availed themselves of the opportunity thus offered have become convinced of the genuineness of Dr. Hahn's discovery.

It is very interesting to note the position taken by the greatest of living evolutionists in this controversy, if it can still be called such. Charles Darwin, on receipt of Dr. Hahn's work, wrote to him:

" . . . It seems to be very difficult to doubt that your photographs exhibit organic structure . . . ," and furthermore:

" . . . your discovery is certainly one of the most important."

Not content with the mere presentation of his work, Dr. Hahn visited the veteran zoologist and brought his preparations to him for inspection.

No sooner had Mr. Darwin peered through the microscope on one of the finest specimens when he started up from his seat and exclaimed:

" Almighty God! what a wonderful discovery! Wonderful!"

And after a pause of silent reflection he added:

" Now reaches life down!"

The latter remark no doubt refers to the proof furnished by Dr. Hahn's discovery that organisms can reach our planet from celestial space. It is an acknowledgment of the relief Mr. Darwin must have felt in not being forced to a belief in a primeval "*generatio equivoca*."

As was suggested in the paper referred to, "the Richter-Thomson hypothesis of the origin of life on the earth has become a tangible reality!" R.

AN AFTERNOON ON PASSAIC RIVER.

On the 25th day of last month the editor, in company with his former colleague on the *Quarterly*, Mr. J. L. Wall, escaped from the city and made a trip to the town of Belleville, on the Passaic River. A row-boat was engaged, and we proceeded to collect specimens from along the shores. Not many species of algæ were found, nor was there any great variety of animal forms, but the water-plants, so hardy and useful in aquaria, the *Anacharis Canadensis* and *Vallisneria spiralis*, were abundant. Reaching over into the shallow water, it was an easy matter to obtain perfect plants of *Vallisneria* with good roots, and we collected a number of them. The *Anacharis* grows so readily without roots that the more fresh looking stems were carried home without regard to the roots. An old can was made use of to carry home some of the river mud, in which to plant the *Vallisneria*. The mud was placed in the bottom of a tall specie jar, the roots of the plant were properly embedded, and the jar filled with water. The next morning, after the water was cleared by settling, the mud was covered with a layer of clean sand, which tends to prevent riling of the water by a slight disturbance. All the leaves of the *Vallisneria* were removed, so that a new growth might start in the aquarium. It is probable that we will thus obtain some vigorous plants of *Vallisneria* for use during the coming winter. The *Anacharis* was simply thrown into a large aquarium, where it will doubtless grow without further care. Rowing about slowly, a long, green, spiral filament was observed reaching up to the surface of the water. It was two or three feet in length, and bore a peculiar flower at the end. This was the female flower of *Vallisneria*, a very interesting object for study; it was quite a surprise to us, as the plant does not usually flower as early as July. Looking toward the shore, the water was covered with an innumerable quantity of white specs, which attracted our curiosity. Rowing up to them, we found that they were the male flowers of *Anacharis*. These are very curious flowers. The long, tubular perianth, sometimes two or three inches in length, reaches from the axil of a leaf to the surface of the water, and bears the stamens above. It would easily be mistaken for the flower-stem, but it is really the tubular perianth. These flowers were very abundant, so that the water appeared white with them. The pollen-grains were numerous, and could be seen floating about on the water in little clusters resembling snow-flakes. *Potamogeton* was abundant, in several forms, and the common arrow-plant, so named from the shape of the leaf, *Pontedaria cordata*, which is also good for large aquaria. This plant should be set in a flower-pot, with suitable soil in which to root, and then submerged, either wholly or in part.

Among the algæ, two species of Oscillaricaceæ were found quite actively moving *Oscillaria tenius* and *littoralis*, and *Lyngbya majuscula*. The most interesting specimen of all, however, was a species of *Ulothrix*, a very common, filamentous, green algæ, in which the cells are about as long as they are wide. It was interesting because when we examined it, at about seven o'clock the next morning, the process of giving off swarm-spores had just begun. The entire contents of each cell in whole filaments, quickly formed into green, spherical masses, which began to move about in the confined space within the cells; soon the cell-walls ruptured, and the contents escaped as very active swarm-spores, somewhat elongated in form, and furnished with four long, whip-like appendages, or flagella, by means of which they could

swim about. They measured $5.\mu 8$ to 8μ (.00022 to .00033-inch in diameter). After a while they become attached to some object, lose their flagella, elongate and subdivide, forming new growths of *Ulothrix.—The American Monthly Microscopical Journal.*

SELENOGRAPHICAL.

For the purpose of comparing drawings of lunar objects, it is proposed to circulate at frequent intervals, among observers, a portfolio containing sketches and descriptions of various formations, which will ultimately be presented to the Selenographical Society. To cover expenses, an annual subscription of 2s. 6d. will be required. Among those who have already signified their intention of joining in the movement are Rev. F. B. Allison, Mr. W. R. Birt, Mr. T. P. Gray, Rev. R. S. Hutchings, and the Rev. Dr. Richards. Those who are willing to add their names to the above list are requested to communicate with the editor of "SCIENCE."

BOOKS RECEIVED.

SEA MOSSES. A Collector's Guide and an Introduction to the Study of Marine Algæ, by A. B. HERVEY, A.M. S. E. Cassino, Boston, 1881.

We welcome this excellent book, published at a seasonable moment, which will make it doubly appreciated by the public.

To the thousands who are now making a temporary home within the sound of the surf and who love the sea, seeking its presence for rest of spirit or health of body, the present work will be found a welcome companion and guide, opening up a new channel for the pleasant passage of leisure hours. No longer need the idler watch the incoming and outgoing of the tides with listless indifference, or be weary of the beating of sleepless waves, as they go tumbling among the rocks.

The author prepares the way for another pleasure which "this great and wide sea" can give us, besides that which she offers to our fancy and our dreams. In the contemplation and study of the exquisitely beautiful flora which she nurtures in her ample waters.

If you become acquainted with these plants, their beauty, delicacy and grace, and know their names, habits and history, you will admit the sea has added a new charm to your existence.

There may be no royal road to knowledge, but Mr. A. B. Hervey has certainly selected the shortest and most agreeable path by which the tyro may acquire a practical knowledge of the department of Cryptogamic Botany, included in the study of the most beautiful of Marine Algæ, the Sea Mosses.

The publishers have done justice to Mr. Hervey's work, and have produced a handsome printed book of nearly 300 pages, with twenty full-page colored illustrations of the most beautiful of the Sea Mosses, which will be found of great value to the student engaged in these studies.

No person of intelligence residing within reach of the sea, should remain without a copy of this work.

PROF. S. P. LANGLEY has made the following calculation:—A sunbeam one square centimeter in section is found in the clear sky of the Alleghany Mountains to bring to the earth in one minute enough heat to warm one gramme of water by 1° C. It would therefore, if concentrated upon a film of water 1-500th of a millimetre thick, 1 millimetre wide, and ten millimetres long, raise it $83\frac{1}{3}$ in one second, provided all the heat could be maintained. And since the specific heat of platinum is only 0.0032, a strip of platinum of the same dimensions would, on a similar supposition, be warmed *in one second* to 2603° C.—a temperature sufficient to melt it!

NOTES.

FAURE batteries are now made with flat plates, the rolling up of the sheets having been found to produce many cracks in the minium.

FROM exact experiments, M. Mascart finds that the intensity of current capable of producing in one second the electrolysis of the equivalent of a substance expressed in milligrammes is equal to 96.01 webers.

REMSEN has again investigated the action of finely-divided iron in inducing the formation of cyanide when nitrogen is passed over a hot mixture of carbon, iron, and an alkaline metal; he finds that freshly reduced iron induces a large formation of cyanide, but that iron after keeping for some time loses this power.

THE PHYSIOLOGICAL EFFECTS OF MATÉ.—Maté, or Paraguayan tea, is known to be extensively used in South America, and almost universally in Brazil, the common practice being to pour boiling water on some of the powder (consisting of ground leaves and twigs of certain species), then to suck the infusion through tubes provided with strainers. MM. d'Arsonval and Conty have recently inquired into the action of this substance, administering it to dogs, either by injecting into the veins or by introduction into the stomach, and they have observed a remarkable effect of it on the gases of the blood. It dimishes the carbonic acid and oxygen both of the arterial and of the venous blood to a large extent, sometimes a third or even half of the normal quantity. This action, which is less intense during digestion, and has no necessary relation to phenomena of excitation of the sympathetic nerve-system, is somewhat obscure as to its "mechanism," but its existence proves directly the importance and nutritive value of the aliment in question, which, consumed in such large quantities in South America, is almost unknown in Europe.

PROF. IRA REMSEN, of the Johns Hopkins University, Baltimore, has been lately experimenting as to whether the chemical behavior of a metal is in any way influenced by magnetic action, and has obtained some interesting results. The best effects were got by placing a shallow, thin iron vessel holding copper sulphate solution over the poles of a magnet. Out of the magnetic field the solution would deposit a uniform coating of copper, but in the field the lines marking the outlines of the poles were sharply distinguished as depressions in the deposit. In this case a permanent magnet was used capable of supporting 55 lbs. With an electro-magnet still more striking effects were observed. There was no deposit of copper on a narrow space marking the outline of the poles. Within this the deposit was fairly uniform, but outside the copper was deposited in irregular ridges running at right angles to the lines of force, and apparently coincident with the lines marking the equi-potential surfaces. By increasing the power of the electro-magnet, the action is intensified, and the area affected is broadened. The cause of the phenomenon has not yet been elucidated.

PROF. E. LOMMEL describes in *Wied. Ann.* a new polarising apparatus in which two plates of platinocyanide of magnesium, cut perpendicularly to the optic axis, are used as polariser and analyser, just as in the tourmaline pincette. Such a section of this crystal transmits a blue light, which, when the angle of incidence exceeds 2°, it is found to be perfectly polarised in the plane of incidence, and it therefore can be used, if tilted to that extent out of perpendicularity to the axis, as a polariser for a pencil of parallel blue rays. One curious point in respect to the behavior of thin film thus prepared is the following: Let ordinary non polarised light be looked at through the crystal while the latter is normal in the line of sight. A white central spot, perfectly circular in form, and non-polarised, is observed in the middle of a blue field, which is polarised at every point radially. The only other crystals which can be used for polarising pincettes are the tourmaline and herapathite (iodo-sulphate of quinine); the point of difference between these and the platino-cyanide of magnesium is that while the two former (which are negative crystals) absorb the ordinary ray, and must therefore be cut parallel to the optic axis, the latter absorbs the extraordinary ray, and must therefore be cut at right angles to the optic axis.

A CONTINUOUS registering thermometer for recording the temperature of the body has just been described by its inventor, M. Marey. It consists of a brass tube communicating with a Bourdon manometer, containing oil, and closed. Any change of temperature, by altering the internal pressure, makes the curved manometer tube curl more or less, and to it is fixed an index which registers the movements by inscribing them on a recording cylinder. The thermometric bulb may be at some distance from the inscribing apparatus, being connected by a flexible tube of annealed copper. Two such bulbs may be applied to different parts of the body, even to the interior. It is possible therefore to note the relation between the temperatures of the interior and exterior of the body. If we remember rightly, an analogous but more portable instrument was suggested some time ago by Mr. Donald Macalister, but we are not aware whether his instrument is yet before the public.

SUN SPOTS.

The following record of observations made by Mr. D. P. Todd, Assistant, has been forwarded by Prof. S. Newcomb, U. S. Navy, Superintendent Nautical Almanac Office, Washington, D. C.

DATE, JUNE, 1881.	No. of New		Disappeared by Solar Rotation.		Reappeared by Solar Rotation.		Total Number Visible.		Remarks.
	Groups.	Spots.	Groups.	Spots.	Groups.	Spots.	Groups.	Spots.	
7, 5 p. m.	1	2	-----	-----	1	2	3	9	
11, 7 a. m.	2	20	-----	-----	-----	-----	4	40†	
12, 9 a. m.	2	5	1	3	1	2	5	45†	
15, 9 a. m.	2	10	1	5	1	3	7	50†	
16, 7 a. m.	0	0	0	0	0	0	7	40†	
18, 7 a. m.	0	0	2	10	0	0	5	25†	
19, 2 p. m.	0	0	0	0	0	0	4	15	
21, 10 a. m.	0	0	0	0	0	0	3	10	
22, 10 a. m.	0	0	0	0	0	0	2	5	
23, 8 a. m.	2	5	0	0	2	5	4	10	
24, 5 p. m.	0	7	0	0	0	0	4	17	
26, 1 p. m.	1	25†	1	2	1	15	4	40†	
29, 8 a. m.	1	25†	0	0	1	10	5	65†	Many of the spots small.
30, 9 a. m.	0	0	0	0	0	0	5	60†	Many of the spots small.

† Approximated.

Faculæ were seen at the time of every observation.

Published by order of the Secretary of War.

W. B. HAZEN, *Brig. & Bvt. Maj. Gen'l Chief Signal Officer, U. S. A*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING AUG. 20, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

AUGUST.	BAROMETER. Mean for the Day.	Maximum.		Minimum.		THERMOMETERS. Mean.		Maximum.				Minimum.				Maxi'm
	Reduced to Freezing.	Reduced to Freezing.	Time.	Reduced to Freezing.	Time.	Dry Bulb.	Wet Bulb.	Dry Bulb.	Time.	Wet Bulb.	Time.	Dry Bulb.	Time.	Wet Bulb.	Time.	In Sun.
Sunday, 14	29.686	29.778	12 p. m.	29.596	0 a. m.	71.3	64.0	78	3 p. m.	71	0 a. m.	66	12 p. m.	62	12 p. m.	140.
Monday, 15	29.902	29.986	12 p. m.	29.778	0 a. m.	69.6	63.6	76	3 p. m.	66	2 p. m.	63	5 a. m.	60	5 a. m.	140.
Tuesday, 16	30.031	30.062	10 p. m.	29.986	0 a. m.	66.3	60.7	71	1 p. m.	62	1 p. m.	59	12 p. m.	57	12 p. m.	134.
Wednesday, 17	30.031	30.064	9 a. m.	30 000	12 p. m.	60.3	58.0	67	4 p. m.	60	5 p. m.	56	5 a. m.	55	5 a. m.	115.
Thursday, 18	29.919	30.000	0 a. m.	29.890	12 p. m.	63.6	60.0	67	3 p. m.	62	10 p. m.	57	1 a. m.	57	1 a. m.	98.
Friday, 19	29.786	29.890	0 a. m.	29.718	12 p. m.	67.0	63.6	70	3 p. m.	65	3 p. m.	64	3 a. m.	61	3 a. m.	114.
Saturday, 20	29.651	29.718	0 a. m.	29.600	6 p. m.	73.7	67.0	81	4 p. m.	70	4 p. m.	66	4 a. m.	64	4 a. m.	144.

Mean for the week .... 29.858 inches.
Maximum for the week at 9 a. m., August 17th .... 30.064 "
Minimum " at 0 a. m., August 14th .... 29.596 "
Range .... .468 "

	Dry.		Wet.	
Mean for the week	67.4	degrees	62.4	degrees.
Maximum for the week, at 4 pm. 20th	81.	"	at 0 am 14th, 71.	"
Minimum " " 5 am. 17th	56.	"	at 5 am 17th, 55.	"
Range " "	25.	"	16.	"

AUGUST.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 14	n. w.	n. n. w.	n. n. w.	213	4½	6.20 am	.543	.492	.462	79	53	65	2 cir. cu.	4 cir. cu.	9 cu.	-----	-----	-----	--	8
Monday, 15	n. n. e.	n. n. e.	n. n. e.	234	7	2.00 am	.489	.532	.509	74	63	74	1 cir. s.	7 cir. cu.	4 cu.	-----	-----	-----	--	4
Tuesday, 16	n. n. e.	e. n. e.	e. n. e.	151	2¼	10.20 am	.470	.462	.433	73	65	73	7 cir. cu.	10	1 cu. s.	-----	-----	-----	--	7
Wednesday, 17	n. e.	e.	s. s. e.	145	5¾	9.00 am	.422	.460	.473	87	83	88	4 cir. cu.	10	10	11 p m	12 p m	1.00	.01	7
Thursday, 18	n. n. e.	e. n. e.	n. n. e.	116	2	4.00 am	.456	.470	.483	88	73	78	10	9 cu.	10	-----	-----	-----	--	0
Friday, 19	n.	n. n. w.	n. n. w.	83	¾	2.00 pm	.516	.564	.556	83	80	84	9 cu.	9 cu.	10	1 a m	2 a m.	1.00	.01	0
																11½ a m	12 m.	0.30	.01	1
Saturday, 20	n. e.	n. e.	n. n. w.	73	2½	11.10 am	.556	.537	.628	84	54	72	8 cu.	3 cu.	7 cu.	-----	-----	-----	--	

Distance traveled during the week .... 1,015 miles.
Maximum force .... 7 lbs.

Total amount of water for the week .... .03 inch.
Duration of rain .... 2 hours, 30 minutes.

DANIEL DRAPER, Ph. D.
Director Meteorological Observatory of the Department of Public Parks, New York.

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The attempt to utilize compressed air as a motive power for street cars in cities, appears to have been most unsuccessful. About four years since, a company was organized in New York city for the purpose of building street cars on the pneumatic system, capable of replacing those drawn by horse power, and about the early part of April, 1878, a passenger car propelled by compressed air was running on the Second avenue, New York, between 63d and 93d streets.

The experiment was considered perfectly satisfactory for a first attempt, as the cars performed their work admirably; and the public press and various eminent engineers considered the problem solved. There was, however, an essential element of success that was wanted, which appeared insignificant at the time, but which proved fatal to the whole scheme. This was a failure on the part of the engineers to design machinery which should be constant in its working, requiring little attention from the driver.

It was supposed that in building future composite pneumatic engine cars these defects could be remedied. But when the six cars built on this principle were placed on trial, the same trouble was experienced, and the experiment was abandoned, causing a considerable pecuniary loss to the promoters of the company.

The Pneumatic Tramway Engine Company, undaunted by past losses and failures, have renewed their efforts, and have recently constructed a pneumatic traction engine, which we understand will be immediately placed on trial on one of the New York elevated railroads. The successful working of Electric Railway Engines has probably increased the difficulties of those who are advocating the use of compressed air as a motive power. In the absence of smoke, odor, noise and cinders, both the electric and compressed air systems have many advantages over steam for elevated railroads, and the question of economy wiil probably decide which system shall be finally accepted. At the present moment all the advantages appear to be in favor of the electric railways for use within city limits, and it is probably a mere matter of time, for all the New York elevated railroads to be running their trains by this system.

THE STATE AND HIGHER EDUCATION.*

By Professor N. H. Winchell.

The incentive to the following address appears to have been certain remarks made officially by President John of Hamline University, who considered that "higher education should not be under the control of the State," and that the design of the State Colleges has been a conspicuous and universally acknowledged failure.

In the first part of the paper Professor Winchell presents an historical sketch of the circumstances, the result of which was "that the State finds itself in the conduct of systematic education."

After tracing the progress of education in Europe he states:

Thus we find that none of the old universities, except when under the control of the government, and sometimes not even then, have been willing to modify their curricula in compliance with the demands and spirit of the age. If they have done it, as more lately at Oxford University, it is only after the force of public sentiment has been able to batter down the walls of prejudice and conceit with which they have been surrounded. During this whole conflict throughout Europe the Church, in its various forms, but particularly the Roman church, instead of being the champion and refuge of free thought and free knowledge, has been the most powerful obstacle to its progress, and has persistently opposed every movement to introduce the means for disseminating useful knowledge among the people. The heat of the conflict is passed. The tide has set in the right direction. The old universities perceive the triumph of modern science. European governments are unanimously striving for the establishment of modern schools of science on the broadest foundations, and equipping them with the fullest appliances.

Now let us turn to America, and inquire how this history has been mirrored on our institutions of higher learning.

In the first place the church colleges that arose in this country prior to 1824, or even later, were modeled after the mediæval universities of Oxford and Cambridge, so far as they expanded into the dimensions of a university. For the most part they were simply colleges of classical lore, with but one course of study, aiming specifically. at first, to educate young men for the clerical profession. As they were born of the English universities, so they inherited their mediæval narrowness and bigotry. As the early church had grappled with Copernicus and Galileo, and had been worsted, so the later church would grapple with everything that bore a resemblance to or intimation of any new fangled notions of nature. Although the world had made wonderful strides in human knowledge, the colleges shut their eyes and ears to the change. The age demanded education in the great industries that characterize modern society, but could get only that of the age of Elizabeth. As modern science and civilization began to buzz about their doors, they drew themselves within their shells, affrighted, like snails. Having none of the elements of the

* Delivered before the Minnesota Academy of Natural Sciences, Jan. 12, 1881.

new light within them, they were literally enslaved to themselves and could not escape. They began to sink in public esteem. Their graduates failed conspicuously in competition in all the affairs of life with self-made men. Finally, in view of this disparity between the demand and supply of industrial and scientific instruction in America, a far-seeing and generous business man, Stephen Van Rensselaer by name, came forward with private means, and became the first to endow, in America, a "school of theoretical and applied science." This was done in 1824, and it is located at Troy, New York. Twenty years later the first voluntary effort was made within one of the old church colleges of America to regulate the curriculum so as to conform to the new demands, aud although pushed by one of the ablest educators of America, Francis Wayland, in his own institution, and with his own denomination, at Brown university, the movement ended in a conspicuous defeat of the "new education." After the successful establishment of the Troy Polytechnic Institute, the example of Van Rensselaer was followed in Connecticut by Joseph E. Sheffield in the founding of the Sheffield Scientific school, which became attached to, but by no means recognized as co-ordinate with, the old line course in Yale college. This was in 1860. In 1847, soon after the failure of Dr. Wayland at Brown University, Abbot Lawrence endowed the Lawrence Scientific school at Harvard college.

About this time the legislature of the new States of the West began to express the sentiments of the people. In Illinois conventions met in 1851 to consider such means as might be deemed expedient to further the interests of an agricultural community, and to take steps toward the establishment of an agricultural college. They met not as Presbyterians, or Methodists, or Romanists, but as an agricultural community. The next year petitions were sent to Congress for the endowment of industrial universities in each State. In 1850 the agricultural college of Michigan was provided for by the State constitution, and went into operation in 1855. The scientific course of the University of Michigan was ordered by the State legislature in 1851. In 1858 Iowa appropriated money for a model farm and an agricultural college. In Kentucky, under the guide of Regent Bowman, an institution, chartered in 1858, had been established for "diffusing education among the industrial classes." In Pennsylvania an agricultural college was established in 1854, and in Maryland in 1856. In New York, as early as 1837, a project for establishing an agricultural college at Albany was entered upon and a site was selected. This resulted in failure. It was revived in 1844, and again failed through the death of a liberal friend of the enterprise; but in 1856 the State Agricultural Society of New York induced the legislature to appropriate $40,000 for a college of agriculture. This institution was established at Ovid, and died when the war of the rebellion broke out in 1861. The People's College, at Havana, N. Y., intended entirely for the industrial classes, was at first offered the national agricultural land grant of New York State, but failing to comply with the conditions imposed by the legislature, this fund was passed to Cornell University at Ithaca. These institutions, all established prior to the year 1862, when Congress passed and the President approved the great educational land grant law had come into existence in compliance with the demands of modern civilization, and not at the instance of the church colleges, but often in the face of obstacles and discouragements thrown in their way by the church schools. But President John says that the "facilities of higher education existed in this country, and met all demands, before State colleges were thought of." With the single exception of Yale College and Hamline University at Red Wing, which established a so-called "Scientific Department," the former in 1846 and the latter in 1857, not one of the church colleges, so far as I have been able to learn, showed the first symptom of knowing, much less recognizing, the difference in educational need between the age of Bacon and that of Lincoln.

The soil, therefore, was all ready for the seed. The bill introduced by Mr. Morrill of Vermont was vetoed by conservative Buchanan. Passed again at the instance of Mr. Wade, with only seventeen opposing votes, it was signed by President Lincoln on the 2d of July, 1862. It has been said that times of war witness the birth of great ideas and the initiation of great enterprises. It is true that in the United States, with the establishment, through rivers of blood, of the national idea, was also established the idea of higher education by the State as one of the justifiable means, in a republic, of self-defence and self-perpetuation.

This is all passed now, nearly two decades ago. If we proceed to inquire what has been its effect, we shall be able to answer another of President John's surprising statements. Is the design of the law establishing these industrial colleges by Congress, "a conspicuous and universally acknowledged failure?"

One of the first effects of this land grant by Congress was an awakening in the church colleges, then existing, to the value of the public domain as an educational agency. This was so rapid, and so great, that some of them succeeded in capturing the whole fund almost before the people knew it had been given to them. In others, along with a compliance with the terms of the act, the State demanded representation on the controlling board; but in most cases the church colleges were passed by, and new institutions were founded by the various States, though still, in many cases, combined with some other State or private fund.

In the second place, this law, which has so positively been pronounced a failure, brought into existence up to 1876, about forty schools of agriculture and mechanic arts, often styled national schools of science. These have come into existence since 1862—except in three States where similar institutions had already been endowed by State funds. In some cases also the fund was applied by the State legislatures to rejuvenate weakly scientific institutions, or further endow those that were flourishing. In the meantime, since 1862, the various churches of the United States had founded, up to 1876, 106 denominational schools. Some of these are based on broad foundations, and, like Hamline University, offer the student the most complete scientific as well as classical and literary culture. While the national schools of science are mainly confined to their own sphere—the primary intent of the law creating them—the new church schools cover all the fields of knowledge. It cannot certainly be unjust to them to compare their patronage by the youth of the country, with that received by the State schools. This, perhaps, will throw some light on the question of their asserted failure.

The 106 denominational colleges, established between 1862 and 1876, both inclusive, as reported by the Commissioner of Education, are found to be giving instruction to 13,757 students, including all departments, preparatory and undergraduate, in all branches of knowledge, from theology to chemistry and engineering, giving them an average of 130 students for each institution. Of these students, 9,066 are reported as in the preparatory (or secondary) grade of study, an average of 85 for each institution; and 4,691 are reported in undergraduate studies—an average of 44 for each institution.

Taking the same authority for the statistics of the forty State schools of agriculture and mechanic arts, and including only those students that are strictly in those departments, wherever a pre-existing college received the congressional grant, we find 4,891 students, which gives an average of 122 for each institution. Of these, 631 are reported in preparatory (or secondary) courses, and 4,260 in the undergraduate courses of study. This gives the state schools an average of 16 in the prepara-

tory classes and 106 in the higher classes. Thus it can be seen that, as institutions of higher learning, the attendance on the new church colleges is but 41 per cent of that on the State colleges. Hence, if the law of congress which called into existence these State colleges be a failure, how much greater the failure of that sectarian spirit which called these 106 denominational colleges into existence.

Another remarkable effect of this movement toward popularizing higher education in America was the renovation and elevation of the church colleges, then existing, and the establishment of numerous others with much broader and a more liberal scope of instruction. This of itself has resulted in immense benefit to education, as well as to the church in America. This effect is as important as the creation of the State schools themselves. The church has always been the principal agent of higher education, at least in the United States, and the recognition, by these institutions, of the great underlying truths of nature, and of the ministration of her laws to the daily comfort of man, is an epoch in the history of the nineteenth century, which, in its effects on the race, will exceed all other achievements of the "new education." It will contribute not only to the spread of science, but also to the spread of Christianity, particularly among those intelligent classes of the people who have been hostile to it, or indifferent, because of the attitude of the Christian church toward the truths of modern science. If the church once recognizes the fact that every enlightened nation is in arms against its supine adherence to mediæval education, and condescends to place itself in harmony with the truths of creation as well as revelation, one of the greatest obstacles to the evangelization of the world will be removed. It is easy to see that the material aspects of modern civilization are rapidly penetrating unchristian and uncivilized nations, outstripping the church in evangelizing them. How much better it would be if the two agencies could go harmoniously together into the same field, co-operating to accomplish the same end.

What has been said, so far, relates to the past. A few matters of fact have been stated. They pertain to the title, by which the State received, and holds, the educational structure which she has occupied. But President John not only disputes the title, but also the right of the State to occupy this field. We admit that force does not always coincide with right, and that, although nine points in the law are established when peaceful possession is proven, the tenth point may have the right on its side. Let us enquire, then, if there be a consistent reasonableness in the State's attempting and continuing to do this work. We shall not attempt here the justification of the State in establishing and maintaining primary and secondary schools. It is not demanded. In passing, however, we will except President John's definition of the duty of the State to educate. He fixes it at the "limit of necessity to preserve its own existence." So let it be. We shall recur to it again. But, specifically, as relates to higher education, the leading objections that have been urged are the following: (1) The personality of the State. President Elliot has fully presented this objection. It is foreign to the free spirit of American republicanism to witness the controlling influence and authority of the State in social and educational affairs. It smacks of the divine right of kings, and is a reminder of the despotism of Europe, two centuries ago. Now all this may be an objection in monarchical governments, but it seems rather strange that any promising educator in republican America should forget that here the people are the State. There is no kingly personality interfering with the domestic and social institutions of the community. The authority that controls is the aggregate will of the community. The chief right of the State's power is to conserve this aggregate will. Such an expression of the will of the people is voluntaryism in the discharge of its highest organic function, and is not "paternal government." (2) Again it is objected to State education, that it tends to uniformity, and not to variety, reducing all pupils to the same pattern, and smothering the aspirations of genius which spurns conventionalities and revels in the gratification of its own idiosyncrasies. This objection is more valid in the lower schools than in the higher. In the higher schools it is very questionable if the institutions of the church would be as lenient with idiosyncrasies in pupils, as those of the State. Judging from the past it would be folly for a student with an idiosyncrasy of genius to flee to a church college for its indulgence. We cannot see how this objection applies more fully in State colleges than to church colleges. In fact it is one of the necessary sacrifices which an individual has to make, when he becomes one of an organized community. He receives the benefits of combined effort in all directions, and he has to surrender the personal freedom to act in certain directions in which his action would transgress the aggregate good of the community. The schools are for the average pupil—both State schools and church schools—and he with an idiosyncrasy will look in vain for a place to disport himself. (3). It is urged again that it is not economical. Because, forsooth, a sectarian zeal demands denominational colleges, and "cannot conscientiously accept this service of the State," and will maintain colleges of its own, therefore the State cannot rightfully duplicate these institutions and tax the denominations for their maintenance. Not to mention the brevity of the time elapsed since the sects were willing to "do the same kind of work" as the State University, it is enough to reply that this argument applies against all State organization for education. The Roman Catholic insists on maintaining his own hospitals, and objects to taxation for public schools. The Atheist opposes the public tax because in these schools is taught the idea of a God, the Jew because the New Testament is read, or the Protestant because it is not. This argument against the public schools may be applied with equal reason against the State's management of the deaf and mute. At least, certain medical fraternities might use it because they cannot "conscientiously" endorse nor accept the methods of treatment practised by the State. (4). But the fourth objection, after all, is the chief one urged by the opponents of State schools—they do not correctly indoctrinate the student in matters of religious dogma. It is said that "the State by self-imitation cannot teach religion." This assertion the State accepts, and would fain leave it to the proper agent, yet the State is not therefore "prohibited by statutory limitation from throwing the least safeguard around the minds of our youth," which is one of the surprising inferences of President John. The State in its educational operations will always be governed by the aggregate sentiment of the people. Those fundamental ideas of religion, which are accepted by all sects, the State institutions will be compelled to teach. If, peradventure, for a time they happen to lapse from this high duty, the will of the community will sooner or later be restored. They are creatures of the people. They will teach what the people can agree on shall be taught. While they must not teach sectarian dogma, they must not become centres of atheism nor of infidelity. If they did either, they would not long survive. Like the schools of Switzerland, they are based on the "principles of Christianity and democracy." The special, political and denominational application of these broad platforms is left to party politics, and to various sects.

We venture the assertion, however, that when the true kernel of this objection is found, it will not consist in a fear of the non-inculcation of these truths by the State, but in a jealousy of the sects, one against the other. Education by the church has been considered essentially the training of the youth and doctrines of the

catechism. Though greatly extended in scope, it is still animated by the same cardinal principle. Each sect must defend itself by teaching its own dogmas to the youth, and, though every State college were to be abolished, there would be still as great a reason for maintaining all the denominational colleges. How long it would be before they would degenerate to the condition of mere sectarian propaganda, as before the revival, no one can say, but there would be a strong tendency in that direction. Freed from the competition of State colleges, their zeal in the teaching of science would soon lag. Not having ready access to the public means and resources of instruction, such as the State archives, maps, authorities, explorations, surveys, statistics, and to the avenues by which the State knows and readily regulates the great industries of the people, the church colleges would very soon see that there is an actual incongruity in their assuming to direct the scientific and industrial education of the people. It is the chief business of the church to look after the spiritual well-being of the people and not to fit them to carry forward the complicated machinery of modern civilization. Religion is the lubricator of this vast system, and the church is the agent by which it is applied. When the church departs from this sphere, she forsakes the true idea of the primitive church. When she leaves her spiritual kingdom and assumes to direct in the construction of steam engines, in the handling of theodolites and compasses, in the management of cotton-gins, in the measurement of the angles of crystals, and the distances to the stars, she may very reasonably be held to be out of her sphere. She has the privilege, of course, of doing all these things, and there was a time when she had good reason to do them, and was urged to do them, as the only capable agent; but that time has passed, and it can hardly be considered to be her duty to do them in the nineteenth century, when other agents equally capable have arisen, endowed with that special duty and function.

One of the boasted advanced steps of the nineteenth century is the separation of the church and State. In the mere manipulation of the governmental machine this is fully realized in the United States, and in much of continental Europe. But the administration of the laws is not the State, nor, indeed, is the making up of the laws, nor both of these united. True statesmanship surveys the whole body politic. It foresees and often institutes national enterprises. It watches the external and also the internal influences that move the masses; it takes advantage of the shifting markets for the domestic products. It notes the rise and decline of the various industries. It applies stimulants when needed and repression when necessary. In short, the State is an all-prevading, energizing, regulating, far-seeing organization of the people; the culminating expression of the modern democracy. It is this machinery, which in our day is very closely connected with the appliances of modern science, which is not free from the church, but which the church assumes still to direct. Instead, we claim that it is the right and duty of the State itself to look after its own interests, and especially its highest interests, and to take measures to qualify citizens not only to read their ballots, but to discharge all the duties of high citizenship. There is no limit to this duty short of the necessity of the State, as has already been admitted. That which constitutes a State—"high-minded men"—is its necessity, and that it is the duty of the State to provide, to the end that its multifarious industry may be under the guide of the highest statesmanship.

THE French Government has appointed a committee, presided over by Rear Admiral Bourgeois, to study the different applications of electricity to navigation.

THE Society of Telegraph Engineers and Electricians will hold a meeting in Paris on September 21.

MAGIC MIRRORS*.

BY M. BERTIN.

[Translated from the French by Marchioness Clara Lanza.]

LADIES AND GENTLEMEN:—The term Magic was formerly applied to those metallic mirrors employed by sorcerers, necromancers, astrologers and charlatans, and by means of which spirits were invoked and the future predicted. These mirrors, transmitted from antiquity to the middle ages, were used to a very great extent about the sixteenth century, and up to two hundred years ago they were constantly seen in Europe. Now they are found nowhere except in the far East. We are able to furnish any amount of information about this strange superstition, but it is not of these mirrors that I intend to speak.

There is another kind of magic mirror, so-called because it produces effects apparently marvelous but real. History will tell you nothing, however, about these mirrors, and they are not even mentioned in any book of physics. Their appearance in Europe is quite recent, and as they are exceedingly rare, there is not often an opportunity presented for observing them. It is of this scientific curiosity that I shall talk to you this evening.

These mirrors are an uncommon variety of metallic ones. The latter you know were the first invented by man. The Greeks and Romans had no other kind, except a few specimens of glass mirrors made at the factory in Sidon. But glass when not quicksilvered does not make a good mirror, and it was not until the thirteenth century that quicksilver was employed for the purpose. Up to that date metallic mirrors alone were used, and even now some uncivilized nations employ and mannfacture no other kind.

The Chinese and Japanese, for instance, are an example†. Since they have been in constant communication with European nations, however, they have partially adopted our glass mirrors and send us their metal ones as objects of curiosity. Chinese mirrors are exceedingly rare, so rare, in fact, that there is not one to be had in Paris. This leads me to think that they are no longer manufactured. Japanese mirrors, on the contrary, are very common. This is perhaps owing to the fact that in Japan the mirror is not only a necessary article for the toilet, but also an object of national worship. The primitive religion of the country, which is still embraced by the aristocracy and called *Synthism*, worships the goddess of the sun as its principal divinity, and the Emperors of the nation are supposed to be her descendants. This goddess invented the metallic mirror, and presenting it to her son bade him preserve it religiously. In the palace of Mikado, therefore, the mirror chamber is as carefully attended to as that of the Emperor himself, and often receives greater attention. In the temples of *Synthism* the only object of worship is a mirror, kept in a box covered with several wrappings of silk. Although this religion has been abandoned by the greater portion of the people, who have since become Buddhists, the mirror, nevertheless, has always remained a precious article. The ladies keep it raised upon a tall easel, which brings the glass upon a level with their eyes when they stand upon tip-toe. When they wish to remove it they hold it carefully by the handle, sometimes thrusting the latter into a piece of split bamboo.

These mirrors are of bronze of various sizes and shapes, but always portable. One side is polished and amalgamated. It is also generally convex, so that the images reflected look somewhat distorted. The other side is flat or slightly concave and is always ornamented by figures

* Alecture delivered before the *Association Scientifique de France*.

† This statement is not altogether correct. The Chinese manufacture glass mirrors, and very seldom, if ever, use metallic ones any more.

in relief of more or less perfect workmanship. Japanese mirrors usually are more beautiful than the Chinese.

Here, gentlemen, are a few. I cannot pass them among you, but I can show them to you perfectly well by means of the magic lantern. This one is the copy of a mirror brought from Japan by Professor Dybowski. This second one belongs to General Teissier. The designs are of the reverse or unpolished side.

Among these mirrors there are a few of a lesser thickness, which possess a remarkable property. Although they reflect ordinary images in a diffused light, if a ray of sunlight falls across the polished surface and is reflected upon a white screen, the ornamentation upon the reverse side will be transported to the screen. This is what we call a magic mirror.

The first that appeared in Europe came from China. The Chinese, in fact, have known of them for a very long time. One of their historians who flourished in the eleventh century of our era, mentions them with admiration. Another writer, who lived in the year 1300, gave us a supposed explanation of the phenomenon. The Chinese Encyclopedia contains an article upon the subject, which has been translated by our great sinologue, Stanislas Julien. These mirrors have always been rare, but persons who have lived in China assure me that they can sometimes be found in Chinese curiosity shops.

We are not sure that the mirrors were ever purposely made. It is probable, on the contrary, that they are merely the result of imperfect fabrication. In regard to the Japanese this is absolutely certain, for magic mirrors are unknown in Japan. Neither the manufacturer nor the person who sells them has any idea of their peculiar property. European *savants*, however, have found magic mirrors from Japan many times. In 1832 M. Prinsep described one in the *Journal de la Société Asiatique*, which he had discovered in Calcutta. In 1877 Mr. Atkinson, Professor in the Imperial University of Jeddo, observed that numbers of Japanese mirrors produced magical effects. This attracted the attention of M. Ayrton, Professor in the Engineering School in the same city, who immediately began to investigate the matter. After examining five or six hundred he affirmed that three or four out of every hundred were magic.

Partially magic mirrors ought to be very common, and I am quite sure if I had been permitted to examine the Japanese collections in Paris, I should have found several. I am indebted to General Teissier for two beautiful mirrors that he brought from Japan. One of them is decidedly magic. I will have an electric light thrown across it and then upon the screen. You will see a part of the design upon the back appear.

Although we can furnish no written testimony concerning these mirrors, several learned men however, especially those who had traveled extensively, knew all about them. In the year 1830 Humboldt came all the way from Berlin to Paris, in order to show the Academy of Sciences a mirror which he believed to be magic. The experiments were made at the Observatory. Unfortunately there are no traces of them to be found in the scientific reviews of the period, but we know that the whole affair was a *fiasco*. Our illustrious chemist, M. Dumas, who was one of the investigators, affirms that Humboldt's mirror could not be considered magic in any sense of the word.

The first magic mirror that appeared in Europe was owned by M. Monchez, the Director of the Observatory in Paris. On his return from China he brought with him several mirrors, one of which was magic and had been sold as such. This mirror was presented to the Academy of Sciences on the 22d of July, 1844.

In 1847 a second one appeared belonging to the collection of the Marquis de la Grange. Stanislas Julien gave a detailed description of this one, in which he stated that the reflection obtained was identical with the reverse of the mirror, but that the latter was not in relief. This mirror, therefore, should not have been magic at all, or if it was, all our modern theory would be upset. Many attempts have been made to find this mirror, but up to the present time the search has proved fruitless.

A third magic mirror was presented to the Academy in 1847 by Person, Professor of Physics in Besançon. Person's report consists of twenty-five lines only, but it is extremely important, as it contains the whole theory of magic mirrors, which, until then was unknown.

Finally, in 1853, Maillard presented the Academy with a fourth mirror, which was not magic to any great extent. It is now in the Collège de France. I have held it in my hands, examined it carefully, and I can assure you that it is an exceedingly bad specimen. A great deal of imagination must be possessed by any person who can call the effects of this mirror magic.

This, gentlemen, was the last, and the excitement about magic mirrors began gradually to subside. Nothing more was heard of them till the year 1878, when MM. Ayrton and Perry, both professors in the Engineering School, at Jeddo, presented the Royal Society of London with several magic mirrors which they had brought from Japan. For the first time, technical observations were made concerning the construction of these mirrors. As to the mirrors themselves, the effects produced by them were truly marvellous. We were unable, however, to form any correct judgment upon them until last year, when M. Ayrton brought four to Paris. The experiments made by him proved very successful, and were witnessed by me with great interest.

Since then, the fame of magic mirrors has revived with double intensity.

A few days after M. Ayrton's séance I received a visit from my old pupil M. Dybowski, Fellow of the Academy of Physical Science, who returned from Japan after a professorship of more than two years at the University of Jeddo. Of course, you all know that after the revolution of 1869, Mikado's government founded large scientific schools in the capital. Unfortunately, they no longer "import" professors from Europe, but content themselves with such pupils as we turn out.

Like all Japanese, M. Dybowski was ignorant of the existence of magic mirrors. He brought with him, however, as curiosities, four mirrors of antique manufacture, which are called *Temple mirrors* in Japan, and considered to be superior to modern ones, as the fabrication has grown exceedingly defective of late, owing, probably, to the competition of European mirrors. We experimented together with these four specimens, one of which was found to be magic in a slight degree. This mirror has been the starting-point of all our subsequent progress. Of course, this was naturally the consequence of a sound theory, which, however, was not immediately established.

The oldest on record is that given by a Chinese author of the thirteenth century. According to him, "the cause of the phenomenon is due to the use of fine and coarse copper. If, in manufacturing the mirror, the image of a dragon is produced in relief upon the reverse, a similar dragon is engraved upon the polished side. This last is concealed by filling up the lines of the engraving with copper. The metal is then incorporated with a purer quality of copper, while the mirror is submitted to the action of fire. Finally, the surface is polished and washed over with amalgam." The author, however, does not seem to see that if the difference in the reflective power of the two qualities of copper was sensible enough to make the phenomenon appear, this variation must necessarily disappear under the application of the amalgam.

Brewster's theory does not differ notably from the Chinese explanation. He says the polishing effaces the engraving and renders it invisible in a diffused light, leaving upon the metal, however, variations of density and reflective power, which makes the image quite visible

when exposed to the sun. But Brewster was not aware that the surface of the mirrors was amalgamated, and we may safely say that he was wrong to attempt a solution of the mystery, without ever having seen a magic mirror.

Nevertheless, before rejecting his idea completely, let us seek to verify it. I myself have had an engraving made upon copper, then caused it to be effaced. When it was no longer visible in a diffused light, it was, unfortunately no longer visible when exposed directly to the sun. Perhaps I went badly to work, and very likely a more careful and delicate operator might have succeeded better. We might have explained in this way the effects of an extraordinary mirror mentioned by M. Ayrton, which, instead of reproducing the image engraved upon the reverse, disclosed to the astonished spectators the grotesque features of a Buddhist saint.

Brewster's theory, fortunately, was not known in France when public attention was directed upon magic mirrors. I say fortunately, because his opinion, being that of a celebrated man, might have led us astray. The first French physician who examined a magic mirror, Person, immediately discovered the true solution of the problem. In the first place, he found that the polished surface of the mirror was not perfectly convex, but only so in certain parts, corresponding with the lines of the figure upon the reverse. In the portions corresponding to the relief, it was almost flat. Rays of light reflected upon the convex parts diverge, and produce but a faint image. On the contrary, rays reflected upon the flat portions retain their parallelism, and produce an image which is very intense. This is why the ornaments in relief appear brilliant upon a dark background.

This irregularity of the surface depends of course upon the method of fabrication. When taken out of the cast, the mirror presents the appearance of a flat disk. Before being polished it is scratched in every sense of the word with a pointed instrument, to which it naturally offers more resistance in the thick parts than in the thin. This operation makes it at first slightly concave, and by the elastic reaction of the metal it becomes convex. This convexity is more sensible in the thin places than in those corresponding to the relief of the design. The mirror is finally polished with a whet-stone, then with charcoal, which must frequently destroy the irregularities which produce the magical effect. The surface thus becomes perfectly smooth, but generally one or two cavities can be found. The manufacturer fills these in with balls of copper which he has ready prepared and of all dimensions, and which he afterwards rubs and polishes until he thinks they are invisible to the naked eye. An expectation, however, which is but imperfectly realized, generally speaking. The entire surface is then rubbed by hand with an amalgam composed of equal portions of mercury and tin.

Such are the details of the manufacture of magic mirrors. It is easy to see that they quite agree with Person's explanation, but the latter has one objection. How, it will be asked, can the surface of the mirror be irregular without this being apparent in the images it reflects in a diffused light?

However, this objection is removed when we come to consider the facts attentively.

A mirror with a perfectly regular surface is an exceedingly rare object. Here, for instance, is a flat, metallic mirror employed in astronomical observations. The reflections it gives are very good.

Here also is a silver plaque which reflects in a manner equally perfect. If however, it is made to reflect an electric light, we can see clearly that the surface is not uniform, for we are able to perceive, so to speak, every blow of the hammer which it received during the process of manufacture. Here is one of those little round mirrors which we buy for a few pennies at the bazaars. It is excellent and extremely serviceable if you desire to comb your beard, but detestable if you wish it to reflect light.

By means of these examples you can easily see that all our common mirrors are irregular and reflect light imperfectly, although forms can be reflected by them very well. These are true magic mirrors, only the image reflected is as irregular as the mirror itself, while that of the Japanese mirror is regular like the curves of the surface which produces it.

But are we quite certain that the flat and the convex parts of an irregular mirror reflect a sufficiently variable amount of light to make them quite distinct, one from the other. Let us see:

Here is a convex mirror, the summit of which has been planed off, making a flat mirror in the middle of a convex one. I will now reflect it upon the the screen by means of an electric light. You see the central portion is a very brilliant disk which shows that the flat mirror reflects the cylindric portion. Around it is a black circle in which there is no light at all. This is the space between the cylinder and a sort of funnel which contains the light reflected by the convex mirror. This light forms around the black circle a grayish ring of feeble intensity and quite distinguishable from the white central disk. The difference would be much more marked if the two lights were closer together.

Here I have another mirror which is flat. To the middle of it has been attached a convex lens. The whole has then been silvered. A reflecting light you see, shows us upon the screen, a large spot nearly black surrounded by a brilliant ring which has another ring around it of a gray color. The centre and the rings about it are produced by the reflection upon the central convex lens, across which comes the cylinder formed by the reflection upon the flat mirror.

The variation in intensity of the two reflections is enormous, particularly in the centre, which only appears black by contrast. For, of course, there is just as much light upon the central disk as upon the edges. We know that it is really luminous for we can cast upon it the shadow of an opaque body.

It has been, I hope, clearly demonstrated to you, therefore, that the curves upon the surface of the mirror produce inequalities of marked intensity when reflected. You have, however, a perfect right to remain in doubt as to Person's theory, because in all magic mirrors, these irregularities are very faint, being almost invisible to the naked eye. Although Person has endeavored to sustain his theory by direct observations upon the surface of his mirror, it was necessary to support it still further by means of new experiments.

An Italian *savant*, M. Govi, has undertaken this task, and in 1864 and 1865 presented two papers upon the subject to the Academy of Turin.

The first one contains several experiments made for the purpose of upholding Person and utterly demolishing Brewster. But Brewster was determined not to give in, and after having translated M. Govi's article for the *Scientific Review*, he followed it up with a quantity of remarks and objections which he certainly never would have made had he ever been fortunate enough to hold a magic mirror in his hands.

The stupidity of the illustrious Scotch physician had a very good result, for it incited M. Govi to seek new proofs and obtain a surer ground than ever for his opinion. In this way he conceived the idea of making the most important and most curious experiment which had yet been seen in regard to metallic mirrors. He thought that by heating the mirrors on the back, the warmth would take effect sooner upon the thin parts than upon the thick; that the former would become more convex, and thus the magic property would increase in such mirrors as already possessed it in some degree, and might possibly be produced in those which were not magic.

Here is General Teissier's mirror—you have already seen that it was slightly magic—I shall now have it heated

by means of a gas light placed behind it, and you will immediately see that the magical effects become more intense. It develops by degrees and produces nearly all the forms and images which are on the back, quite perfectly. You observe that the great quantity of small figures which are in but slight relief are not visible, while all the others of pronounced relief are clearly brought out. This fact is an important one. It shows us that we must look for magic mirrors only among those having ornaments in decided relief upon their backs. You must also know that they are not to be found among very thick mirrors. The experiment is still more successful with this Japanese toilet mirror.

The first experiments, after reading M. Govi's papers, were made by M. Ayrton and myself, as we desired to verify the investigation of the Italian *savant* before publishing them, and at the same time study thoroughly this very interesting subject, hoping that we might be able, perhaps, to reproduce the mirror in France instead of importing them from the extreme East. You must bear in mind that we had but one mirror at our disposition and that one but slightly magic. It belonged to M. Dybowski. We began by heating it as I shall show you presently. Here is the natural mirror which is hardly magical at all. You see the effect is produced in proportion to the amount of heat employed. Heat applied to several other Japanese mirrors bought in Paris or borrowed from collectors produced a magical effect upon them all.

These experiments were repeated by us very often. But it was not long before we discovered the inconveniences of the heating method. First of all, as it is extremely difficult to preserve an equal degree of warmth upon the entire surface. The image is rarely perfectly regular; then the mirror itself is somewhat altered. The reverse becomes covered with a bronze iridescence while the surface loses its polish because the heat destroys the amalgam which covers it, the mirror loaned to me was in a frightful condition when I returned it, but it was finally put in order again. The spots upon the back were removed by a coating of slightly acidulated water, and the amalgamation replaced by nickel plating which made a more perfect and durable polish. Before giving it back to its owner, however, we had numerous copies made from it, and it was one of these which I showed you a few moments since.

The disadvantages of heating made us wonder if the same effect could not be produced by a different method, and we thought of pressure. M. Duboscq solved the problem by means of this box. You see it is not thick, and is of the precise diameter of the mirror which is attached to the upper part by an iron ring and one of India rubber. The under portion is closed and provided with a spout and plug which it connects with the little hand pump well known as the Gay-Lussac. This pump inhales on one side, and exhales on the other. If we attach an India rubber tube to the spout, on the exhaling side the movement of the piston will compress the air behind the mirror. We will now try it.

The mirror becomes more convex and the image widens. The thin portions protrude more than the others and the magical effect grows more and more pronounced. It will be quite complete when the pressure attains two atmospheres. We have it now! You see, the effect is perfect. It is certainly much finer than anything M. Ayrton has shown us, although his experiments astonished us so much.

We can also produce an inverse effect, by attaching the rubber tube to the inhaling spout. The action of the pump will remove the air beneath the mirror, which will become less convex and you see the luminous disk contract. The thin portions corresponding to the outlines of the design will yield more than the others, become less convex and perhaps concave. They will reflect more light and we may see a new image appear which will be the exact reverse of the preceding. That is to say, that the parts in relief will appear black upon a white ground. This is a *negative* form of the first, in which we saw the relief traced in white upon a black surface.

M. Deboscq made many other experiments, one of which I will relate to you before I conclude.

I wished to go still further. I wished to have a cast taken of the mirror while it is magic, and make a galvano-plastic deposit in the mould so that we might have a magic surface instead of a mirror. We tried this three times. The plaster moulding was very successful and the surface magic, but the galvano-plastic deposit was a complete failure. If any one here among my audience can give me any advice upon the subject I would be most grateful.

Gentlemen, I hope sincerely I have been able to interest you in this new subject of magic mirrors. If I have succeeded in making my meaning throughout, clear to you, these mirrors will no longer be a mystery, and you will have seen once more how Science, by slow but sure efforts, is finally able to explain and reproduce phenomena, which at first sight seemed miracles, always provided that the phenomena are real and not mere phantoms of human credulity.

RECENT SURGICAL CASE.

The following case, which, in some respects, is similar to that of President Garfield's, may be read with interest for the purpose of comparison. The man was sent to St. Michael's Hospital, Newark, N. J., where his case was considered hopeless at the date of his entry. We are indebted to Dr. H. C. H. Herold for a copy of the following report:

George Freund, age 36, Germany, ex-policeman.—Admitted to hospital July 4th, suffering from bullet wound of chest. The wound was produced by a 22-inch calibre pistol, and situated an inch and three-quarters below and half an inch to the left of the left nipple. When seen half an hour after admission his pulse, temperature and respiration were all normal. On examining his lungs the percussion note was normal. On auscultation, rales were heard over both lungs, resulting from chronic bronchitis. He is subject to attacks of asthma. Heart sounds normal. Ordered one-quarter of a grain of morphine every two hours until sleep was obtained.

July 5.—Morning. Passed a very restless night, not seeming to feel the effects of the morphine. Temperature, 102; pulse, 120; respiration, 32, and very labored. It was ascertained on examination that he was suffering from an asthmatic attack. He has had no spitting of blood and no sign of any lung trouble. Ordered grains x of iodide of potash, three times a day. July 5.—Evening. Complains of great pain in the vicinity of the wound, extending toward the stomach. Temperature, 102; skin feeling to the hand cold and covered with a clammy sweat. Pulse, 80; quite feeble and compressible, intermitting at every second beat. Respiration, 30; not labored, having recovered from his asthmatic attack.

July 6.—Passed a very restless night; one-eighth grain of morphia given every two hours; temperature, 103; pulse, 110; respiration, 40; labored and sighing; slight hemorrhage from wound; all pain left him.

July 7.—Passed a quiet night, sleeping very well; only one-eighth grain of morphia administered; temperature, 101; pulse, 106; respiration, 18; slight hemorrhage from wound; expectoration of a sputa which looks very much like laudable pus.

July 8.—Slept quite well, taking one-eighth grain of morphia; complains of some pain in vicinity of wound; hemorrhage from wound ceased; has taken no food since admission, being sustained by stimulants, beef tea, milk, etc.; temperature, 102; pulse, 115, quite strong, intermitting at every fifth beat; respiration, 26.

July 9.—Very comfortable night, taking only one-

eighth grain of morphia; wound discharging laudable pus; temperature, 101½; pulse, 110; respiration, 24.

July 10.—Restless night, no morphia being given. Wound still discharging healthy pus. Temperature, 101½; pulse, 110; respiration, 26.

July 11.—Temperature, 101; pulse, 110; respiration, 24.

July 12.—Temperature, 102; pulse, 110; respiration, 22. Ordered digitalis.

July 13.—Temperature, 100½; pulse, 100; respiration, 22. Urine containing traces of albumen. Solid food taken and retained.

July 14.—Temperature, 101; pulse, 108; respiration, 20.

July 15.—Temperature, 100½; pulse, 95; respiration, 22.

July 16.—Temperature, 100½; pulse, 100; respiration, 22.

July 17.—Temperature, 101; pulse, 100; respiration, 23.

July 18.—Temperature, 101; pulse, 100; respiration, 22.

July 19.—Very restless night. Temperature, 101½; pulse, 130; respiration, 34. Complains of pain in the region of the heart.

July 20.— Temperature 101½; pulse, 120; respiration, 34.

July 21.—Temperature, 101; pulse, 112; respiration, 32.

July 23.—Restless night, troubled much by a short, hacking cough; wound entirely healed. Temperature, 100¾; pulse, 106; respiration, 32. Vomited his breakfast.

July 24.—Passed a restless night notwithstanding the free use of bromide. Temperature, 103; pulse, 130; respiration, 38. Still troubled with cough, which distresses him greatly; cannot retain solid food. Stimulants freely given.

July 25.—Slept better, but cough still troubles him; breathing labored. Temperature, 100¾; pulse, 65; respiration, 39; muscular twitching of hands and feet.

July 26.—Much more comfortable this morning. Temperature, 100½; pulse, 92; respiration, 40; digitalis discontinued.

July 27.—Temperature, 99; pulse, 58; respiration. 36.

July 28.—Temperature, 98½; pulse, 56; respiration, 30.

July 29.—Temperature, 99; pulse. 60; respiration. 32.

July 30.—Delirious during the night, attempted to get out of bed. Temperature, 99½; pulse, 52; strong and full; respiration, 28.

July 31.—Temperature 99½; pulse, 68; respiration, 32. Delirious during night. Bromides given freely.

August 1.—Temperature, 100; pulse, 52; strong and full; respiration, 34.

August 2.—Temperature, 98½; pulse, 51; respiration. 30. Delirious during night.

August 3.—Temperature, 98½; pulse, 108; respiration, 22. Troubled very much with attacks of coughing.

August 4.—Temperature, 98½; pulse, 100; respiration, 24.

August 5.—Temperature, 98½; pulse, 96; respiration, 24.

August 6.—Temperature, 100; pulse, 96; respiration, 20.

August 7.—Temperature, 99; pulse 94; respiration 19.

August 8—Temperature, 98¾; pulse, 88; respiration, 22; sleeps well; appetite, good.

August 9.—Temperature, 98½; pulse, 90; respiration, 20.

August 13.—Temperature, pulse and respiration have remained the same as on August 9. The patient for the first time to-day since his injury has been allowed to get up and dress.

August 18.—Doing well since last report. Walks around the wards; eats and sleeps well, the bullet remaining in his body.

ON THE GERM THEORY.*

By Prof. Pasteur.

"The subject of my communication is vaccination in relation to chicken cholera and splenic fever, and a statement of the method by which we have arrived at these results—a method the fruitfulness of which inspires me with boundless anticipations. Before discussing the question of splenic fever vaccine, which is the most important, permit me to recall the results of my investigations of chicken cholera. It is through this inquiry that new and highly important principles have been introduced into science concerning the virus or contagious quality of transmissible diseases. More than once in what I am about to say I shall employ the expression virus-culture, as formerly, in my investigations on fermentation, I used the expressions, the culture of milk ferment, the culture of the butyric vibrion, etc. Let us take, then, a fowl which is about to die of chicken cholera, and let us dip the end of a delicate glass rod in the blood of the fowl with the usual precautions, upon which I need not here dwell. Let us then touch with this charged point some *bouillon de poule*, very clear, but first of all rendered sterile under a temperature of about 115° centigrade, and under conditions in which neither the outer air nor the vases employed can introduce exterior germs —those germs which are in the air, or on the surface of all objects. In a short time, if the little culture vase is placed in a temperature of 25° to 35°, you will see the liquid become turbid and full of tiny microbes, shaped like the figure 8, but often so small that under a high magnifying power they appear like points. Take from this vase a drop as small as you please, no more than can be carried on the point of a glass rod as sharp as a needle, and touch with this point a fresh quantity of sterilized *bouillon de poule* placed in a second vase, and the same phenomenon is produced. You deal in the same way with a third culture vase, with a fourth, and so on to a hundred, or even a thousand, and invariably within a few hours the culture liquid becomes turbid and filled with the same minute organisms.

"At the end of two or three days' exposure to a temperature of about 30° C. the thickness of the liquid disappears, and a sediment is formed at the bottom of the vase. This signifies that the development of the minute organism has ceased—in other words, all the little points which caused the turbid appearance of the liquid have fallen to the bottom of the vase, and things will remain in this condition for a longer or shorter time, for months even, without even the liquid or the deposit undergoing any visible modification, inasmuch as we have taken care to exclude the germs of the atmosphere. A little stopper of cotton sifts the air which enters or issues from the vase through changes of temperature. Let us take one of our series of culture preparations—the hundredth or the thousandth, for instance—and compare it in respect to its virulence with the blood of a fowl which has died of cholera; in other words, let us inoculate under the skin ten fowls, for instance, each separately with a tiny drop of infectious blood, and ten others with a similar quantity of the liquid in which the deposit has first been shaken up. Strange to say, the latter ten fowls will die as quickly and with the same symptoms as the former ten; the blood of all will be found to contain after death the same minute infectious organisms. This equality, so to speak, in the virulence both of the culture preparation and of the blood is due to an apparently futile circumstance. I have made a hundred culture preparations—at least, I have understood that this was done—without leaving any considerable interval between

* "International Medical Congress." London, 1881.

the impregnations. Well, here we have the cause of the equality in the virulence.

"Let us now repeat exactly our successive cultures with this single difference, that we pass from one culture to that which follows it—from the hundredth to, say, the hundred and first, at intervals, of a fortnight, a month, two months, three months or ten months. If, now, we compare the virulence of the successive cultures, a great change will be observed. It will be readily seen from an inoculation of a series of ten fowls that the virulence of one culture differs from that of the blood and from that of a preceding culture when a sufficiently long interval elapses between the impregnation of one culture with the microbe of the preceding. More than that, we may recognize by this mode of observation that it is possible to prepare cultures of varying degrees of virulence. One preparation will kill eight fowls out of ten, another five out of ten, another one out of ten, and another none at all, although the microbe may still be cultivated. In fact, what is no less strange, if you take each of these cultures of attenuated virulence as a point of departure in the preparation of successive cultures and without appreciable interval in the impregnation, the whole series of these culures will reproduce the attenuated virulence of that which has served as the starting point. Similarly, where the virulence is null it produces no effect. How, then, it may be asked, are the effects of these attenuating virulences revealed in the fowls? They are revealed by a local disorder, by a morbid modification more or less profound in a muscle, if it is a muscle which has been inoculated with the virus. The muscle is filled with microbes which are easily recognized, because the attenuated microbes have almost the bulk, the form, and the appearance of the most virulent microbes.

"But why is not the local disorder followed by death? For the moment let us answer by a statement of facts. They are these: the local disorder ceases of itself more or less speedily, the microbe is absorbed and digested, if one may say so, and little by little the muscle regains its normal condition. Then the disease has disappeared. When we inoculate with the microbe, the virulence of which is null, there is not even local disorder, the *naturæ medicatrix* carries it off at once, and here, indeed, we see the influence of the resistance of life, since this microbe, the virulence of which is null, multiplies itself. A little farther, and we touch the principle of vaccination. When the fowls have been rendered sufficiently ill by the attenuated virus which the vital resistance has arrested in its development, they will, when inoculated with virulent virus, suffer no evil effects, or only effects of a passing character. In fact, they no longer die from the mortal virus, and for a time sufficiently long, which in some cases may exceed a year, chicken cholera cannot touch them, especially under the ordinary conditions of contagion which exist in fowl-houses. At this critical point of our manipulation—that is to say, in this interval of time which we have placed between two cultures, and which causes the attenuation—what occurs? I shall show you that in this interval the agent which intervenes is the oxygen of the air. Nothing more easily admits of proof. Let us produce a culture in a tube containing very little air, and close this tube with an enameller's lamp. The microbe in developing itself, will speedily take all the oxygen of the tube and of the liquid, after which it will be quite free from contact with oxygen. In this case it does not appear that the microbe becomes appreciably attenuated, even after a great lapse of time. The oxygen of the air, then, would seem to be a possible modifying agent of the virulence of the microbe of chicken cholera—that is to say, it may modify more or less facility of its development in the body of animals. May we not be here in presence of a general law applicable to all kinds of virus? What benefits may not be the result? We may hope to discover in this way the vaccine of all virulent diseases; and what is more natural than to begin our investigation of the vaccine of what we in French call charbon, what you in England call splenic fever, and what in Russia is known as the Siberian pest, and in Germany as the Milzbrand.

"In this new investigation I have had the assistance of two devoted young *savants*—MM. Chamberland and Roux. At the outset we were met by a difficulty. Among the inferior organisms, all do not resolve themselves into those corpuscle germs which I was the first to point out as one of the forms of their possible development. Many infectious microbes do not resolve themselves in their cultures into corpuscle germs. Such is equally the case with beer yeast which we do not see develop itself usually in breweries, for instance, except by a sort of scissiparity. One cell makes two or more, which form themselves in wreaths; the cells become detached, and the process recommences. In these cells real germs are not usually seen. The microbe of chicken-cholera and many others behave in this way, so much so that the cultures of this microbe, although they may last for months without losing their power of fresh cultivation, perish finally like beer yeast which has exhausted all its aliments. The anthracoid microbe in artificial cultures behaves very differently. In the blood of animals, as in cultures, it is found in translucid filaments more or less segmented. This blood or these cultures freely exposed to air, instead of continuing according to the first mode of generation, show at the end of forty-eight hours corpuscle germs distributed in series more or less regular along the filaments. All around these corpuscles matter is absorbed. as I have represented it formerly in one of the plates of my work on the diseases of silkworms. Little by little all connection between them disappears, and presently they are reduced to nothing more than germ dust.

"If you make these corpuscles germinate, the new culture reproduces the virulence peculiar to the thready form which has produced these corpuscles, and this result is seen even after a long exposure of these germs to contact with air. Recently we discovered them in pits in which animals dead of splenic fever had been buried for twelve years, and their culture was as virulent as that from the blood of an animal recently dead. Here I regret extremely to be obliged to shorten my remarks. I should have had much pleasure in demonstrating that the anthracoid germs in the earth of pits in which animals have been buried are brought to the surface by earthworms, and that in this fact we may find the whole etiology of disease, inasmuch as the animals swallow these germs with their food. A great difficulty presents itself when we attempt to apply our method of attenuation by the oxygen of the air to the anthracoid microbes. The virulence establishing itself very quickly, often after twenty-four hours in an anthracoid germ which escapes the action of the air, it was impossible to think of discovering the vaccine of splenic fever in the conditions which had yielded that of chicken-cholera. But was there, after all, reason to be discouraged? Certainly not; in fact, if you observe closely, you will find that there is no real difference between the mode of the generation of the anthracoid germ by scission and that of chicken-cholera. We had therefore reason to hope that we might overcome the difficulty which stopped us by endeavoring to prevent the anthracoid microbe from producing corpuscle germs, and to keep it in this condition in contact with oxygen for days, and weeks, and months. The experiment fortunately succeeded.

"In the ineffective (*neutre*) *bouillon de poule* the anthracoid microbe is no longer cultivable at 45° C. Its culture, however, is easy at 42° or 43°, but in these conditions the microbe yields no spores. Consequently it is possible to maintain in contact with the pure air at 42° or 43° a *mycélienne* culture of bacteria entirely free of germs. Then appear the very remarkable results which follow. In a month or six weeks the culture dies—that

is to say, if one impregnates with it fresh *bouillon*, the latter is completely sterile. Up to that time life exists in the vase exposed to air and heat. If we examine the virulence of the culture at the end of two days, four days, six days, eight days, etc., it will be found that long before the death of the culture the microbe has lost all virulence, although still cultivable. Before this period it is found that the culture presents a series of attenuated virulences. Everything is similar to what happens in respect to the microbe in chicken cholera. Besides, each of these conditions of attenuated virulence may be reproduced by culture; in fact, since the charbon does not operate a second time (*ne récidive pas*), each of our attenuated anthracoid microbes constitutes for the superior microbe a vaccine—that is to say, a virus capable of producing a milder disease. Here, then, we have a method of preparing the vaccine of splenic fever. You will see presently the practical importance of this result, but what interests us more particularly is to observe that we have here a proof that we are in possession of a general method of preparing virus vaccine based upon the action of the oxygen and the air—that is to say, of a cosmic force existing everywhere on the surface of the globe.

"I regret to be unable, from want of time, to show you that all these attenuated forms of virus may very easily, by a physiological artifice, be made to recover their original maximum virulence. The method I have just explained of obtaining the vaccine of splenic fever was no sooner made known than it was very extensively employed to prevent the splenic affection. In France we lose every year, by splenic fever, animals of the value of twenty million francs. I was asked to give a public demonstration of the results already mentioned. This experiment I may relate in a few words. Fifty sheep were placed at my disposition, of which twenty-five were vaccinated. A fortnight afterward the fifty sheep were inoculated with the most virulent anthracoid microbe. The twenty-five vaccinated sheep resisted the infection; the twenty-five unvaccinated died of splenic fever within fifty hours. Since that time my energies have been taxed to meet the demands of farmers for supplies of this vaccine. In the space of fifteen days we have vaccinated in the departments surrounding Paris more than twenty thousand sheep, and a large number of cattle and horses. If I were not pressed for time I would bring to your notice two other kinds of virus attenuated by similar means. These experiments will be communicated by-and-by to the public. I cannot conclude, gentlemen, without expressing the great pleasure I feel at the thought that it is as a member of an international medical congress assembled in England that I make known the most recent results of vaccination upon a disease more terrible, perhaps, for domestic animals than small-pox is for man. I have given to vaccination an extension which science, I hope, will accept as a homage paid to the merit and to the immense services rendered by one of the greatest men of England, Jenner. What a pleasure for me to do honor to this immortal name in this noble and hospitable city of London!"

FROM a privately issued report on silk cultivation in the Chinese province of Kwangtung, we learn that in the Pakhoi district, on the southern seaboard, wild silkworms are found which feed on the camphor tree, and their silk is utilized in a singular manner. When the caterpillar has attained its full size, and is about to enter the *pupa* state, it is cut open and the silk extracted in a form much resembling catgut. This substance, having undergone a process of hardening, makes excellent fish line, and is generally used for that purpose in the Pakhoi district.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

Mr. Samuel J. Wallace, commenting on my paper on "The Use of Water as a Fuel" ("SCIENCE," Vol. II., p. 321), in an interesting communication to you ("SCIENCE," Vol. II., p. 373), suggests an inadvertancy on my part in "not more clearly distinguishing between the degrees of temperature at which the transfer of oxygen takes place from the hydrogen of the water to the carbon set free by the dissociation of the naphtha and the number of heat units set free or absorbed by such transfer, which is a very different thing."

To this I would state in reply that I have purposely refrained from an elaborate calculation of the thermal effects in heat units for several reasons. Of these I shall detail but a few of the more important at present.

In the first place, my intention was to give the scientific rationale of the chemical processes involved in the generation of the tremendous heat produced by the Holland retort with so insignificant an amount of naphtha; and, furthermore, I wanted to show that the application of the principle of the correlation of forces and conservation of energy to this new and original process of combustion has been undertaken heretofore on an erroneous assumption; lastly, I intended to prove, in the shortest and clearest possible manner, what a proportion of heat was gained, and in what manner—viz., by the dissociation of steam in the presence and by the agency of the carbon contained in the naphtha.

For these and other reasons, I avoided long explanations and calculations of other points, such as, for instance, the "dissociation of the naphtha," as Mr. Wallace puts it, and the figuring up of the heat units generated by the several elements on combustion. In order to re-affirm my position, which is, on most points, not that assumed by Mr. Wallace, I may be allowed to offer the following remarks:

It is self-evident that the carbon of the naphtha, in order to act independently, must first be set free; this is accomplished by the heating of the naphtha, in its chamber of the retort, up to the point of gasification. On meeting the steam in the manifold, the carbon of the naphtha leaves its hydrogen and forthwith unites with the oxygen of the watery vapor, forming *either* carbonic oxide *or* carbonic acid, according to the amount of steam introduced.

Thus there is certainly a decomposition of the naphtha into its elements, as Mr. Wallace intimates; but by far the most important process is the *dissociation of the watery vapor* which Mr. Wallace refuses to recognize, insisting, as he does, that there is only a *transfer* of the oxygen from the hydrogen of the steam to the carbon of the naphtha. How this is possible, without the previous dissociation of the steam, I am unable to understand. Mr. Wallace furnishes, indeed, the best argument against his own statement, by mentioning the well-known fact that the carbon in the naphtha is very loosely held by its hydrogen. But it is also a well-known fact that the oxygen of the steam is very tenaciously held by its hydrogen, so much so that it was considered impossible to separate, *to dissociate*, them by heat for a long time. Not until the late Henri St. Clair Deville * devised an appa-

* It is with profound grief that the announcement of the great chemist's death has been received everywhere. At his funeral (July 5th) M. Pasteur made an eloquent speech. The London *Chemical News* has an obituary in which occurs the following passage: "His highest achievement, from a strictly scientific point of view, was the establishment of the laws of dissociation. Previously, decomposition was regarded as a simple phenomenon, effected and completed, in the case of every substance, at a fixed temperature. Deville showed that in some cases it is effected within certain limits of temperature, being arrested at a given heat by the equilibrium established between the decomposing body and the product of decomposition."

ratus *for this purpose* was this dissociation accomplished by heat alone.

The reason for this state of things is apparent. Hydrogen in uniting with oxygen to form water develops the greatest amount of heat, a greater amount, in fact, than an equal weight of any other known element. And just here I would ask Mr. Wallace which authority has stated that the "absolute heat of carbon and hydrogen are almost exactly equal in complete burning." On the contrary, all authorities agree, and all investigators have established beyond doubt the fact, that hydrogen develops *more than four times as much heat* on combustion than does an equal weight of carbon. The figures at present universally accepted as a standard are those determined by Favre and Silberman, during their carefully conducted and numerous experiments. According to them—

1 grain of hydrogen develops 34,462 units of heat.
1 grain of carbon " 8,080 units of heat.

on complete combustion.

This great advantage in the heat-producing power of hydrogen is the principal reason why scientists have constantly striven to substitute this element for carbon, which is now universally used as fuel. But until lately only this was thought impracticable, because it was believed that the same amount of heat was necessary to obtain the hydrogen by the dissociation of water, which would ultimately be obtained by the combustion of said hydrogen. And even the processes of Strong and others for the generation of so-called 'water-gas' have not changed this erroneous view. For, the advantages arrived at by them were ascribed rather to various extraneous causes* than to the one principal cause, *i. e.*, the dissociation of steam by the chemical affinity of carbon, and the consequent generation of a not inconsiderable amount of hydrogen.

This very same line of argument has been followed in the discussions about the Holland process, and the principal aim of my paper was to controvert it, and to show that it is *not heat, but chemical affinity*, which does by far the greatest part of the work of dissociation. I was enabled to do so on the basis of Dahlerus' experiments, which have proved conclusively that carbon will dissociate water at 400° C, instead of 8000° C, which is Deville's figure for the dissociation-temperature of water in the absence of any other element.†

From the foregoing the readers of "SCIENCE" will perceive that the enormous gain obtained by actual experiment with the Holland locomotive is satisfactorily explained on scientific grounds.

All the further argumentation of Mr. Wallace covers the earlier water-gas processes of Lowe, Strong and others; they generate their gaseous fuel in a separate contrivance (the generator) and afterward burn it. For this reason they want to accomplish only the first stage of carbon, combustion water-gas, consisting of hydrogen and carbonic oxide. In the process under consideration, however, the retort which prepares the fuel, *i. e.*, gasifies the naphtha and the water, is placed just where the heating is to take place. All the heat, therefore, that is developed by the carbon combustion is utilized, as is also all the heat which is developed by the dissociated hydrogen burning up with atmospheric oxygen.

Indeed, this process is the only one which comes pretty near to fulfill all the requirements of an ideal method. These are;

1. Gaseous condition of the fuel.
2. Complete combustion (no smoke, no ashes).
3. Full effects of the caloric energy developed.
4. Regulation of the draught-air, so as to admit the least amount of atmospheric air practicable.
5. Greatest possible percentage of oxygen in atmosphere of combustion. (The oxygen derived from the dissociation of the steam being employed for the combustion of carbon, the necessary draught-air is thereby materially reduced and thus the percentage of oxygen increased).
6. Universal adaptibility (kitchen and parlor stoves, fire places, stationary and other boilers, locomotives and ocean steamers can be accommodated with it, and illuminating gas is prepared automatically by an additional chamber in the retort).
7. Simplicity of apparatus. (May be managed by the turning of a few faucets.)
8. Cheapness of the fuel employed. (Water is certainly to be obtained everywhere at small expense, while the price of naphtha is only three cents a gallon if bought by the single barrel.)
9. Fuel used of the greatest heating capacity, with each atom of Carbon burned, there are burned at the same time *four* atoms of Hydrogen, thus:

$$\underset{\text{(Naphtha.)}}{C_2H_4} + \underset{\text{(Water.)}}{4\,HO} = 2\,(CO_2) + 8\,H.$$

The eight Hydrogen are burned with atmospheric Oxygen.

Crude oil *i. e.* petroleum that has undergone one distillation, to free it from its mineral and waxy ingredients, may be used, but would be much dearer. Naphtha, it may be said in conclusion, is not one of the distillation products, as might be inferred from the name: It is the unused residue from all the various distillation-processes to which petroleum is subjected. *It is a waste product* and therefore cheaper than anything else.

It is obtained in this wise:

The crude oil as it is received from the stills of the petroleum regions is subjected to twelve successive distillations, and the following products result:

Cymogene.
Rhigolene.
Gasolene.
C. Naphtha.
B. Naphtha.
A. Naphtha.
Benzine.
Kerosene.
Mineral Sperm Oil.
Lubricating Oil.
Paraffin.

Of these only the last four are completely used in the arts and for illuminating purposes. The unused residue of all the others is thrown back into the residue remaining from the last distillation. The quality of the mixture called Naphtha, and used in the Holland process is therefore not always the same, but this does not at all alter its value as a fuel, as it does not alter the main features of this process, as they have been explained in our remarks.

GEO. W. RACHEL, M. D.

* Some of these are: gasification of the fuel, substitution of the second for the first stage of carbon—combustion, reduction of the amount of draft-air, etc.

† The combustion- (and dissociation-) temperature may be found by calculation from the thermal effect and the specific heat of the product (*i. e.*, water) in the following manner, according to MOHR (Mech. Theorie d. chem. Affin., p. 102):

If one part of hydrogen burns up with eight parts of oxygen, forming nine parts of water, 34462 units of heat are generated which are contained in the watery vapor thus formed. If the specific heat of steam would be the same as that of water, the actual temperature of these nine parts of water would be:

$$\frac{34462}{9} = 38290^\circ \text{ C.}$$

Since the specific heat of steam is, however: .475, the actual temperature of the nine parts of watery vapor is:

$$\frac{3829}{.475} = 8061^\circ \text{ C.}$$

For this reason 8000° C has been assumed as the dissociation-temperature of watery vapor.

King's Cross Station, Great Northern Railway, is now lighted by means of electricity, a beginning having been made last week by means of the Crompton system. There are 12 Crompton lamps within the station, six being placed over the arrival, and a similar number over the departure platform. Two other lamps of larger size are placed outside the station building. The interior area lighted consists of two bays, each 880 feet long and 105 feet wide, and 72 feet high, as well as the cab-rank adjoining the arrival platform, which is 40 feet wide. The total area lighted is 220,000 square feet, giving an area of 18,133 square feet, or nearly half an acre, to each lamp. The lamps are suspended at a height of 30 feet from the platform level, and are arranged on four circuits, the light of each lamp being computed as equivalent to 4000 candles. Any unpleasantness from the intensity of the light is obviated by the use of semi-transparent glass in the lower portion of the lanterns. The two exterior lights are estimated at 6000 candles each, are placed at an altitude of 70 feet, the lanterns being of clear glass. The current is supplied by means of five Burgin dynamo-electric machines, which are driven by a semiportable engine by Messrs. Marshall, Sons & Co., of Gainsborough, working up to 35-horse power.

THE death of John Duncan, the Alford, England, botanist, is announced as having taken place last week in his 85th year. The deceased adopted the occupation of a weaver by trade, but devoted all his spare time to the study of botany. His splendid collection of plants he handed over to Aberdeen University a year ago, but he has lived barely six months to enjoy the fund which public recognition of his merits placed at his disposal in his declining years. The story of John Duncan's life is to be told by Mr. Jolly, himself an enthusiastic botanist.

Various attempts have been made to explain the tails of comets. A recent one by M. Picart is as follows: The Sun, the stars, nebulæ and comets, are composed not only of ponderable matter in the gaseous state, but of imponderable matter, the luminous ether, revealed, in the case of the sun by the zodiacal light, and in that of nebulæ, by their irregular forms contrary to gravitation. A comet far from the sun, appears in spheroidal form, due to gravitation of its ponderable matter (its luminous ether being then invisible because of distance and feeble light). But on nearing the sun, the luminous ether of this body repels that of the comet (this being a characteristic property of the ether) so forming the tail. The form and direction of the tail are thus quite independent of gravitation; and the enormous velocity ceases to be a difficulty, as it is if the matter of the tail be thought ponderable. M. Lamey has observed that the solar light, being unable completely to penetrate the comet's tail, illumines only the left part, producing a true cometary phase.

THE assimilation of nitrogen by plants has, of late, been carefully studied by Signor Lamattina, of Rome, who arrives at the following results: Plants absolutely require to assimilate nitrogen, and they obtain it in three forms: (1) In the nitrates of the ground; (2) In the ammonia of the air; (3) In the State of protoxide in the atmosphere. The nitrogen in the state of nitrates, absorbed by the roots, is for transport and diffusion of mineral substances, principally potash, in the leaves, helping to form chlorophyll and hydrocarbons. The nitrogen absorbed in the form of ammonia by respiration, serves for formation of albuminoids, fibrine, etc. The nitrogen absorbed in the state of protoxide, appears to serve as complement of the food of the plant, acting both as corrective, by neutralising the basis in excess, and helping in the formation of alkaloids.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING AUG. 27, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

AUGUST.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 21	29.628	29.678	12 p. m.	29.598	5 p. m.	77.0	69.0	85	5 p. m.	73	5 p. m.	68	5 a. m.	65	5 a. m.	144.
Monday, 22	29.802	29.896	12 p. m.	29.678	o a. m.	74.0	66.0	81	3 p. m.	69	2 p. m.	67	12 p. m.	62	12 p. m.	139.
Tuesday, 23	29.979	30.046	12 p. m.	29.896	o a. m.	72.3	64.0	83	5 p. m.	69	2 p. m.	61	6 a. m.	59	6 a. m.	140.
Wednesday, 24	30.138	30.196	12 p. m.	30 046	o a. m.	73.3	66.6	82	2 p. m.	73	2 p m.	63	5 a. m.	60	6 a. m.	141.
Thursday, 25	30.200	30.212	9 a. m.	30.168	6 p. m.	70.3	64.6	76	3 p. m.	67	3 p. m.	65	3 a. m.	62	3 a. m.	131.
Friday, 26	30.151	30.198	o a. m.	30.110	4 p. m.	71.7	67.0	82	3 p. m.	73	3 p. m.	63	7 a. m.	62	8 a. m.	134.
Saturday, 27	30.114	30.156	9 a. m.	30.072	6 p. m.	72.0	66.7	78	2 p. m.	70	2 p. m.	66	6 a. m.	64	6 a. m.	128.

Mean for the week .... 30.002 inches.
Maximum for the week at 9 a. m., August 25th .... 30.212 "
Minimum " at 5 p. m., August 21st .... 29.598 "
Range .... .614 "

	Dry.		Wet.	
Mean for the week	72.9	degrees	66.2	degrees.
Maximum for the week, at 5 pm. 21st	85.	" at 5 pm 21st,	73.	"
Minimum " " 6 am. 23d	61.	" at 6 am 23d,	59.	"
Range " " ---- ----	24.	" ----	14.	"

AUGUST.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast. 10. 7 a. m	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE.
Sunday, 21	n.	n. n. e.	w. n.w.	123	3¼	11 am	.537	.623	.652	71	53	73	3cir.cu.s	3 cu. s.	10	11 p m	11½ p m	0.30	.01	3
Monday, 22	n. w.	n. w.	n. n. w.	153	3	3.30 pm	.516	.547	.537	70	52	71	0	3 cu.	0	-----	-----	-----	--	7
Tuesday, 23	n. w.	n. w.	n. n. w.	155	2¼	4.40 pm	.433	.547	.489	72	52	62	0	3 cu.	0	-----	-----	-----	--	1
Wednesday, 24	n. n. e.	n. e.	s. e.	121	4	1.30 pm	.462	.663	.564	65	63	79	0	1 cir. cu.	0	-----	-----	-----	--	0
Thursday, 25	s. s. e.	s.	s. s. w.	140	2¼	3.20 pm	.509	.554	.543	74	64	79	8 cu.	0	0	-----	-----	-----	--	0
Friday, 26	w. s. w.	s.	s. s. w.	150	2	3.00 pm	.576	.624	.608	100	59	80	10	1 cu.	0	-----	-----	-----	--	0
Saturday, 27	s. w.	s.	s. s. w.	140	2	4.40 pm	.543	.625	.586	79	65	80	8 cu.	1 cu.	0	-----	-----	-----	--	0

Distance traveled during the week .... 982 miles.
Maximum force .... 4 lbs.

Total amount of water for the week .... .01 inch.
Duration of rain .... 00 hours, 30 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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Residents of New York city who visited Cincinnati on the occasion of the meeting of the American Association for the Advancement of Science, doubtless returned with a better appreciation of the water supply of their own city.

Cincinnati draws its supply of water direct from the Ohio river, at a point within the city limits, and within a few yards of the outlet of a main sewer which discharges its abominations into the already discolored and muddy waters of the river.

Some idea may be formed of the condition of this water, when we state, that a small quantity poured into a washing basin, obscured the view of the bottom of the utensil, so opaque is the water by reason of its muddy impurity. And yet, the river at this time was at its best, for, undisturbed by rains or floods, it flowed past the city reduced to its lowest limits, and in its highest condition of purity.

Unanimity among the population of a large city on any one point, is not to be expected, but, it was with some surprise we heard expressions of admiration regarding this water, from some Cincinnatians. The majority of the people, however, were disgusted with the water supply of the city, and many were seeking their own remedy by the construction of artesian wells. The public press of Cincinnati, during our visit was loud in its denunciations of the evil, making excellent suggestions for obtaining the water supply from a purer source, and other needed improvements.

Recently the question has been much discussed, as to whether a city should draw its supply from a river, or from lakes and storage reservoirs. Which will give the best results?

This question is beset with many difficulties, and, in our opinion, cannot be determined in such a manner, that any particular decision for future guidance, *in all cases*, can be delivered. We apprehend that local causes and conditions which vary for every locality, having due weight and being well considered, should decide the question.

Of course absolute purity is not demanded, neither is it essential. The object to be aimed at, and that must be secured at any cost, is such a condition of purity which may be expressed by the term "fitness."

A water that is free from any impurities dangerous to health, of a good color and inodorous, may be considered "fit" for the supply of a city.

The question as to the best source for a supply of water, has of late received much attention from chemists and sanitary engineers. Reviewing the discussions, we express the opinion, that water drawn from a river which is free from sewage contaminations and not subject to discoloration, is preferable to water collected in lakes and storage reservoirs. The storage of water in reservoirs for long periods, without doubt, causes a deterioration in the quality of the water, generating a variety of animal and vegetable forms that are characteristic of stagnant waters, and which are dangerous to health. River water, on the contrary, if not contaminated directly near the source of supply, is usually free from those impurities which are most undesirable.

On this point we refer our readers to "Science," Vol. I. page 67, where will be found an analysis of the water supply of Newark, N. J., obtained from the river Passaic, contrasted with water used in that city, obtained from driven wells. The result showed that the water from the Passaic river, although contaminated with sewage to a certain extent, and below what may be considered a satisfactory condition, stood at the head of the list in regard to purity and general fitness for sanitary purposes. We believe that recently Professor Leeds, of Hoboken, has made analyses of the same waters, with very similar results.

But, from whatever source water may be obtained, a certain amount of manipulation appears to be essential before it is fit for distribution in a city. In the first place it should be held in a reservoir for 24 hours, to permit the suspended matter to subside; it should go through some simple process of filtration; and, lastly, be pumped to a sufficient elevation to secure a supply of water to the upper part of every house in the city.

The question of the public filtration of water for city use no doubt presents many difficulties, but until such filtration is accomplished by the authorities, every householder should make use of a filter, to cleanse from impurities, the water used for drinking and cooking purposes; for apart from the question of health, the interest of the public in securing pure water is

not confined to its use as an article of diet, because for all purposes for which water is employed, the purer it is, the better it is adapted for use.

THE CONNECTION OF THE BIOLOGICAL SCIENCES WITH MEDICINE.*

By T. H. Huxley, LL.D.

"The great man whose name is inseparably connected with the foundation of medicine, Hippocrates certainly knew very little—indeed, practically nothing—of anatomy or physiology; and he would probably have been perplexed even to imagine the possibility of a connection between the zoölogical studies of his contemporary, Democritus, and medicine. Nevertheless, in so far as he and those who worked before and after him in the same spirit ascertained, as matters of experience, that a wound or a luxation, or a fever, presented such and such symptoms, and that the return of the patient to health was facilitated by such and such measures, they established laws of Nature and began the construction of the science of pathology. All true science begins with empiricism, though all true science is such exactly in so far as it strives to pass out of the empirical stage into that of the deduction of empirical from more general truths. Thus, it is not wonderful that the early physicians had little or nothing to do with the development of biological science; and, on the other hand. that the early biologists did not much concern themselves with medicine. There is nothing to show that the Asclepiads took any prominent share in the work of founding anatomy, physiology, zoölogy and botany. Rather do these seem to have sprung from the early philosophers, who were essentially natural philosophers, animated by the characteristically Greek thirst for knowledge as such. Pythagoras, Alcmæon, Democritus, Diogenes of Apollonia, are all credited with anatomical and physiological investigation; and though Aristotle is said to have belonged to an Asclepiad family, and not improbably owed his taste for anatomical and zoölogical inquiries to the teachings of his father, the physician Nicomachus, the 'Historia Animalium,' and the treatise 'De Partibus Animalium,' are as free from any allusion to medicine as if they had issued from a modern biological laboratory.

"It may be added, that it is not easy to see in what way it could have benefited a physician of Alexander's time to know all that Aristotle knew on these subjects. His human anatomy was too rough to avail much in diagnosis, his physiology was too erroneous to supply data for pathological reasoning. But when the Alexandrian school, with Erasistratus and Herophilus at their head, turned to account the opportunities of studying human structure afforded to them by the Ptolemies, the value of the large amount of accurate knowledge thus obtained to the surgeon for his operations, and to the physician for his diagnosis of internal disorders, became obvious, and a connection was established between anatomy and medicine, which has ever become closer and closer. Since the revival of learning, surgery, medical diagnosis, and anatomy have gone hand in hand. Morgagni called his great work 'De Sedibus et Causis Morborum per Anatomen Indagatis,' and not only showed the way to search out the localities and the causes of disease by anatomy, but himself travelled wonderfully far upon the road. Bichat, discriminating the grosser constituents of the organs and parts of the body one from another, pointed out the direction which modern research must take; until at length histology, a science of yesterday, as it seems to many of us, has carried the work of Morgagni as far as the microscope can take us, and has extended the realm of pathological anatomy to the limits of the invisible world.

"Thanks to the intimate alliance of morphology with medicine, the natural history of disease has, at the present day, attained a high degree of perfection. Accurate regional anatomy has rendered practicable the exploration of the most hidden parts of the organism, and the determination during life of morbid changes in them; anatomical and histological post-mortem investigations have supplied physicians with a clear basis upon which to rest the classification of diseases, and with unerring tests of the accuracy or inaccuracy of their diagnosis. If men could be satisfied with pure knowledge, the extreme precision with which, in these days, a sufferer may be told what is happening, and what is likely to happen, even in the most recondite parts of his bodily frame, should be as satisfactory to the patient as it is to the scientific pathologists who gives him the information. But I am afraid it is not; and even the practising physician, while nowise underestimating the regulative value of accurate diagnosis, must often lament that so much of his knowledge rather prevents him from doing wrong than helps him to do right. A scorner of physic once said that Nature and disease may be compared to two men fighting, the doctor to a blind man with a club, who strikes into the *mêlée* sometimes hitting the disease and sometimes hitting all Nature. The matter is not mended if you suppose the blind man's hearing to be so acute that he can register every stage of the struggle and pretty clearly predict how it will end. He had better not meddle at all until his eyes are opened—until he can see the exact position of the antagonists, and make sure of the effects of his blows. But that which it behooves the physician to see, not indeed with his bodily eye, but with clear intellectual vision, is a process, and the chain of causation involved in that process. Disease, as we have seen, is a perturbation of the normal activities of a living body; and it is and must remain unintelligible so long as we are ignorant of the nature of these normal activities. In other words, there could be no real science of pathology until the science of physiology had reached a degree of perfection unattained, and indeed unattainable, until quite recent times.

"So far as medicine is concerned, I am not sure that physiology, such as it was down to the time of Harvey, might as well not have existed. Nay, it is, perhaps, no exaggeration to say that, within the memory of living men, justly renowned practitioners of medicine and surgery knew less physiology than is now to be learned from the most elementary text book, and, beyond a few broad facts, regarded what they did know as of extremely little practical importance. Nor am I disposed to blame them for this conclusion; physiology must be useless, or worse than useless, to pathology, so long as its fundamental conceptions are erroneous. Harvey is often said to be the founder of modern physiology, and there can be no question that the elucidations of the function of the heart, of the nature of the pulse, and of the course of the blood, put forth in the ever-memorable little essay, 'De motu cordis,' directly worked a revolution in men's views of the nature and of the concatenation of some of the most important physiological processes among the higher animals, while indirectly their influence was perhaps even more remarkable. But, though Harvey made this signal and perennially important contribution to the physiology of the moderns, his general conception of vital processes was essentially identical with that of the ancients; and in the 'Exercitationes de generatione,' and notably in the singular chapter, 'De calido innato,' he shows himself a true son of Galon and of Aristotle. For Harvey, the blood possesses powers superior to those of the elements; it is the seat of a soul which is not only vegetative, but also sensitive and motor. The blood maintains and fashions all parts of the body, *idque summâ cum providentia et intellectu, in finem certum agens, quasi ratiocinio quodam uteretur*. Here is the doctrine of the *pneuma*, the product of the philosophical mould into which the animism of primitive men ran in Greece, in

* International Medical Congress London, 1881.

full force. Nor did its strength abate for long after Harvey's time. The same ingrained tendency of the human mind to suppose that a process is explained when it is ascribed to a power of which nothing is known except that it is the hypothetical agent of the process, gave rise, in the next century, to the animism of Stahl; and later to the doctrine of a vital principle, that *asylum ignorantiæ* of physiologists, which has so easily accounted for everything and explained nothing, down to our own times.

"Now, the essence of modern, as contrasted with ancient physiological science, appears to me to lie in its antagonism to animistic hypotheses and animistic phraseology. It offers physical explanations of vital phenomena, or frankly confesses that it has none to offer. And, so far as I know, the first person who gave expression to this modern view of physiology, who was bold enough to enunciate the proposition that vital phenomena, like all the other phenomena of the physical world, are in ultimate analysis, resolvable into matter and motion, was René Descartes. The fifty-four years of life of this most original and powerful thinker are widely over-lapped on both sides by the eighty of Harvey, who survived his younger contemporary by seven years, and takes pleasure in acknowledging the French philosopher's appreciation of his great discovery. In fact, Descartes accepted the doctrine of the circulation as propounded by 'Hervæus, médecin d'Angleterre,' and gave a full account of it in his first work, the famous 'Discours de la Méthode,' which was published in 1637, only nine years after the exercitation 'De motu cordis;' and, though differing from Harvey in some important points (in which it may be noted, in passing, Descartes was wrong and Harvey right), he always speaks of him with great respect. And so important does the subject seem to Descartes, that he returns to it in the 'Traitè des Passions,' and in the 'Traitè de l'Homme.'

"It is easy to see that Harvey's work must have had a peculiar significance for the subtle thinker, to whom we owe both the spiritualistic and the materialistic philosophies of modern times. It was in the very year of its publication, 1628, that Descartes withdrew into that life of solitary investigation and meditation of which his philosophy was the fruit; and, as the course of his speculations led him to establish an absolute distinction of Nature between the material and the mental worlds, he was logically compelled to seek for the explanation of the phenomena of the material world within itself, and having allotted the realm of thought to the soul, to see nothing but extension and motion in the rest of Nature. Descartes uses 'thought' as the equivalent of our modern term 'consciousness.' Thought is the function of the soul, and its only function. Our natural heat and all the movements of the body, says he, do not depend on the soul. Death does not take place from any fault of the soul, but only because some of the principal parts of the body become corrupted. The body of a living man differs from that of a dead man in the same way as a watch or other automaton (that is to say, a machine which moves of itself) when it is wound up, and has in itself the physical principal of the movements which the mechanism is adapted to perform, differs from the same watch or other machine when it is broken, and the physical principle of its movements no longer exists. All the actions which are common to us and the lower animals depend only on the conformation of our organs and the course which the animal spirits take in the brain, the nerves, and the muscles, in the same way as the movement of a watch is produced by nothing but the force of its spring and the figure of its wheels and other parts.

"Descartes' treatise on 'Man' is a sketch of human physiology in which a bold attempt is made to explain all the phenomena of life, except those of consciousness, by physical reasonings. To a mind turned in this direction Harvey's exposition of the heart and vessels as a hydraulic mechanism must have been supremely welcome. Descartes was not a mere philosophical theorist, but a hard-working dissector and experimenter, and he held the strongest opinion respecting the practical value of the new conception which he was introducing. He speaks of the importance of preserving health, and of the dependence of the mind on the body being so close that perhaps the only way of making men wiser and better than they are is to be sought in medical science. 'It is true,' says he, 'that as medicine is now practised it contains little that is very useful; but without any desire to depreciate, I am sure that there is no one, even among professional men, who will not declare that all we know is very little as compared with that which remains to be known; and that we might escape an infinity of diseases of the mind, no less than of the body, and even perhaps the weakness of old age, if we had a sufficient knowledge of their causes and of all the remedies with which nature has provided us.' * So strongly impressed was Descartes with this that he resolved to spend the rest of his life in trying to acquire such a knowledge of nature as would lead to the construction of a better medical doctrine.* The anti-Cartesians found material for cheap ridicule in these aspirations of the philosopher; and it is almost needless to say that, in the thirteen years which elapsed between the publication of the 'Discours' and the death of Descartes, he did not contribute much to their realization. But for the next century all progress in physiology took place along the lines which Descartes laid down.

"The greatest physiological and pathological work of the seventeenth century, Borelle's treatise 'De motu animalium,' is, to all intents and purposes, a development of Descartes' fundamental conception; and the same may be said of the physiology and pathology of Boerhaave, whose authority dominated in the medical world in the first half of the eighteenth century. With the origin of modern chemistry and electrical science, in the latter half of the eighteenth century, aids in the analysis of the phenomena of life, of which Descartes could not have dreamed, were offered the physiologist. And the greater part of the gigantic progress which has been made in the present century is a justification of the provisions of Descartes. For it consists essentially in a more and more complete resolution of the grosser organs of the living body into physico-chemical mechanisms. 'I shall try to explain our whole bodily machinery in such a way that it will be no more necessary for us to suppose that the soul produces such movements as are not voluntary than it is to think that there is in a clock a soul which causes it to show the hours.'† These words of Descartes might be appropriately taken as a motto by the author of any modern treatise on physiology.

"But though, as I think, there is no doubt that Descartes was the first to propound the fundamental conception of the living body as a physical mechanism, which is the distinctive feature of modern as contrasted with ancient physiology, he was misled by the natural temptation to carry out, in all its details, a parallel between the machines with which he was familiar, such as clocks and pieces of hydraulic apparatus and the living machine. In all such machines there is a central source of power, and the parts of the machine are merely passive distributors of that power. The Cartesian school conceived of the living body as a machine of this kind; and herein they might have learned from Galen, who, whatever ill use he may have made of the doctrine of "natural faculties," nevertheless had the great merit of perceiving that local forces play a great part in physiology. The same truth was recognized by Glisson, but it was first prominently brought forward in the Hallerian doctrine of the 'vis insita' of muscles. If muscle can contract without nerve, there is an end of the Cartesian me-

* Discours de la Méthode. 6mo. partie. Ed. Cousin. P. 193.

† De la Formation du Fœtus.

chanical explanation of its contraction by the influx of animal spirits.

"The discoveries of Trembley tended in the same direction. In the fresh water *Hydra* no trace was to be found of that complicated machinery upon which the performance of the functions in the higher animals was supposed to depend. And yet the hydra moved, fed, grew, multiplied, and its fragments exhibited all the powers of the whole. And, finally, the work of Caspar F. Wolff,‡ by demonstrating the fact that the growth and development of both plants and animals take place antecedently to the existence of their grosser organs, and are, in fact, the causes and not the consequences of organization (as then understood), sapped the foundations of the Cartesian physiology as a complete expression of vital phenomena. For Wolff, the physical basis of life is a fluid, possessed of a "vis essentialis' and a 'solidescibilitas;' in virtue of which it gives rise to organization; and, as he points out, this conclusionstrikes at the root of the whole iatro-mechanical system.

"In this country the great authority of John Hunter exerted a similar influence, though it must be admitted that the too sibylline utterances which are the outcome of Hunter's struggles to define his conceptions are often susceptible of more than one interpretation. Nevertheless, on some points Hunter is clear enough. For example, he is of opinion that 'spirit is only a property of matter' ('Introduction to Natural History,' page 6), he is prepared to renounce animism (l. c., p. 8), and his conception of life is so completely physical that he thinks of it as something which can exist in a state of combination in the food. 'The aliment we take in has in it, in a fixed state, the real life, and this does not become active until it has got into the lungs, for there it is freed from its prison' (Observations on Physiology,' p. 113). He also thinks that: 'It is more in accord with the general principles of the animal machine to suppose that none of its effects are produced from any mechanical principle whatever, and that every effect is produced from an action in the part, which action is produced by a stimulus upon the part which acts, or upon some other part with which this part sympathizes, so as to take up the whole action' (l. c., p. 152). And Hunter is as clear as Wolff, with whose work he probably was unacquainted, that 'whatever life is, it most certainly does not depend upon structure or organization' (l. c. p. 114).

"Of course, it is impossible that Hunter could have intended to deny the existence of purely mechanical operations in the animal body. But while with Borelli and Boerhaave, he looked upon absorption, nutrition, and secretion as operations effected by means of the small vessels, he differed from the mechanical physiologists, who regarded these operations as the result of the mechanical properties of the small vessels, such as the size, form, and disposition of their canals and apertures. Hunter, on the contrary, considers them to be the effect of properties of these vessels which are not mechanical, but vital. 'The vessels,' says he, 'have more of the polypus in them than any other part of the body,' and he talks of the 'living and sensitive principles of the arteries,' and even of the 'dispositions or feelings of the arteries.' 'When the blood is good and genuine, the sensations of the arteries, or the dispositions for sensation, are agreeable. . . . It is then they dispose of the blood to the best advantage, increasing the growth of the whole, supplying any losses, keeping up a due succession, etc.' (l. c., p. 133).

"If we follow Hunter's conceptions to their logical issue, the life of one of the higher animals is essentially the sum of the lives of all the vessels, each of which is a sort of physiological unit, answering to a polyp; and, as health is the result of the normal "action of the vessels," so is disease an effect of their abnormal action. Hunter thus stands in thought, as in time, midway between Borelli, on the one hand, and Bichat, on the other. The acute founder of general anatomy, in fact, outdoes Hunter in his desire to exclude physical reasonings from the realm of life. Except in the interpretation of the action of the sense organs, he will not allow physics to have anything to do with physiology. 'To apply the physical sciences to physiology is to explain the phenomena of living bodies by the laws of inert bodies. Now, this is a false principle, hence all its consequences are marked with the same stamp. Let us leave to chemistry its affinity, to physics its elasticity and its gravity. Let us invoke for physiology only sensibility and contractility'* Of all the unfortunate dicta of men of eminent ability this seems one of the most unhappy, when we think of what the application of the methods and the data of physics and chemistry has done towards bringing physiology into its present state. It is not too much to say that one half of a modern text-book of physiology consists of applied physics and chemistry, and that it is exactly in the exploration of the phenomena of sensibility and contractility that physics and chemistry have exerted the most potent influence.

"Nevertheless, Bichat rendered a solid service to physiological progress by insisting upon the fact that what we call life in one of the higher animals is not an invisible unitary archæus dominating from its central seat the parts of the organism, but a compound result of the synthesis of the separate lives of those parts. 'All animals,' says he, 'are assemblages of different organs, each of which performs its function and concurs, after its fashion, in the preservation of the whole. They are so many special machines in the general machine which constitutes the individual. But each of these special machines is itself compounded of many tissues of very different natures, which, in truth, constitute the elements of these organs (l. c., lxxix.) The conception of a proper vitality is applicable only to these simple tissues, and not to the organs themselves (l. c., lxxxiv.).' And Bichat proceeds to make the obvious application of this doctrine of synthetic life, if I may so call it, to pathology. Since diseases are only alterations of vital properties, and the properties of each tissue are distinct from those of the rest, it is evident that the diseases of each tissue must be different from those of the rest. Therefore, in any organ composed of different tissues, one may be diseased and the other remain healthy, and this is what happens in most cases (l. c., lxxxv.). In a spirit of true prophecy, Bichat says: 'We have arrived at an epoch in which pathological anatomy should start afresh.' For, as the analysis of the organ had led him to the tissues as the physiological units of the organism, so, in a succeeding generation, the analysis of the tissues led to the cell as the physiological element of the tissues. The contemporaneous study of development brought out the same result, and the zoölogists and botanists, exploring the simplest and the lowest forms of animated beings, confirmed the great induction of the cell theory. Thus the apparently opposed views which have been battling with one another ever since the middle of the last century have proved to be each half a truth.

"The proposition of Descartes, that the body of a living man is a machine, the actions of which are explicable by the known laws of matter and motion, is unquestionably largely true. But it is also true that the living body is a synthesis of innumerable physiological elements, each of which may nearly be described in Wolff's words, as a fluid possessed of a *vis essentialis*, and a *solidescibilitas;* or, in modern phrase, as protoplasm susceptible of structural metamorphosis and functional metabolism; and that the only machinery, in the precise sense in which the Cartesian school understood mechanism, is that which co-ordinates and regulates

‡ Theoris Generationis, 1759.

* Anatomie générale, i., p. liv.

these physiological units into an organic whole. In fact, the body is a machine of the nature of an army, not of that of a watch, or of a hydraulic apparatus. Of this army, each cell is a soldier, an organ a brigade, the central nervous system headquarters and field telegraph, the alimentary and circulatory system the commissariat. Losses are made good by recruits born in camp, and the life of the individual is a campaign, conducted successfully for a number of years, but with certain defeat in the long run.

"The efficacy of an army at any given moment depends on the health of the individual soldier, and on the perfection of the machinery by which he is led and brought into action at the proper time; and, therefore, if the analogy holds good, there can be only two kinds of diseases, the one dependent on abnormal states of the physiological units, the other on perturbation of their co-ordinating and alimentative machinery. Hence, the establishment of the cell theory in normal biology was swiftly followed by a 'cellular pathology' as its logical counterpart. I need not remind you how great an instrument of investigation this doctrine has proved in the hands of the man of genius, to whom its development is due, and who would probably be the last to forget that abnormal conditions of the co-ordinative and distributive machinery of the body are no less important factors of disease. Henceforward, as it appears to me, the connection of medicine with the biological sciences is clearly defined. Pure pathology is that branch of biology which defines the particular perturbation of cell-life, or of the co-ordinating machinery, or of both, on which the phenomena of disease depend.

"Those who are conversant with the present state of biology will hardly hesitate to admit that the conception of the life of one of the higher animals as the summation of the lives of a cell-aggregate, brought into harmonious action by a co-ordinative machinery formed by some of these cells, constitutes a permanent acquisition of physiological science. But the last form of the battle between the animistic and the physical views of life is seen in the contention whether the physical analysis of vital phenomena can be carried beyond this point or not.

"There are some to whom living protoplasm is a substance even such as Harvey conceived the blood to be, *summâ cum providentia et intellectu in finem certum agens, quasi ratiocinio quodam;* and who look, with as little favor as Bichat did, upon any attempt to apply the principles and the methods of physics and chemistry to the investigation of the vital processes of growth, metabolism, and contractility. They stand upon the ancient ways; only, in accordance with that progress toward democracy which a great political writer has declared to be the fatal characteristic of modern times, they substitute a republic formed by a few billion of 'animulæ' for the monarchy of the all-pervading 'anima.' Others, on the contrary, supported by a robust faith in the universal applicability of the principles laid down by Descartes, and seeing that the actions called 'vital' are, so far as we have any means of knowing, nothing but changes of place of particles of matter, look to molecular physics to achieve the analysis of the living protoplasm itself into a molecular mechanism. If there is any truth in the received doctrine of physics, that contrast between living and inert matter, on which Bichat lays so much stress, does not exist. In nature nothing is at rest, nothing is amorphous; the simplest particle of that which men in their blindness are pleased to call 'brute matter' is a vast aggregate of molecular mechanisms, performing complicated movements of immense rapidity, and sensitively adjusting themselves to every change in the surrounding world. Living matter differs from other matter in degree and not in kind; the microcosm repeats the macrocosm; and one chain of causation connects the nebulous original of suns and planetary systems with the protoplasmic foundation of life and organization. From this point of view pathology is the analogue of the theory of perturbations in astronomy; and therapeutics resolves itself into the discovery of the means by which a system of forces competent to eliminate any given perturbation may be introduced into the economy. And as pathology bases itself upon normal physiology, so therapeutics rests upon pharmacology, which is, strictly speaking, a part of the great biological topic of the influence of conditions on the living organism, and has no scientific foundation apart from physiology.

"It appears to me that there is no more hopeful indication of the progress of medicine toward the ideal of Descartes than is to be derived from a comparison of the state of pharmacology at the present day with that which existed forty years ago. If we consider the knowledge positively acquired in this short time of the *modus operandi* of urari, of atropia, of physostigmin, of veratria, of casca, of strychnia, of bromide of potassium, of phosphorus, there can surely be no ground for doubting that, sooner or later, the pharmacologist will supply the physician with the means of affecting, in any desired sense, the functions of any physiological element of the body. It will, in short, become possible to introduce into the economy a molecular mechanism which, like a very cunningly contrived torpedo, shall find its way to some particular group of living elements, and cause an explosion among them, leaving the rest untouched. The search for the explanation of diseased states in modified cell-life; the discovery of the important part played by parasitic organisms in the etiology of disease; the elucidation of the action of medicaments by the methods and the data of experimental physiology—appear to me to be the greatest steps which have ever been made toward the establishment of medicine on a scientific basis. I need hardly say they could not have been made except for the advance of normal biology.

"There can be no question, then, as to the nature or the value of the connection between medicine and the biological sciences. There can be no doubt that the future of pathology and of therapeutics, and therefore that of practical medicine, depend upon the extent to which those who occupy themselves with these subjects are trained in the methods, and impregnated with the fundamental truths, of biology.

"And, in conclusion, I venture to suggest that the collective sagacity of this Congress could occupy itself with no more important question than with this. How is medical education to be arranged, so that, without entangling the student in those details of the systematist which are valueless to him, he may be enabled to obtain a firm grasp of the great truths respecting animal and vegetable life, without which, notwithstanding all the progress of scientific medicine, he will still find himself an empiric?"

NOTES ON EXPERIMENTAL CHEMISTRY.*

By Professor Albert B. Prescott.

I. Determinations of the limits of (1), temperature in solution; (2), temperature in dry state; (3), alcoholic fermentation; and (4), acidity, compatible with the starch converting power of diastase of barley malt.

II. Determinations of the solubility of precipitated aluminium hydrate in excess of ammonium hydrate, with and without ammonium chloride.

In a paper by M. L. Boudenoot in the *Nouvelles Annales de la Construction*, describing the various forms of explosives of the nitro-cellulose class, a new compound is mentioned, called by its inventor, M. Anders, gelatino-diaspon. It is composed of wood-cellulose and nitro-glycerine, is unaffected by cold, is not sensible to blows or shocks, and explodes only by a sudden increase of temperature to about 160° C. (320° Fahr.) It burns quietly when ignited in the open air, and is not injured by water.

* Read before the A. A. A. S., Cincinnati, 1881.

THE PARIS ELECTRICAL EXHIBITION.

[FROM OUR PARIS CORRESPONDENT.]

To the Editor of "SCIENCE."

This letter leaves Paris somewhat late, considering the official opening of the Electrical Exhibition took place eight days ago, and that the opening to the public followed the next day, viz., the 11th of August, but in fact the exhibition is not opened even yet, although the public is admitted during some hours of the day to look at the half-finished structures and to inspect the dust-covered instruments.

The daily newspapers and some so-called scientific papers, which give to their readers sensational articles rather than correct information, have been for about ten days crowded with descriptions of the opening and the progress of the electrical exhibition, but the real good scientific papers have hitherto only given short notes, because it has, as yet, been impossible to study the value of the different instruments in the exhibition building, where everything is still in a half-finished state and where the noise of hammers and carpenters' instruments are still heard in every corner.

Notwithstanding this, I will endeavor to give you in this letter a description of the actual state of the exhibition, which will serve your readers as an introduction to the more special articles with which I will furnish your paper weekly.

When we first enter the Palais de l'Industrie through the principal pavilion, which is situated on the side of the Champs Elysées, we observe a series of beautiful statues which serve as "candelabres" for lamps of the Werdermann system, and when we approach the entrance to the great nave our eyes are attracted with two enormous images representing a male and female lion, while we observe above our head a beautiful chandelier of iron wrought in tasteful style, furnished with Siemens lamps. This lustre will undoubtedly be very attractive if the arrangements for the light are made as carefully by the French firm of the well-known house of Siemens, as those in the German department, where some evenings ago the preliminary experiments made with the Siemens lamps attracted the general admiration of all those who had the privilege to witness them.

In the centre of the nave a light-house is erected, which is a copy of the light-houses that guard the coasts of France. It is surrounded by a small water-basin, which, although it may be called ornamental, is perfectly useless for the purpose for which it is destined, on account of its limited dimensions and the outlines of its borders, which form a star. This basin is intended as a field of exercise for the boat of M. Trouvé, called the *Telephon*, which is driven by an electric motor, in connection with a Bunsen battery, and the length of which nearly equals the radius of the circumference of the basin.

I may here say a few words about M. Trouvé's boat, on account of which a good deal of nonsense has been published in European and American papers, one of the latter mentioning not long ago that M. Trouvé's boat, with which he experimented upon the Seine, contained a battery of M. Faure, but M. Trouvé is too well acquainted with the value of scientific instruments to depreciate the merits of the Planté battery and to substitute for it Faure's modification, as long as the former is better.

Count Du Moncel, whose name is well known among all electricians, on account of his excellent work on the "Application of Electricity," which is the most complete work of its kind in existence, and also on account of his other numerous publications and inventions relating to this part of Science, presented on the 7th of July last a note to the Academy of Sciences, in which M. Trouvé describes in a very precise manner the motor used by him in propelling a little boat. This note will give to your readers exact and correct information regarding the merits and properties of the motor used in the little canoe which is now seen in the Electrical Exhibition, and I therefore quote this note verbatim:

"A motor having a weight of 5 kilogrammes and in connection with six elements of a secondary battery of Planté, which produces a labor of 7 kilogrammeters per second, was placed on the 8th of last April upon a tricycle, which latter, rider and battery included, had a weight of 160 kilogrammes, and gave to the vehicle a celerity of 12 kilometers per hour."

"The same motor, used on the 26th of May, in a boat having a length of 5.50 meters and a breadth of 1.20 meters, holding three persons, gave to this boat a celerity of 2.50 meters in descending the Seine at Pont-Royale and of 1.50 meter in moving against the current. The motor obtained its electro-motive power by means of two batteries, consisting each of 6 elements of bichromate of potash, and the propeller was furnished with a coil having 3 branches.

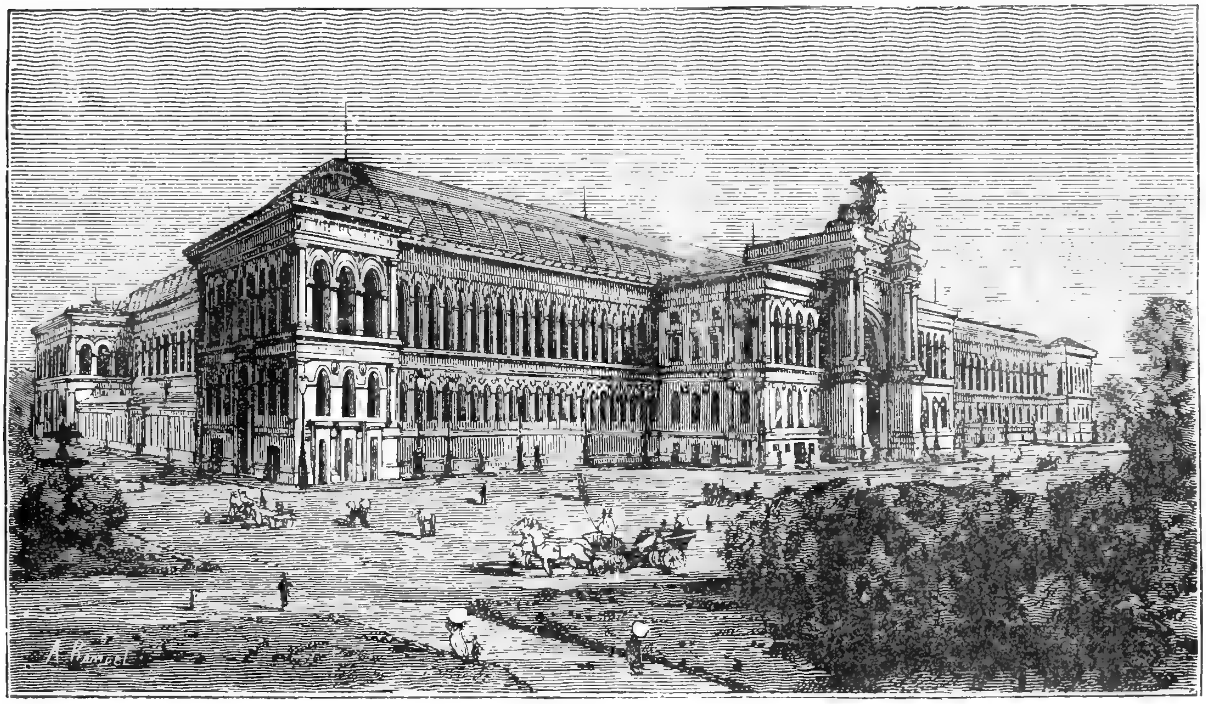
"On the 26th of June I renewed the experiment upon the quiet waters of the upper lake of the Bois de Bologne, using a coil with 4 branches having diameters of 0.28 meter and being in connection with 12 elements of Bunsen with flat plates such as are used in the Ruhmkorff battery. The liquid of these elements consisted of one part of hydrochloric-acid, one part of nitric acid, and two parts of water in the porous vessel, in order to diminish the disengagement of hypoazotic vapors.

"The celerity of the little boat, which was measured with an ordinary log, rose in the commencement to 150 meters within 48 seconds, or a little more than 3 meters per second; but after three hours of working it had diminished to 150 meters during 55 seconds. After five hours of working the electricity was still 2.30 meters per second."

So much about M. Trouvé's boat, of which a number of miniature specimens, in good working order, may be seen in the upper story of the Exposition building.

At the left-hand side of the nave, nearest to the light-house, are the exhibitions of Great Britain, Germany and the United States. The exhibition of Germany is that which has the most imposing appearance and is also that which was first completed. Two enormous "candelabres" in forged iron ornament the entrance of the department, and contain lamps of "Gebrueder Siemens" of Berlin. Near them stand two trophies crowned with the Prussian eagle, and behind them, upon a large number of tables, may be seen a collection of electrical instruments of all kinds, which we will describe in our reports hereafter. At the right-hand side of the department we see the busts of five German pioneers in the field of Electrical Science, viz.: Otto von Guericke, Ohm, Sömmering, Steinheil, and Gauss.

The historical collection of instruments in the German department is of the highest interest in a retrospective way. I will only mention an exact copy of the first machine for static electricity, constructed in the year 1670 by Otto von Guericke, consisting of a sulphur globe, which was electrified by turning it by means of an axis and using the hand as a rubber; an electrical egg, so-called, property of Prince Pless, of Germany, and constructed at the commencement of the 18th century; an electro-chemical apparatus for telegraphing, constructed by Thomas Sömmering in Munich in the year 1809—the telegraphing with this instrument is done by the decomposition of water. A magneto-electric telegraph of Gauss and Weber, constructed in 1833—this telegraph was used in 1837 in order to keep up a telegraphic communication between the physical laboratory aud the magnetical observatory in the University of Göttingen; the copy of the first telephone ever constructed, and invented in the year 1861 by Reis, and a great many other apparatus of equal interest.



EXTERIOR.

INTERIOR.

ELECTRICAL EXHIBITION.—PARIS, 1881.

Amongst the other electrical instruments in the front part of the department, especially worthy of attention, are some of the most important instruments used in thé physiological institution of Berlin and mostly due to the genius of the celebrated German professor "Dubois-Reymond" of Berlin. These instruments will be used in experiments before the French Academy of Sciences and the Electric Congress, by Professor Arthur Christianï, of the Medical Faculty of the Berlin University, who has been sent for this purpose to Paris and arrived here a few days ago.

A large space in the German department is occupied by the extensive exhibitions of the firm of Siemens & Halske of Berlin, and there may be also seen the first electro-locomotive constructed by this firm in the year 1867 and the first machine of that renowned type, which was invented at nearlv the same time by Mr. Wheatstone in England and Mr. Siemens in Berlin.

The exhibit of Mr. Siemens is, in my opinion, one of the most important of all the exhibits in the whole building of the Palais de l' Industrie, and the excellent instruments and apparatus manufactured by this firm, which occupy a large place in the German, French and English departments of the exhibition create general admiration. I shall endeavor, during the following week, to furnish you with an explicit description of some of the more important instruments of the Siemens exhibition and your readers will then be able to judge for themselves of their value.

The exhibition of *Great Britain* is still far from being complete, and this is very easily explained, by the fact that the English government, although after a very long hesitation, finally having taking part in the exhibition has not consented to contribute anything for the expenses, and it is marvellous that under such circumstances the English exhibitors have been able to contribute so much as they have. The only portion of the English exposition which is nearly complete is that of Siemens Brothers, who, among other things, exhibit a great number of apparatus for submarine telegraphy which are displaved on several large tables, and of which the most conspicuous is a full sized buoy ornamented with a flag. One fact which I must not forget to mention, is, that the pavilion of the British post-office is undoubtedly erected in excellent taste and is not only the most conspicuous but also the most beautiful in the exhibition (with the exception of the pavilions of the Italians). This pavilion is divided into two parts, the one containing a historical collection which is very remarkable and consists of the first instruments of telegraphy, among others that ingenious telegraph apparatus of Cook and Wheatstone, which was used with several wires and which now, after the invention of duplex, quadruplex and multiplex systems seems rather primitive, while the other part consists of a collection of all the modern instruments of telegraphy which can only be appreciated by a more minute description, and a great help to the study of these instruments is their excellent arrangement, which is due to the labors of Mr. Preece.

The exhibition of the United States is not at all what it should be, and it is greatly to be regretted that Europeans will receive a very wrong impression of the productiveness of your country if they judge by the scanty exhibits which America has sent to the Paris exhibition.

Most of the Americans to whom I have spoken seem to recognize this fact very fully, and it is generally regretted that many of the beautiful electrical inventions of the United States cannot be displayed here, where they would certainly create a sensation. But it seems that the United States Government was too interested in politics to care for a worthy representation at the electrical exhibition, and the more intelligent class of the French public know how to appreciate the difficulties with which the American exhibitors had to contend on this account, as well as on account of the great distance which separates their country from Europe.

Among the apparatus, which are already installed, may be mentioned the Bell telephone, the automatic time-register and alarm, the Dolbear telephone, of which you have already given a long description in your paper, and which is now exhibited in a neat little pavilion erected by Mr. Buck and Mr. Stetson, who, by their industry, set an example to the tardy French workmen; and the exhibition of the United States Signal Service, which contains an ingeniously constructed distance-barometer, anemoscope and anemometer, invented by Mr. Eccard. The display of the other exhibitors are not yet finished, and I reserve that of Mr. Edison's until it is more complete, as it promises to be the most interesting and valuable one in the building, and will demand a special report to do it justice.

If we continue our walk through the Exhibition towards the East Entrance, we come to the department of Belgium and Austria. That of Belgium offers a very beautiful aspect, and is displayed in two fine pavilions, which are furnished with several crystal cases ornamented by copper posts. The galvano-plastic exhibits in this department are worthy of the greatest attention, but scientifically, the most interesting exhibit is that of the meteorological station of Brussels. Austria has contributed a great many apparatus which serve for the security of railways.

The pavilion of Italy, which we next enter, is a beautiful oblong building, and attracts much attention. Until yesterday it was nearly empty, but the instruments begin now to be installed, and within a few days the visitors of the Exhibition will have the privilege of seeing the ingenious instruments that Volta, Gulvani and Nobili constructed with their own hands, and to read the original letters in which these great scientists published their first ideas regarding their new and wonderful discoveries. This Italian pavilion, in connection with the post office pavilion of England and the retrospective collection of the Germans, form together the material for the three most important chapters in the history of electricity.

The exhibition of Holland offers, so to say, an appendix to them, and the instruments for static electricity there shown, excel, perhaps not in quality, but at least in grandeur all other instruments of this kind. The enormous machine and battery of Leyden-bottles of Van Marum, of which we have all heard when we were school boys, form the most interesting part of this exhibition, and the whole, including the enormous natural magnets, makes upon the visitor an impression that he is visiting an exhibition of the Scientists of the land of the pyramids.

Passing the departments of Spain and Switzerland, and leaving at the left the exhibition of Russia and Norway, which have all contributed in an appreciable manner to the interesting show of apparatus, we arrive at the entrance station of the Siemens' electrical railway which is not yet completed on account of several modifications which his construction had to undergo. It will be running, however, within a few days.

Returning now again to the centre of the nave, and entering the western half of the building, we see before us the French portion of the exhibition.

At the left hand side viewed from the light-house, is a pavilion in very good taste filled with the shining silver and gold exhibits, and with the highly artistic galvano-plastic reproductions of the renowned firm of Christofle & Co., and on the right hand side from the light-house in a pavilion corresponding exactly in style with the former, we see the exhibits of the Jablochkoff Electric Light Company. This department contains a complete collection of all the different kinds of apparatus used by this company, and amongst others a new dynamo-electric machine of Jablochkoff, which is of excellent consttron.

The pavilion of the *City of Paris* contains instruments for the distribution of time and electrical instruments for the service of the fire companies. This pavilion is surrounded by the exhibitions of different French railroad lines, which contain an enormous amount of apparatus too complicated and too numerous to mention in this short review.

One of the most interesting parts of the French exhibition is the pavilion of the Ministry of Posts and Telegraphs, which contains a complete collection of all the modern apparatus employed in the telegraph service of France. This pavilion is bounded on its North, South and East sides by highly interesting collections of different French firms, while on its West side the great staircase leads to the upper stories. Of the exhibits in the upper story I will give only a general catalogue because the installments are as yet too unfinished to render it possible to give any detailed description of them, and the experiments with the electric lights and telephones, to which this portion of the palace is mostly dedicated, will not commence before eight days.

Hall A, immediately opposite to the grand staircase, is a beautifully furnished drawing room called the "Salon du President" and will be lighted by the Werdermann light.

Hall 1 contains a gallery of paintings but it is to be hoped that the light of the "Lampe-Soleil" which is here exhibited will be better than the pictures, which are wretchedly bad.

Hall 2 contains a stage which once figured in the, so-called, "Athenæum," in the "Rue des Martyrs." This stage will be used for showing stage effects produced by electric lights, and the light will be furnished by the Werdermann Company.

Hall 3 is a tastefully furnished dining-room, with table temptingly set, in which the Werdermann light will also be displayed.

Hall 4 is an apartment consisting of vestibule, kitchen and bathing-room, which will be lighted by incandescent lamps fed from reservoirs consisting of Faure's secondary battery and furnished by "La Société de la Force, et la Lumière."

I have made it a special object to study the value of the Faure-battery in regard to which so much has been said pro and contra, and propose to furnish your excellent paper with impartial reports on this subject as soon as any definite knowledge of it can be obtained.

Halls 5 and 6, which are united in one, will display lights of the "Systeme-Jamin" and contain a collection of Gramme-machines modified by M. Jamin.

Hall B contains a collection of smaller electrical apparatus, of electrical toys and also an exhibition of Jablochkoff candles.

Halls 7 and 8 are dedicated to telephone experiments, hall 7, being lighted by "La Société de la Force et la Lumière" while the light of hall 8, is furnished by Mr. Brush. The preliminary experiments with the telephones in these halls have been exceedingly satisfactory, the music of the Grand Opera and the words spoken in the "Théatre-Francais" (both of these buildings being connected by telephone-wires with halls) can be so plainly heard that one may really imagine himself to be one of the audience present, instead of being several kilometers distant from the places of performance. A person, who has never witnessed these telephone experiments can have no idea of the value of the microphone and telephone, and the public, before which these experiments will be made in about eight days, will be greatly astonished to see those reports verified which it has hitherto taken for exaggerated descriptions of sanguine writers.

Hall 9 contains chiefly electrical apparatus devoted to medical purposes, and will be lighted by Méritens, who also has there exhibited the most of his special apparatus.

Hall 10 is dedicated to the exhibition and the light of the firm of "Sautter et Lemonnier."

Hall 11 has Jablochkoff light and will also exhibit the apparatus used for photographing by electric light.

Hall 12 will be lighted by the Spanish society of electricity which employs Gramme's lamps.

Hall 13 serves for the display of Siemens' differential lamps and contains an excellent collection of instruments of precision and of Geissler tubes.

Hall 14 contains machines of the system Wilde and Alliance, it will be lighted by means of Wilde's candles, furnished by the Parisian Company of Wilde's light.

Hall C contains cables, telephones, and telegraph instruments and will be lighted by incandescent lamps of Maxim furnished by the United States Electric Lighting Company.

Hall 15 has, among other things, a nice collection of lightning-rods and contains Jaspar's light.

Hall 16 has lamps of M. Anatole Gérard.

Hall 17 contains electro-chemical instruments, apparatus for galvano-plastics, etc., and lamps of the Gramme system.

Hall 18 contains a highly interesting museum of historical instruments of electricity. The light is furnished by Messrs. Mignon and Rouart.

Hall 19 will be lighted by a company from Lyons, displaying the processes of Lontin, Bertin and Mersanne, and also contains the electro-pneumatic clocks of Mr. Mayrhofer, which form one of the most interesting parts of the electrical exhibition.

Hall 20 contains a retrospective museum and a library of works on electricity; the light in the former will be furnished by Mr. James Fyfe, that in the latter by Mr. Daft.

Hall 21 serves as a restaurant and is ornamented by a large chandelier containing Swan's incandescent lamps.

Hall 22 serves as a reading-room and will be lighted by the Brush system.

Hall D is the place where the Congress will meet, and halls 23 and 24 contain the exhibition of Edison, of which I shall not now speak in detail, reserving a description for a special letter, when I will attempt to do justice to this interesting exhibit. GUSTAVE GLASER, *Ph. D.*

PARIS, *August* 17, 1881.

THE AMERICAN CHEMICAL SOCIETY.

The first meeting of the American Chemical Society, after the summer vacation, was held on Friday evening, September 2, with Vice-President Leeds in the chair. The minutes of the previous meeting were duly passed on and Dr. H. Endemann elected to the position of Editor of the Journal. The first paper presented to the Society was "On the Detection of Oleomargarine," by Mr. P. Casamajor. This method is based on the differences between the density of butter and oleomargarine. A drop of the suspected fat is melted and poured into alcohol at 15°C; if it is butter, on account of its greater specific gravity, it immediately sinks to the bottom of the vessel, while if it is oleomargarine it remains on the surface.

Mr. Casamajor followed by a second paper on the "Detection of Sugar House Syrups from Starch Sugar Syrups." The author found that by dissolving the given sample (10c.c. are taken) in three times its volume of methylic alcohol, the ordinary sugar syrup will become entirely dissolved, while the starch sugar syrup becomes precipitated under the same conditions. Partially dissolving indicates, of course, a mixture of both.

"A Short Table for Testing Sugar by Inversion" was the title of the third paper. It was also by Mr. Casamajor. Assuming that $D =$ the first deviation in a reading of a polariscope and $- D'$ the second, substracting them we have $D + D'$.

$t =$ the temperature.

When a solution of pure sugar is 100, the sum of the two readings will equal 144. Making Δ equal to the

true deviation, we have from the above constants $\Delta = D + D' \times \frac{100}{144 - t}$.

In the table given by Mr. Casamajor the quantities which t was equal to was given, so that by a simple calculation it became readily possible to determine the value of the true deviation. The table was based on the much larger one of Clergets.

The fourth paper of the evening was by Mr. A. H. Elliot and it consisted of a description of "A New Form of Apparatus for the Analyses of Gases." It was very severely criticised by Dr. Endeman as being decidedly inferior to the more complicated forms devised by Professor Hempel. M. B.

THE SUCCESSFUL ADMINISTRATION OF NITROUS OXIDE AS AN ANÆSTHETIC FOR DENTAL AND SURGICAL OPERATIONS.*

DR. E. P. HOWLAND, Washington, D. C.

The successful administration of nitrous oxide consists in administering it to patients in such a manner that during operations they will not suffer pain, and that they will be in such a condition that the dentist and surgeon can successfully perform the operation and afterwards that the patients are found not to be injured by its administration. The first requisite for success is that the nitrous oxide should not have more than one per cent of pure oxygen or three per cent of atmospheric air, and that it should be perfectly free from all other gases or vapors. Nitrous oxide with two per cent or more of pure oxygen or five per cent or more of atmospheric air, will not produce perfect anæsthesia and the patient will feel the pain of the operation and pronounce the gas a failure. The adding of one per cent of pure oxygen to nitrous oxide has the benefit of partially oxygenating the blood and in a measure preventing the spasmodic action of the muscles and at the same time produce satisfactory anæsthesia. According to experiments made in France by P. Bert, ten per cent of oxygen or fifty per cent of atmospheric air can be added to nitrous oxide to oxygenate the blood, and at the same time produce perfect anæsthesia if it is breathed in a chamber under a pressure of two atmospheres. A certain amount of nitrous oxide taken into the lungs is necessary to produce insensibility, and it can be diluted with any innocuous gas and still produce anæsthesia, provided this amount is inhaled in the given time. Under pressure in a chamber more gas is breathed in a given time, as the nitrous oxide is condensed the same as the air in the chamber and under a pressure of two atmospheres, two volumes of nitrous oxide would be condensed into one volume, so that the nitrous oxide could be diluted with equal measures of atmospheric air and still the quantity of nitrous oxide inhaled would be the same as if breathed ordinarily and the quantity of oxygen breathed sufficient to arterialize the blood. Rapid breathing of nitrous oxide produces quick anæsthesia, but nothing is gained by it in practice. It is very difficult to produce anæsthesia with nitrous oxide at high elevations above the ocean, because the low pressure of the atmosphere allows the gas to expand so that a less quantity is taken into the lungs in a given time than is required to produce insensibility. Valve inhalers have generally proved a failure, because they admit atmospheric air with the gas in sufficient quantity to to prevent perfect anæsthesia. As near as I can ascertain, more than one-half of all the dentists of the United States who have used nitrous oxide have abandoned its use on account of want of success in producing satisfactory insensibility and thereby injuring instead of benefiting their practice. One cause of failure is the unskillful administration of the gas in allowing air to be inhaled with it, by not having the lips closed tight around the inhaler, and other causes; not using the nose as a valve for expiration exactly at the right time; not stopping the administration at the point of greatest anæsthesia and not having sufficient self-possession under all circumstances and emergencies to know just what to do and when to do it. But the greatest cause is the failure of producing perfect anæsthesia from the mixture of atmospheric air in the nitrous oxide that has been kept in a gasometer over water for a few days. The gas becomes mixed with air through the medium of the water and defective gasometers and cocks. The trouble and cost of making fresh gas every few days has caused the great abandonment of its use. Skillful administrators, who have a large practice and make fresh gas before deteriorated by air, are making nitrous oxide a success. Other dentists can make gas a success by obtaining it condensed in cylinders, when the gas will keep unadulterated and unchanged for years. The only drawback to a paying success is the present great cost of the condensed gas, which in the small cylinders amounts to about thirty-five cents for each administration, when the gas can be made in the dentists' laboratory for about three and a half cents for each administration. An apparatus can now be obtained that enables each dentist to make and condense his own gas and keep it for any length of time. Physicians and surgeons do not use nitrous oxide on account of the trouble and cost of making and keeping it, and the greater amount of practice and skill required in its successful administration than with the more dangerous ether and chloroform. Nitrous oxide requires a costly apparatus to manufacture it, and bulky receptacles to hold and administer it from, and the gas is for sale in but two places in the United States, while ether and chloroform can be carried in a bottle in the pocket and purchased at every drug store in the land. Nitrous oxide can be administered with almost absolute safety, while ether and chloroform can point to their victims in every city and hospital. Money, labor and skill can make nitrous oxide successful with both dentist and surgeon, and taking into account the value of human life, nitrous oxide should stand at the head of all anæsthetics, and its practical use be encouraged instead of ether and chloroform.

I have administered nitrous oxide in over thirty thousand cases for dental and surgical operations, and have had uniform success. I have never had a case of injury from lung or heart disease, but in many cases of throat and lung diseases a marked and permanent improvement. I have kept a large number of patients perfectly anæsthetic for surgical operations from five to thirty-five minutes, and the pulse during these operations has been nearly uniform and full. The success of prolonged operations consists in first producing perfect anæsthesia and then breathing air to arterialize the blood and before consciousness returns again breathing nitrous oxide, the necessary intervals varying in different patients from one-fourth to one-half minute. The average length of time occupied in dental operations from the first commencement of breathing the gas till return of consciousness has been two minutes. To encourage and make nitrous oxide a greater success in the future, the dental and medical colleges should employ successful operators to lecture and instruct graduates so that the particular knowledge and skill acquired by them in practice can be learned by others.

* Read before the A. A. A. S., Cincinnati, 1881.

ON October 17 next, fifty years will have elapsed since Prof. Bunsen, the eminent chemist, received his doctor's diploma from Göttingen University. He, however, intends to absent himself from Heidelberg on the day in question, in order to avoid all congratulations and speech-making.

MR. W. H. M. CHRISTIE, F. R. S., First Assistant at Greenwich Observatory, has been appointed Astronomer Royal, in succession to Sir George Airy, who retires after holding the office for nearly half-a-century.

SOME NEEDED REFORMS IN THE USE OF BOTANICAL TERMS.*

By CHARLES E. RIDLER, M.A., Master of High School, Kingston, Mass.

I.

Seventy per cent of 700 examined species and varieties of "flowering plants," and 65 per cent of all the "flowerless plants," as given in Mann's Catalogue, have different names; 3646 "flowering plants" and 178 "flowerless" are given in the list. If to these per centages, the names of the genera and orders be added, there will be a total of more than 4000 different ones to be remembered, east of the Mississippi; and if collections are made elsewhere, the number becomes appalling. Only 14 names are used five times or more, and over 50 per cent are used but once; that is, among the flowering plants every other name is new, and among the flowerless two out of every three are new.

Many of the specific names describe the plants as being "like" some other plant or thing, and both Latin and Greek terms are employed to do this. Thus, over a hundred different specific names were found ending in *folium* or *phyllon* (leaf), and *oides* (like)[1] Among some other things noted are the following: Adjectives are frequently used in their different degrees of comparison without any meaning whatever; there is a great diversity in the use of proper names of persons, countries and States; specific words are frequently found differing only in their endings and not in their roots; one English word is often described by several Latin, with only a slight difference in meaning, and the question is whether one word might not be used in place of several given in a set[2]; Greek and Latin names exist with the same meaning; Greek and Latin terms are used to describe the same plant; double specific names, and similar specific and generic terms are common; occasionally a term is employed which denotes a specific difference far more common than it is used; and many compound and coined words of doubtful authority[3] are scattered throughout the list—in all of which there is a great need of reform. The plan is suggested, at least in this country, and especially for use in the school-room, of having in the study of botany nothing but English words for the English-speaking race. If Greek and Latin, however, are to be retained, they should be kept in their purity. These reforms in the use of botanical nomenclature are urged for the great mass of tired students of both sexes, and their teachers, in the United States, rather than for the eminent botanists and horticulturists, who may remonstrate against any change which will rob the science of its choicest literature.

THE *Révue Industrielle*, in a recent number, gives a curious instance of the spontaneous galvanization of an engine piston, which took place at Cette, Hérault. The boiler having become much encrusted, some scraps of zinc were introduced to loosen the coating. Several days afterwards, the piston began to work with difficulty; when it was taken out, it was found to be covered with a thick coating of copper. This is supposed to have occurred from the particles of zinc carried with the steam into the copper steam-pipes forming a number of minute galvanic elements in combination with the copper; the vibration of the piston then attracted the copper molecules to itself, whilst the heat and the electric properties of the steam are considered to have facilitated their attachment to it.

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* Read before the A. A. A. S., Cincinnati, 1881.

[1] With *folium: Alismae, apii, alni, bellidi, delphini-ilici. myrti, parnassi, primulæ, rosmarini*, etc., etc.; with *phyllon: tricho, argo, chryso, lepto, rhizo, lepido*, etc., etc.; with *oides: anemon-lunarin, scirp, hesperid, cheiranth, melilot*, etc., etc.

[2] Such as, *Vulgaris, officinalis, vulgata, media, communis* (common); *sylvestris, nemorosa, sylvatica* and the like.

[3] The paper gave a long list of words used by botanists which cannot be found in the lexicon, such as *grandiflora*, and other compounds of *flos; arabisans, advensis, cucullaria, variolaris, cataria, asprellum, lateri—folia* and other compounds of *folia; salina, atro-purpurea*, and others.

BOOKS RECEIVED.

THE ANCIENT BRONZE IMPLEMENTS, WEAPONS AND ORNAMENTS OF GREAT BRITAIN AND IRELAND, by JOHN EVANS, D. C. L., LL. D , F. R. S., &c. D. Appleton and Company, 1, 3, and 5 Bond street, New York, 1881.

As Dr. Evans admits, the period covered by the Bronze age cannot be fixed within a precise limit, especially for any particular country. Through the successive stages of civilization, when the Stone period gave way to that of the bronze period, and was succeeded by the Iron, a long course of years must have passed, and even in any particular district the change could not have been sudden.

There must, therefore, have been a time when in each district the new phase of civilization was introduced, and the old conditions had not been changed; the three stages of progress represented by the Stone, Bronze and Iron periods, like the three principal colors of the rainbow, overlapping and intermingling one with the other, through their succession.

In discussing the chronological position of the bronze-using period, the possible use of copper unalloyed with tin, cannot be overlooked; in fact the probability that native copper may have continued for a lengthened period before it was discovered that the addition of a small portion of tin rendered it not only more fusible but added to its elasticity and hardness, must be apparent to all. While dwelling on this point Dr. Evans points out that even after the advantages of the alloy over the purer metal were known, the local scarcity may at times have caused so small a quantity of that metal to be employed, that the resulting mixture could hardly be recognized as bronze; or at times the dearth may have necessitated the use of copper alone, either native or as smelted from the ore.

Of this Copper Age, however, but feeble traces are to be found in Europe, if, indeed, any can be said to exist, but in India important discoveries have been made of copper instruments; these, however, were accompanied with ornaments of silver, which appeared to mitigate against their extreme antiquity, as the production of silver involves a considerable amount of metallurgical skill, and probably an acquaintance with lead and other metals.

The most instructive instance of a Copper Age, as distinct from one of Bronze, is that which has been discovered in our own country, where we find good evidence of a period when, in addition to stone as a material from which tools and weapons were made, copper also, was employed, and used in its pure native condition without the addition of any alloy. The State of Wisconsin alone, has furnished upwards of a hundred axes, spear heads and knives formed of copper, and to judge from some extracts from the writings of the early travellers given by the Rev. E. F. Slafter, that part of America would seem to have entered on its Copper Age long before it was first brought into contact with European civilization, towards the middle of the sixteenth century. On some parts of the shores of Lake Superior native copper occurs in great abundance, and no doubt attracted the attention of the early occupants of the country, who undoubtedly availed themselves of its ductile property to produce spear-heads and other weapons.

To those who have supposed that iron, which is a simple substance and easily produced from its ores, may have been in use before copper; the author replies, that without denying the abstract possibility that in some parts of the globe such might have been the case, he considers that among the nations occupying the shores of the Mediterranean—a part of the world which may be regarded as the cradle of European civilization—not only are all archælogical discoveries in favor of the suc-

cession of iron to bronze, but even historical evidence supports their testimony.

The study of this subject necessarily involves an investigation relating to the date when man first became acquainted with the methods of working the various metals, and the reader will find in this work a carefully prepared synopsis of all the evidences bearing on their disputed points. The introductory chapter describing this controversy will be found one of the most interesting and instructive in the book.

The great body of the work is devoted to an examination and description of the various forms of Bronze weapons and instruments which have been found in the British Isles, treating separately the different classes of instruments, intended each for special purpose, and at the same time pointing out their analogies with instruments of the same character found in other parts of Europe. To bring this department within the comprehension of all readers, Dr. Evans has presented five hundred and fifty superb wood engravings of specimens; thus the archæologist who possesses this work, finds himself, as it were, passing through a museum of Bronze antiquities, aided by the friendly guidance of one who is a master of the subject, and capable of pointing out important details and characteristics, even in the most ordinary implements, which, to the cursory observation of a student, would appear devoid of meaning.

Dr. Evans concludes this interesting work with a chapter on the chronological arrangements of the various types of bronze, and an examination of the various means at our command for fixing the *approximate* date and duration of the period. On the latter point, after what we have stated on the subject, no surprise need be expressed when we state, that Dr. Evans offers an opinion only with great reserve. Subject to this reservation, we find that he attributes eight or ten centuries as the total duration of the Bronze Period, placing the beginning some 1200 or 1400 years before the Christian era. It is questionable whether such an antiquity will meet all the necessities of the case, for as Professor Evans himself points out, it is difficult to believe that the Phœnicians, or those who traded with them, landed in Britain and spontaneously discovered tin.

This work will prove to be of the highest value to archæologists and to all who would trace the course of human progress to its earliest phases. Its general arrangement is most excellent, and adapted for practical work. In addition to a general index, a geographical and topographical index is presented, which greatly adds to the value of the work. The publishers have performed their part of the work most efficiently, and have produced a handsome volume, illustrated in the highest style of the engravers' art, which will in future be held as an authoritative work of reference, and a store-house of facts from which the student and specialist may draw material of the highest value.

IT has been resolved to invite the British Association to meet in Aberdeen in 1883. The invitation will be presented at the forthcoming meeting of the Association at York. The Association will meet in Southampton in 1882, and an influential local committee has already been appointed.

THE Government of India has declined for the present to award the prize of £100 offered for the best "manual of hygiene" for the use of the British soldier.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING SEPT. 3, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

AUGUST AND SEPTEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 28	30.020	30.088	0 a. m.	29.990	4 p. m.	76.3	69.0	89	4 p. m.	75	4 p. m.	67	6 a. m.	64	7 a. m.	139.
Monday, 29	30.102	30.128	12 p. m.	30 032	0 a. m.	76.0	70.0	86	3 p. m.	74	5 p. m.	67	5 a. m.	66	5 a. m.	139.
Tuesday, 30	30.129	30.190	9 a. m.	30.086	5 p. m.	77.6	72.0	85	4 p. m.	75	4 p. m.	70	5 a. m.	68	5 a. m.	135.
Wednesday, 31	29.988	30.100	0 a. m.	29.900	12 p. m.	82.7	73.3	93	4 p. m.	78	4 p. m.	72	6 a. m.	68	6 a. m.	140.
Thursday, 1	29.823	29.900	0 a. m.	29.780	12 p. m.	81.0	72.6	87	1 p. m.	76	1 p. m.	75	6 a. m.	70	6 a. m.	131.
Friday, 2	29.786	29.800	9 p. m.	29.748	4 p. m.	73.3	69.3	78	3 p. m.	71	3 p. m.	68	12 p. m.	66	12 p. m.	97.
Saturday, 3	29.864	29.966	11 p. m.	29.800	0 a. m.	69.6	65.3	74	3 p. m.	68	3 p. m.	66	7 a. m.	63	7 a. m.	128.

Mean for the week.. 29.958 inches.
Maximum for the week at 9 a. m., August 30th .... 30.190 "
Minimum " at 4 p. m., Sept. 2d.................. 29.748 "
Range .... .442 "

Dry. Wet.
Mean for the week..................76.6 degrees .... 70.2 degrees.
Maximum for the week, at 4 pm. 31st 93. " at 4 pm 31st, 78.. "
Minimum " " 7 am. 3d 66. " at 7 am 3d, 63 "
Range " " ---- ---- 27. " .... 15. "

AUGUST AND SEPTEMBER	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast. 10 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0 10
Sunday, 28	w. s. w.	s.	e. s. e.	94	½	3.15 pm	.543	.623	.679	79	48	81	3 cir.	0	0	-----	-----	-----	--	0
Monday, 29	e. n. e.	s. s. e.	s. s. e.	102	1	2.30 pm	.622	.650	.693	85	54	85	0	1 cir.	5 cu.	-----	-----	-----	--	0
Tuesday, 30	s. s. e.	s. s. e.	s. s. w.	103	2¼	9.50 pm	.682	.746	.704	90	64	73	10	0	3 cir.	-----	-----	-----	--	0
Wednesday, 31	w. s w.	s. s. w.	s. w.	187	2¾	2.00 am	.631	.724	.746	80	48	64	3 cir.	0	0	-----	-----	-----	--	1
Thursday, 1	w.	w.	s. w.	179	1½	9.50 pm	.666	.705	.703	77	55	66	0	9 cu.	10	-----	-----	-----	--	3
Friday, 2	n. n. w.	e.	e.	114	1	1.40 am	.668	.678	.644	85	73	85	10	10	10	4.40pm	5.30pm	0.50	.05	0
Saturday, 3	n. n. e.	e.	s. e.	104	¾	4.20 pm	.536	.604	.564	84	72	79	8 cu.	4 cu.	8 cu.	-----	-----	-----	--	1

Distance traveled during the week.............................. 883 miles.
Maximum force.. 2¾ lbs.

Total amount of water for the week.............................. .05 inch.
Duration of rain.. 00 hours, 50 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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JOHN MICHELS, Editor.

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SATURDAY, SEPTEMBER 17, 1881.

In a recent Government publication, prepared by Professor F. W. Clarke, S. B., of Cincinnati, we find the following paragraph relating to the purchase of scientific apparatus, which may be studied with profit by the manufacturers:

"Some years ago Congress passed an act authorizing schools and colleges to import apparatus free of duty. This act is not so widely known among teachers as it ought to be, nor do those who know it fully realize the saving in expense which it implies. Goods bought of a local middleman cost their European price, plus a heavy duty and the expense of transportation, with a large profit to the dealer over and above the sum of the foregoing items. A school, by importing its apparatus directly, can save the duties and the local dealer's profit—a retrenchment of from forty to fifty per cent. A hundred dollars thus expended on a direct foreign order will buy as much material as a hundred and fifty laid out at home. A knowledge and an application of these facts will enable many a school to do far more in the way of laboratory work than is considered possible now. To be sure, it is desirable that home trade should be patronized, but not in such a way as to cripple science. The present duties bring in but a trifling revenue to the government and might be abolished without injury to any one. If this were done, our schools and colleges could afford to buy more goods of American dealers; the latter, with larger sales, could ask more reasonable profits; and so both buyer and seller would be benefited.

This paragraph once more revives a question which we trust will not be dismissed until some practical decision has been arrived at. Congress has abolished the duty on scientific apparatus and instruments, in the interest of colleges and other rich corporations, but, demands of the poor student, a tax of fifty per cent. upon every instrument purchased by him.

Such a discrimination in the collection of duties is neither just nor reasonable, and appears to have failed even in achieving any good results in the direction anticipated. On the contrary, it has crippled the business of the American manufacturers, and forced them to charge exorbitant prices on the limited sales they could make under such a system.

We fully concur in the suggestion made by Professor Clarke, that, as these duties bring in but a trifling revenue to the government, they might be abolished, and that without injury to anyone.

We are also glad to find Professor Clarke, while speaking as the representative of the class most benefited by this discriminative legislation, taking such a liberal view, and advocating its entire abolition.

We are unable to offer the view that "the trade" may take on a measure which will bring them in open competition with European manufacturers, possibly they may require to be educated to an apppreciation of a course, that will ultimately result in a condition of business, which will be beneficial to their best interest.

With the abolition of the discrimination in favor of colleges, etc., and of all duties on scientific instruments, the first result would be to equalize the prices of such manufactures, irrespective of the place where they are made. Universities and colleges in such a case could afford to buy of the domestic manufacturer and would doubtless do so. The one point that would have to be considered in such an open market, would be that of *quality*, and the American manufacturer of scientific apparatus has nothing to fear on that head, while with larger sales more reasonable profits could be accepted; thus both buyer and seller would be benefited. We trust that the next Congress will take some action in this matter, and place scientific apparatus and instruments on the free list of the tariff, and thus remove an obnoxious tax on knowledge, and increase the facilities for the acquisition of scientific and technical education among the masses of the people.

We are informed that Dr. T. Sterry Hunt, of Montreal, and Professor James Wall, sailed for Europe on the 10th instant, for the purpose of attending the International Geological Congress, to be held at Bologna, Italy, on the 26th of September. We have written to Dr. T. Sterry Hunt, who is both a subscriber and contributor to this journal, to send us a report of this meeting, and have no doubt that we shall be thus enabled to place before our readers a reliable account of the doings of this Congress.

We understand the Edison Light Company has been notified that the French Government, after inspecting all the electric lights in the Paris Electrical Exposition, has selected the Edison Company to light the Grand Opera-house of Paris with the Edison electric light. The Edison Company will ship the necessary electrical machinery to France by the next French steamer, and will light up 800 Edison electric lamps in the opera-house on Oct. 7.

HISTORIC NOTES OF COSMIC PHYSIOLOGY.*

By Dr. T. Sterry Hunt.

[Abstract.]

The author began by insisting that general physiology, as the philosophy of material nature, is co-extensive with general physiography, in which sense it was employed by the best writers up to the first year of this century. In the abridgements of the Philosophical Transactions of the Royal Society up to 1700, and to 1720, the chief division is into Mathematical and Physiological subjects, the latter including the phenomena of the three kingdoms of nature. There is a physiology not only of animals and plants, but of the inorganic world, and from terrestrial physiology we rise to a conception of the physiology of the Cosmos or material universe; a subject which from the earliest times has attracted the attention of philosophers. One of the most evident of the problems thus presented is that of interstellar space, and its relations to our earth and its gaseous envelope. After noticing the views of the ancient Greeks, the author referred to the discovery by Alhazen of the refraction of light, from the phenomena of which the Arab philosopher attempted to fix the limit of the terrestial atmosphere. He then noticed the similar attempts of later observers, and adverted to the well-known hypothesis of Wollaston, who endeavored to assign thereto an absolute limit on grounds which are inadmissible. He adverted to various views as to the so-called ether of space, which Newton thought, must include exhalations from celestial bodies, and noticed the hypothesis of Grove that the medium for the transmission of radiant energy through space is but a more attenuated form of the matter which constitutes the gaseous envelopes of the earth and other celestial bodies, between which, through this medium, Grove supposed material interchanges might take place. The suggestion of Arago as to the possibility of determining the density of the rare matter of interstellar space was noticed, as well as that of Sir William Thomson, who has even attempted to fix the minimum density of the luminiferous medium, which he, like Grove, conceives may be a rarified extension of the terrestrial atmosphere. Mattieu Williams, adopting the hypothesis of the atmospheric nature of the interstellary matter, has attempted to show how the sun in its course through space may condense this matter with the evolution of heat and thus replenish the solar fires. From this ether also by a stoichiogenic process the various chemical species are perhaps generated.

The author had endeavored to approach the study of interstellary matter from a wholly different side. From a consideration of the chemical and geological changes of which we have evidence in the earth's crust since the beginning of life on the planet, it is clear that enormous volumes of carbonic dioxide have become fixed partly in the form of carbon, with evolution of oxygen, and partly as carbonates—equal in the aggregate to 200 atmospheres or more. This enormous volume, it is held, must have come from outer space to supply the gradual absorption of the gas from the atmosphere, while by a reverse process of diffusion the great amount of liberated oxygen may have been got rid of, and the equilibrium of the atmosphere in this way maintained. The consequences, both meteorological and geological of this process were discussed by the author in 1878, and more fully in 1880 in an essay on The Chemical and Geological Relations of the Atmosphere in the *American Journal of Science.* As a farther contribution to the history of these views, the author proceeded to show that Sir Isaac Newton not only held to the presence in interstellar space of exhalations from the sun, the fixed stars, and the tails of comets, which he supposed to become diffused in and to form part of the ether, but even suggested that this etherial matter is the solar fuel and essential to planetary life. From a consideration of the processes of vegetable growth and decay, Newton arrived at the conclusion that elements from interstellar space, brought by gravity within the terrestrial atmosphere, serve to nourish vegetation, and by its decay are converted into solid substances. In this way are, according to him, generated not only combustible (sulphureous) bodies, but calcareous and other stones, whereby the mass of the planet is augmented. These views put forward in Newton's famous Hypothesis concerning Light and Color in 1675, and in the Queries to the *Optics*, are more definitely enunciated in Propositions 41 and 42 of Book III of the *Principia.*

* Read before the A. A. A. S., Cincinnati, 1881.

ON THE UNIFICATION OF GEOLOGICAL NOMENCLATURE.

By Richard Owen.

With a view to proposing such Geological Terminology as would probably be acceptable to a large majority of the scientific representatives of those nations sending delegates to the International Congress for the Unification of Geological Nomenclature, it seems necessary to offer for discussion some principles, and to lay down some

SUGGESTIVE RULES:

1. To agree that all questions shall be decided by a plurality vote; or, if thought best, by a two-third majority.

2. To assign distinctive names for the headings of geological divisions and subdivisions, instead of calling, for instance the "Silurian," sometimes an "Age," at others a "Period, System, Era, Formation," or as by the French "Etage," which is translated by Surenne as meaning (when applied to Geology) stratum or layer. Further suggestions on this point will be given in the "Conspectus of Headings."

3. To arrange under these heads, when thus decided upon, such formations as are generally considered of nearly coördinate value, in lieu of giving the same apparent importance to a minor subdivision, say of Upper Silurian (such as Salina), or one of the Devonian (e. g. Chemung) that we assign to the whole Tertiary. The subjoined Tabular View offers a modified coördination.

4. To select, as far as practicable, for the geological formations thus arranged, geographical terms, indicating the areas where these formations prevail extensively, or have been studied very thoroughly. This would obviate any controversy on mooted points regarding the lithological or paleontological character of the formation. In order to illustrate the practical application of this rule, let us take for examination the nomenclature proposed by the illustrious Sir R. Marchison, in his great work of 1854, "Siluria," descriptive of the geological formation in the country inhabited by the ancient "Silures." His work of 1839 was entitled "The Silurian System," but his later publication showed a preference for the shorter and more expressive form as a noun. The adjective has, with slight modifications, been adopted in most modern languages; but by selecting the noun "Siluria," we unify for universal recognition. The same may be said for "Devonia." If it is not considered too great an innovation to alter terms already so well received, we might say "Silur-Britannia," "Devon-Caledonia," and proceed then to distribute the honors among different nationalities, as more fully exhibited in the Conspectus. The term Carboniferous is not correct when applied to Mountain Limestone or Millstone Grit, besides Coal Measures cannot be so rendered into other modern languages as to make a suitable subdivision, it is therefore suggested to name the system after the region having the greatest Coal area (the United States), and the Coal Measures after a European country in which coal is well developed. This would give us Appalachia or Carbon-Appalachia for the system, and Belgia for

the productive Era, while the great development of Millstone Grit in Ireland furnishes the term "Hibernia," and the celebrated Adelsberg grotto or cave in Mountain Limestone suggests the term "Austria," as appropriate for that Era. So also Perm-Russia, because the Permian system prevails so extensively from Kasan to the Gulf of Tscheskaïa, near the White Sea; Trias-Germania, because the Germans have given to all geologists the subdivision of that system (Bunter, Muschelkalk and Keuper) also Jura-Gallia, because the system prevails largely, and has been studied minutely in France. This plan of nomenclature would also serve to recall to the geologist, and convey to the student, important facts regarding the distribution of the formation.

5. Somewhat in the same manner, uniformity might be given to the names of nonfossiliferous rocks, by adopting Dr. Dana's orthography, as given in his latest works, where he employs the letter "y" instead of "i" as in Granyte, to distinguish rocks from minerals with names ending in "ite." Even the final "e" may as well be omitted in order to unify for other languages; and we then have, for example, Granyt, Syenyt, etc., or we may even so apply the rule as to have international names for Limestone, Sandstone, Magnesian limestone and the like, calling them Calkyt, Silikyt, Dotomyt, etc. Some German scientists (see Cotta's "Gesteinslehre") use, for instance, the word Quartzit, writing it however with an "i."

6. It is thought further that, in many cases, geological terms might be abbreviated, so as to be readily intelligible in all languages, somewhat on the symbolic system adopted in chemistry. A conspectus of the proposed modifications in geological nomenclature, with a column

SUGGESTIVE CONSPECTUS, TOWARDS UNIFICATION OF GEOLOGICAL NOMENCLATURE.

EON.	PERIOD.	ERA.	EPOCH.	EQUIVALENTS.
Neozoic or Quaternary	XIII. Cimbria	33 Antillia or Gaudeloupia 32 Madagascaria	b Mentonia a Neandertha b Nile-Gange a Nora-Toxandria (or Auckland-Kawhia)-(New Zealand)	Recent: Modern (Neolithic, Paledithic); Reindeer
	XII. Patagonia	31 Scythia or Siberia 30 Pampas-Virginia	a Rhenus-Port Hudsonia (Loess) b Ebor-nora-Kentuckyia (Newburg, N. Y., and Big-bone Lick) a Darling—Australia	Champlain: Alluvium; Diluvium
	XI. Scandinavia	29 Suedia 28 Laboradoria	b Regio Grœnlandica a Arctico-Boria or Borea	Glacial or Drift
Cenozoic or Tertiary	X. Ter-Pliocene or Italia	27 Sicilia 26 Subapenninia	a Girgenti—Catania b Etruria—Apulia a Carolina—Virginia	Pliocene: Sumter (Dana) Aralo—Caspian of Lyell
	IX. Ter-Miocene or Græcia	25 Emodi—India (Himalaya) 24 Helvetia	b Sivalik—Pannonia a Cecropia—Sardinia (or Pikermi, or Pentelicus-Sar) b Caucaso—Tongria a Spitzbergen—Alaskia	Miocene: Yorktown
	VIII. Ter-Eocene or Afric-Asia	23 Parisia 22 Aegypto—Persia or Suessonia	b Alabama—Georgia a Berlin—Westphalia b Tyrol—Istria a Dalmatia—Syria	Eocene: Alabama; Lignitic Epoch
Mesozoic or Secondary	VII. Creta-Hispania	21 Anglo-Senonia 20 Lusitania 19 Neocomia	Mæstricht—Venetia Pyrenœi—Carpatesia Texas—Niobraria Dacia—Cimmeria Magellan—Circassia Wealden — Sussexia-Néocomien of d'Orbigny	Cretaceous: Up. Wh. Chalk; Lower do.; Upper Green sand; Lower do.
	VI. Jura-Gallia	18 Portlandia 17 Bavaria 16 Vindelicia	Calais—Bononia- (Boulogne sur mer) Solenkofen—Oxfordia Boll—Avonia	Jurassic: Wealden; Oolitic Epoch; Liassic do.
	V. Trias-Germania	15 Bolivia 14 Hanoveria 13 Saxonia	Cheshire—Somersetia Beaufort—Lunevillia (Dieynodon and Ceratites nodosus) Brunopolis—Polonia Brunswick loc. Eng. lilif. Connecticut—Moravia	Triassic: Keuper; Muschelkalk; Bunter
Paleozoic or Primary	IV. Perm—Russia	12 Kasan-Tscheskaïa	Thuringia Nosgesusia	Permian
	III. Carbonaria or Carbon-Appalachia	11 Carbonia or Belgia 10 Hibernia 9 —— Austria Subcarb 5 9° —— Alemania	Missouri—Pennsylvania Namur—Coimbria Michigan—Tasmania Monongolia (W. Virginia) Cumbria (Yorkshire Mt.) Adelsberg—Nova Scotia Iowaia: Kaskaskia, St. Louis, etc., etc., etc. Waverleyia (Ohio) Volga—Uralia	Carboniferous: Coal Measure; Millstone Grit; Subcarb 5 Limestone or Sandstone
	II. Devonia or Devon-Caledonia	8 Livonia or Caledonia 7 Rhen—Prussia 6 Neregonia (Norway)	Villmar—Catskillia Nassau—Westphalia or Franconia Plymouth—Eifelia Hamilton—Chemungia Cathay—Potoria Dalecartia	Devonian: Catskil; Chemung; Hamilton; Corniferou
	I. Siluria or Silur-Britannia	5 Scania 4 Niagaria 3 Boiohemia or Trentonia 2 Canadia 1 Cambria	Gottland—Tyrolia Helderberg—Ludlovia Medina—Clintonia Wenlock—Pentlandia Augers—Hudsonia Caradoc—Trentonia Vitré—Murcia Llandeils—Esthonia Potsdam—Longmyndia Acadia	Silurian: Upper: Helderberg; Niagara. Lower: Trenton; Canadian; Cambrian

DIFFERENT HEADINGS, WITH ONE OR TWO EXAMPLES.

MACROCHRONE.	EON.	PERIOD.	ERA.	EPOCH.	MICHROCHRONE.
(Greatly extended time.) *Examples.* Neptunia (all aqueous rocks.)	(" A space of time, a life-time.") Paleozoic Eon.	(" An inteıval of indefinite time.") Permia Carbonaria Devonia Siluria	(" A succession of years between two fixed points.") Austria (Mt. limest.) Cambria	(" A pause.") Iowaia Potsdamia Acadia	(Comparatively short time.) Karkaskia, St. Louis, etc., etc.

EXAMPLES SHOWING THE ADAPTABILITY OF CERTAIN HEADINGS TO MOST OF THE MODERN LANGUAGES.

		System. (" An assemblage of objects ranged in regular sub-ordination, or related by some common law.") Σύδεημχ (το) Le système. Das System, Il sistéma. El sistéma.	*Sub-system.*	*Group.* (" An assemblage of objects in a certain order.") Le Groupe. Die Grüggen. Il grúppo. El grúpo.	*Sub-group* or *Section.* Sectio. La section. Die section. La sezione. La seccion. or *grade* or *member*, with slight modifications can be used in the above languages.

ABBREVIATED FORMS WITH SOME EXAMPLES.

		For *Periods.* Roman numerals, I, II, III, etc., or Capital letters A B C, etc., applied thus: I = Silusia. or A = "	For *Eras.* Arabic numerals 1, 2, 3, etc., applied thus: 2. = Canadia 1. = Cambria	For *Epochs.* Small letters a, b, c, etc., [repeated for the epochs of each era.] 2^b = Vitrè-Murcia 2^a = Llandeils-Esthonia 1^b = Potsdamia 1^a = Acadia	For *Members,* marks used to the right and above the era letter, similar to the power-sign in mathematics. Thus to designate the Burlington member of the Iowa subcarniferous, we would write: III. $9^{a''}$ or C. $9^{\circ}a''$

of the leading present equivalents is submitted below, in which it will be observed that one great object, kept in view, was the recording particularly by the Epoch names, such localities as are noted for having given us remarkable fossils, characteristic of that peculiar formation, whether found in well-known regions of Europe and America, or in such distant countries as Patagonia, N. Zealand, the Cape of Good Hope, Greenland or Spitzburgen, etc.

NOTE TO TABLE I.

To further facilitate the understanding of some of the suggestions submitted, a tabular view is subjoined, giving different headings, with their definitions from standard dictionaries, as well as a conspectus of the symbols.

NOTE TO TABLE 2.

Probably some difficulties, and, despite of care exercised, some errors in the details may be pointed out; but if the general principles are found acceptable, or suggestive of such discussion as may ultimately lead to unification of our Geological Nomenclature, the object proposed, in the preparation of this paper, will be attained.

A NEW MATERIAL FOR STOP-COCKS AND STOPPERS FOR REAGENT BOTTLES.*

By H. W. WILEY.

For some time I have been working with a compound invented by Mr. T. J. Mayall, of Reading, Mass., and known as the Mayall metal. One form of this compound was intended as a material for journals, pneumatic tubes, etc. It is made of 5 to 6 parts graphite, 1 part rubber and ½ part sulphur. Instead of sulphur, sulphide of antimony can be used. The material is a perfect self-lubricant and to a high degree resists the action of acids and alkalis.

From its properties I was led to believe that it would be especially useful for chemical apparatus, in the manufacture of stop-cocks, connecting tubes, etc. My expectations were fully realized.

I have used it with success for burettes, cocks for hydro-sulphuric acid, stoppers for hydrote bottles, etc, These never stick, no difference how firmly they are pressed in nor how long they are left. The material is firm and elastic and will hold threads nearly as well as a metal.

I regard it as peculiarly useful for stop-cocks for acids, especially hydro-sulphuric. It is capable of a high polish, and will not tarnish. Slightly modified in composition it is used for covering houses and plating the bottoms of ships. Placed on ships it seems to prevent entirely the adhesion of barnacles, Strange as it may seem, it also makes an excellent insulating material for telegraph wires. I have not yet tried the effect of ozone upon it and only partially of permanganate of potassium.

* Read before the A. A. A. S., Cincinnati, 1881.

PHONETICS OF THE KAYOWE LANGUAGE.*

By ALBERT S. GATSCHET.

Books printed in Indian languages often render those tongues in a most imperfect manner, on account of the deficient knowledge of Indian phonetics on the part of the authors. The Kayowe language is a fair average specimen of Indian pronunciation, and is very rich in sounds, having no less than forty-four sounds, if we count in the long and the nasalized vowels. In its phonetic series the most conspicuous fact is the prevalence of the nasals and the total absence of dsh, tch, which are so conspicuously frequent in the majority of American languages, of r and of v. The palatal series is represented by one consonant only; the guttural and dental series are well represented, while in the labial series p, b, and m are the only frequent sounds. F is found in some words only, where it alternates with p, pai, or fai, land, earth. Among the sounds not frequently met with are sh, w.

* Read before the A. A. A. S., Cincinnati, 1881.

Nasalizing is a prominent feature in Kayowe phonetics, more so in the vocalic than in the consonantic series No word begins in l or w. Final syllables of words terminate just as often on a consonant as in a vowel, but all other syllables usually end in a clear or nasalized vowel. Every diphthong is adulterine; that is, every combination of two colliding vowels differing from each other can be pronounced as a monosyllable and a dissyllable. Thus we can pronounce as well ze-iba as zeiba arrow. The fact that every vowel can become nasalized (and many of the consonants also) is one of the curious features of the language. This nasalization is either the one observed in the French an, in, on, un, or it consists in the addition of an n to the vowel. All these Kayowe peculiarities are very commonly observed in the majority of American languages, and also in most of the unwritten languages of other parts of the world. The standard orthography which is adopted for recording a written literary language exercises undoubtedly some influence upon the pronunciation of the natives, but where the language is not fixed by writing, we perceive constant alternation of the sounds pronounced with the same vocal organ, as of the gutturals, dentals, and labials among themselves.

"This is also the case in Kayowe, and a full list of the sounds in it is as follows:

CONSONANTS:

Gutturals: k, g, kh (aspirate), h, ng.
Palatals: y.
Linguals: *k̲*, g̲, sh, l.
Dentals: t, d̄, s̄, z, n, nd, 'dl.
Labials: p, b, f, w, m, mb.
VOWELS: a, ā, â, ä, e, ē, ĕ, (the primitive vowel), i, ī, î, o, ō, u, ū, û."

TYPICAL THIN SECTIONS OF THE ROCKS OF THE CUPRIFEROUS SERIES IN MINNESOTA*.

By PROFESSOR N. H. WINCHELL.

This paper was in pursuance of the same line of investigation as that by the same author read last year before the Association, but gave the detailed methods by which general results had been attained in the study of the stratigraphy of the cupriferous rocks. By means of the microscopic examination of the crystalline rocks of the series, two groups of rocks were discovered, one being those generally accepted as igneous by Pumpelly, Chamberlin and by Owen, and the other the result of change from the sedimentaries. The former one dark colored and heavy, consisting essentially of labradorite, augite and magnetite, but the latter are lighter colored, generally showing a reddish tint, and consist essentially of orthoclase, quartz and hornblende. It is the latter group that in this connection possesses the greatest interest, as the author regards them as the true equivalents of the shales and sandstones that in some places are seen interbedded, without metamorphism, with the igneous rocks of the other group. They play a very important part in the geology of northeastern Minnesota, where, in their varied lithology, exhibiting different stages of crystallization, they not only are spread over a large geographical area, but afford some of the most interesting geological studies.

The author suggested that probably the titaniferous iron ore which is so largely associated with the igneous rocks of the cupriferous series, had its origin in the ferruginous shales of the sedimentary series, by the reduction of the oxides with which they are colored, at the time of the igneous disturbances.

The paper was accompanied by a series of fifty thin sections made by the author, with brief descriptions, and by samples of the rocks from which they were taken, intended to illustrate the lithological distinctions pointed out.

* Read before the A. A. A. S., Cincinnati, 1881.

WORKED SHELLS IN NEW ENGLAND SHELL-HEAPS.*

BY PROF. EDWARD S. MORSE.

Mr. Morse called attention to the fact that heretofore no worked shells had been discovered in the New England shell heaps. A similar absence of worked shells had been noticed in the Japanese shell heaps. Worked shells were not uncommon in the shell heaps of Florida and California. Mr. Morse then exhibited specimens of the large beach cockle (Lunatia), which showed unmistakable signs of having been worked. The work consisted in cutting out a portion of the outer whorl near the suture. To show that this portion could not be artificially broken he exhibited naturally broken shells of the same species, both recent and ancient, in which the fractures were entirely unlike the worked shells.

A REMARKABLE INSTANCE OF RETENTION OF HEAT BY THE EARTH.†

BY H. C. HOVEY.

The fact is well known that heat may be retained for a long period by the rocks and soils of the earth; but it is seldom that dates can be fixed with approximation to accuracy as can be done in the instance the particulars of which are now given.

My attention was called, a year ago, by Mr. James Hudson, manager of the Albion mines, in Pictou county, Nova Scotia, to a peculiar area including about two acres of ground, where the snow never lay long without melting, and the frost, even in severe winters, never penetrated but for a short distance. All over this space are scattered fused masses of clay and ironstone, resting on the outcrops of what are locally known as the "Main" and the "Deep" seams of bituminous coal, which at this point are about 450 feet apart and partially affecting the outcrops of other seams. On inquiry as to the probable date of the fire that had left this area of scoriæ and ashes, I was told that this portion of Nova Scotia was visited early in the seventeenth century by French explorers, and that an account of the harbor called Pictou was given in 1672 by Monsieur D'Enny, who was appointed Governor of the Gulf of St. Lawrence in 1654. The name "Pictou" is said to be derived from a Micmac word signifying *fire;* and the traditions of the Indians still point to this locality as having been, a long time ago, the scene of a fierce and long-continued fire, which made them avoid the place as being visited with the anger of the gods.

The coal measures of Pictou were discovered in 1798, at the very point now described; and the discoverers represented the spot as covered with ashes, over which grew large hemlock trees. About twenty years ago, while a drain was being dug in this locality, a tree was cut down that showed 230 rings of annual growth; and three feet below the root of this tree a large piece of wood, fashioned by some sort of axe, was found in a good state of preservation. It is Mr. Hudson's opinion that at least 300 years must have passed since the extinction of the fire at this point, and it is known that none has been rekindled since; its ignition may have been effected by chemical action, or by a stroke of lightning, or by artificial means applied to one of the so-called springs or feeders of inflammable gas that issue along the outcrops of these unusually thick seams.

Last spring it was found necessary to sink a small pit at the crop of the Deep seam on this area, in doing which

* Read before the A. A. A. S., Cincinnati, 1881.

† Read before the A. A. A. S., Cincinnati, 1881.

the facts were obtained concerning the long retention of heat by the earth, to which I have already referred. Mr. Edwin Gilpin, Government Inspector of Mines, has kindly placed at my disposal what information he could gather on the subject, which I give, using, to some extent, the language of this careful and accurate observer. Mr. Gilpin has prepared a comparative view of sections of the same strata, made only a short distance apart, the design being to exhibit the changes made by igneous action.

The *present section* is taken at the new pit sunk by the Albion Mines Company on the burnt area; and what is termed the *original section* is one given in Sir William Logan's Report of the Geological Survey of Canada, 1869, p. 69. The distance between the localities where these two sections were made is so small that the comparison is at least instructive, and answers our purpose as well as anything that can be had.

Present Section.	ft. in.	Original Section.	ft. in.
Surface of burned clay	22. 0	Black, argillaceous shale, with many bands of iron-stone 1 to 2 inches thick. Total thickness 144 ft. 6 in.	2. 6
Band of hard scoriæ	4. 0	Brown carbonaceous shale	1.10
		Bad coal	0. 2
Reddish ashes	3. 0	Good coal	3. 7
Hardened shale	2. 0	Black shale with iron-stone bands	1. 2
Good coal (being upper part of the Deep Seam)		Good coal	3. 5
		Coarse coal	0. 8
		Good coal	3. 9
Depth of Pit	32. +	Coarse coal	0.11
		Good coal	3. 4
		Coarse coal	5.10
		Total thickness of the Deep Seam.	22.10

The surface cover consists of clay, with boulders of sandstone and layers of gravel. The small portion of the 144 feet of black argillaceous shale filled with iron-stone balls, passed through by the shaft, has been converted into an almost continuous mass of scoriæ, very hard and compact, and difficult to drill through.

The next layer represents the upper portion of the deep seam, which has been completely burned away, leaving a *compact, laminated reddish ash*. And it was in this ancient bank of ashes, known to be more than 300 years old, that the retention of heat was observed, which it is now my object to place on record. Immediately on opening the pit, the heat of the ashes, at a point 30 feet below the surface, was tested by a reliable thermometer, and was found to be 80° Fahr. at a time when the surface temperature varied from a minimum of 45° to a maximum of 65° Fahr.

Soon after an opening had been made through the pit to the workings in the mine, the air-currents caused the temperature rapidly to fall to the normal point.

The consideration of the gradual radiation of the heat of the earth suggests the idea that abnormal increases in the temperatures of deep mines may be due in some cases to the presence, at comparatively short distances, of masses of heated matter, which are, geologically speaking, modern, though they may be historically ancient.

Recovery of Old Vulcanized Caoutchouc.—The pieces are heated in contact with steam, when the sulphur is volatilized and the caoutchouc melts and is collected as a liquid, used in preparing water-proof covers, etc.

Radiophony.—Professor Mugna, repeating M. Mercadier's experiments, in which an intermittent beam meets a smoked surface within a glass tube, containing aqueous or ammoniacal vapor, and furnished with an ear tube, adds to the effects by attaching a small microphone to an elastic membrane closing the tube. By this means he finds it possible to operate at a sufficient distance from the interrupting disc to render its noise no longer disturbing.

PILOCARPIN:—ITS ACTION IN CHANGING THE COLOR OF THE HUMAN HAIR.*

By D. W. Prentiss, M. D. Washington, D. C.

Pilocarpin is an alkoloid of Jaborandi and the active principle.

Jaborandi is a Brazilian drug recently introduced into medicine.

The leaves are the official part of the plant. (Pilocarpus Pennatifolius.)

The effect upon the human system is powerful and peculiar.

(It produces profuse sweating and salivation, and stimulates the growth of the hair.)

Two cases were reported.

In the first case, the medicine was given to relieve uraemia consequent upon suppression of urine due to *Chronic Pyelitis.*

The patient was a lady twenty-five years of age, a blonde of petite figure.

The pilocarpin (hydrochlorate) was administered by hypodermic injection, commencing December 16, 1880. and being continued at intervals until February 22, 1881. The usual dose given was one centigram, but on several occasions this dose was doubled.

The object of its use was to eliminate urea from the system by sweating and salivation.

The immediate effect produced was profuse sweating and salivation, calculated to amount to not less than fourteen pints. (See Phila. *Med. Times*, July 2, 1881.)

The result to the patient on each occasion was great exhaustion, but the ureamic symptoms were relieved.

Twenty-two "sweats" were administered in all, and from thirty-five to forty centigrams of *pilocarpin were used.*

CHANGES IN THE COLOR OF THE HAIR.

Specimens of the hair were exhibited to the section, as also a colored plate showing the changes in the color.

Two specimens dated respectively November 1879, and November, 1880, were of a very light color, tinged with yellow, and showed that the color of the hair had not changed during that year.

The third specimen dated January 12, 1881, was a chestnut brown, and the fourth dated May, 1881, almost pure black.

The administration of the Pilocarpin began December 16, 1880, the change was first noticed December 28. 1880, and was thenceforth progressive.

In addition to the change of color the hair has become thicker and coarser than formerly, and while previously dry, is now quite oily.

The hair on other parts of the body is also changed in color.

The eyes have become a much darker blue.

In the second case, the Pilocarpin was administered to an infant fourteen months of age, afflicted with Membraneous Croup. (See Phila. *Medical Times*, August 13, 1881.)

The treatment was commenced June 19, 1881; two milligrams of hydro-chlorate of Pilocarpin being given every hour, afterwards increased to four milligrams every hour. It was administered for nine days, the amount being diminished towards the last.

The first specimen of hair was taken June 17, 1881, and the second June 27, 1881.

The color of the first is light yellow, and the second is a decided shade darker. This effect, of changing the color of the hair, if subsequent experience shall confirm it, adds another to the marvellous influences of Jaborandi on the human system.

The *modus operandi* of the change is still to be determined. It is probably connected with the fact that Jaborandi stimulates the nutrition of the hair.

* Read before the A. A. A. S., Cincinnati, 1881.

There appears to be reason to believe that the color of the hair is due to an oily pigment, and that this is increased under the influence of Jaborandi.

Shaving the scalp usually has the effect of making the hair thicker and darker, on the contrary, as age advances and the processes of nutrition are enfeebled, the hair becomes thin and dry and whitens.

THE CONSTITUTION OF THE "ATOM" OF SCIENCE.*

By MRS. A. B. BLACKWELL, SOMERVILLE, N. J.

[Abstract.]

This paper developed the hypothesis that in each atom of matter a given quantity of force and extension are conditioned by each other to act in special modes, rigidly adjusted in time and space. All atoms react against many opposed and unlike forces simultaneously, hence each atom must be a highly complex (not compound), elastic structure, which, by its changes in space, gives the direction, extent, rate of vibration, and all modes and transformations of the atomic force.

We can explain this variety and change of action, if we suppose every atom to alternately expand and contract unlike filaments or poles that act and react in vibrations towards and from a common axis, which is at rest. No point outside this axis can be at rest, except when held in equilibrium by other atoms. Reaction is equal and opposite between every part of the atom, and between it and all other atoms. Chemical combination is the interlocking, the literal intertwisting of certain poles of the combining atoms. Such combination brings to rest, makes latent, the opposed combining poles, wholly or in part; the more completely this is done the greater the transformed motion called heat, and the more stable the compound.

In combining, the uniting poles are massed or knotted, as any intertwisting cords would be, and many-atomed molecules require no extra room for their vibrations; but all gases contain equal numbers of molecules to the volume. But the atomic axes are shifted to a common centre; and thus the vibrations of all the free poles are more or less modified, according to the number and kind of the combining factors; they are always so far modified that the molecules of any compound vapor cannot repel those of either of its constituents, nor those of any unlike vapor—the explanation being that the periods of greatest expansion, the stretch outwards in their free poles are not synchronous. In like molecules they are synchronous, and the free poles, striking at any point short of greatest expansion, drive the atoms asunder. We call them mutually repellant. The action of all repulsive forces will admit of similar explanation. Push or strain in one direction compels counter-push or strain in another direction; hence opposed electricities, magnetisms, and polarization in general.

Gravitation may be considered the concurrent result of brief intertwisting of the physical poles; cohesion and crystalogenic energy represent more permanent interlocking. But chemical and physical combination are supposed to be alike in kind—the result of opposed, adapted mechanical energy. Chemical action in general produces more radical changes in the sensible properties of substances, because, taking the initiative, it sifts the atomic axes, and subsequent combinations are but in accommodation to these previous changes.

The hypothesis attempts to give a fairly adequate explanation of material changes; of the *how* and *why* of such changes.

The unlike elements of matter are supposed to be conditioned in special groups, but are essentially of the same type, and their changes are all in time and space only. There is held to be a higher type of atoms in the living sentient, or "mind matter" group, which we know only through their active organisms. In these atoms, force is conditioned both by extension and by intensiveness, and not in time and space alone—as with simple matter, but in time and space and sentience.

Possible changes in sentience, emotion, may be nascent in these atoms just as complex motion is nascent in all uncombined or but little combined atoms. Complexity of action in molecule and larger mass against which any atom must react in equal measure and opposite directions, compels complexity in the atomic reactions, and in the higher type of atoms one phase of all these reactions represents changes in sentience-sensations, thoughts, volitions.

Molecular complexity sufficient to excite a pleasurable degree of feeling would tend instinctively to repeat itself; hence the rise of organisms. The organism is the sentient atoms everchanging active molecule; and organic growth is adapted to the more and more complex sentient states. Decadence means failure in such adjustments. Sentient changes vary all the way between the low sentient state of profound sleep and the most alert phase of self-consciousness, but they are all individual or atomic changes. This hypothesis claims to offer an explanation of the joint facts both of matter and of mind.

* From the A. A. A. S., Cincinnati, 1881.

BACTERIA AND THEIR RELATIONS TO PLANT CULTURE.

BY THOMAS TAYLOR, MICROSCOPIST, OF THE DEPARTMENT OF AGRICULTURE.

If we examine, under a high power of the microscope a small portion of the scum of a fermenting infusion of vegetable matter, numerous particles of a globular shape will be observed, measuring about one twenty-thousandth of an inch in diameter, uniform in size and shape, highly refractive and frequently found in gelatinous masses. These are known as micrococci, or spherical bacteria. Associated with them is generally found another description of germs of the same diameter, but of a rod-like shape, jointed and of various lengths. In common vegetable fermenting infusions they are seldom observed over .003 of an inch in length, and are frequently under .001 of an inch. They have generally an active motion, as seen under a high power (as have also the micrococci), and are known as rod-bacteria (from bacterion, a staff). Botanists of the present day assign both of these organisms to the division algæ.

Many investigators believe that certain species of these organisms produce contagious fevers, but there certainly are other species which perform a most useful part in the economy of nature, and in many of our valued industries their active co-operation is absolutely necessary. It is well-known that they are the chief agents of fermentation and putrefaction, and it is to the decomposing power they thus exert, in conjunction with the action of the elements, that all organic bodies decay and restore to the earth soluble fertilizing salts, instead of the insoluble and therefore unavailable material of which, in their unchanged state, they are made up. There is high authority for stating that organic substances are not inherently unstable. Under suitable conditions they may remain for an indefinite period wholly unaltered. It is well-known that in some portions of the earth the carcasses of dead animals tend to dry up and become mummified. In the arctic region the remains of animals imbedded in ice are kept in perfect preservation for centuries. It is only under conditions more or less favorable to the existence and multiplication of the small organisms which produce fermentation and putrefaction that rapid decay takes place.

Without bacterian fermentation the compost heap of

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the farmer would remain valueless as plant food. The stubble and the dead grasses of our fields, and the fallen leaves, twigs, branches and trunks of trees would remain comparatively unchanged but for the chemical action excited by the same agency. Fish guano, and all unfermented organic fertilizers, must undergo bacterian fermentation or putrefaction after their application to the soil, or they will remain in a stable form, and their ammonia, locked up in the tissues of which it forms a component part, will fail to yield its return of profit to the farmer. It is asserted that the great nitre beds of India owe their origin to the action of microscopic germs, and the production of nitrate of lime by artificial means presents a similar instance of the results of bacterian action. In this last-named operation animal and vegetable matter combined with lime is laid out in great beds and left for a period of two years, or until fermentation and putrefaction, coupled with the action of the air, have produced nitric acid, when nitrate of lime is formed, to be subsequently converted into nitrate of potash.

Some of the most beautiful colors used in dyeing are produced by subjecting lichens to bacterian fermentation, and the fermentation of stable refuse yields an even heat, which is extensively utilized in the manufacture of white carbonate of lead, as well as in the cultivation of mushrooms and of various early vegetables. The value of the edible fungi thus produced, alone amounts in Europe, Asia and America to millions of dollars per annum. The utilization of bacteria and similar organisms in the operations of baking and brewing, and the production of wine and vinegar, is familiar to every household.

While bacterian fermentation or putrefaction is an essential part of the process which fits dead organic matter to become food for plants, the former appears to be an incidental source of one of the common practical difficulties encountered by the farmer and horticulturist, viz.: the tendency of soils to become sour. Some of the lower forms of fungi are denominated "acid formers," and the mode in which these act will, I think, illustrate the process by which sourness of soil is brought about. If we dissolve a little sugar in water, add a small quantity of yeast fungus, and subject the solution to a suitable temperature, fermentation ensues—the sugar is converted into alcohol and carbonic acid, and in process of time the alcohol is oxidized, becoming acetic acid. As the result of some late observations, I am convinced that a similar change often takes place during the progress of those fermentations of which bacteria are the agents, and that these organisms, though in a less distinctive sense, might also be called "acid formers." So far as my observation extends, solutions in which bacterian ferments are in active progress, invariably become acidulated, and I have also found that soils in which bacteria and micrococci are revealed by microscopic examination—and I find them in all soils of average fertility—give perceptible acid reactions when tested by litmus paper.

That acidity is so often produced in excessive quantities may be due in part to the character of the unmarketable substances left upon the land in the operations of agriculture, such as the stalks of corn, the stubble of the smaller cereals, decayed grasses, the fallen leaves and twigs of fruit trees, and the roots of field and garden plants in general. In all of these there is a preponderance of cellulose, which substance is resolvable successively into starch, dextrine and glucose, and from this last, as from the solution of sugar in the experiment above referred to, is ultimately produced acetic acid.

The neutralization of the excess of acid in the soil is not the least of the ends subserved by the use of lime and other alkalies in agriculture; but another means which contributes to keep its quantity within wholesome limits is thorough drainage. If the soil of potted plants be not watered with sufficient frequency and copiousness it soon becomes sour, and gardeners have learned by experience to leave at the top of each flower-pot a water space of two inches, more or less, depending on the size of the pot. By filling this space with water as often as necessary the soil is kept sufficiently free from organic acids, which are washed out through the aperture below; and this is precisely similar to what takes place in any well-drained field.

I have already referred to the opinion that certain species of bacteria produce contagious fevers; but from what has been said above, it will be sufficiently apparent that this is by no means the chief function of this class of organisms. However great their baneful activity at times may be, their services to man and to organized existence in general are infinitely greater. Moreover, the former is but occasional and sporadic, while the latter is practically constant and universal. If the materials once used by the life principle in building up organic bodies could not be used over and over again for the same purpose, life must soon cease through the exhaustion of all that is capable of sustaining it. It is in that which has lived, but lives no longer, that life finds the greater part of its sustenance; but, as we have already seen, that vegetable life upon which all animal life ultimately depends can not use this sustenance in the form in which life left it. Before organic matter is available as plant food, it must be reduced almost to its primitive elements; and, as has been pointed out, its reduction is mainly effected through those processes of fermentation and putrefaction, in which bacteria appear to be the most active and important agents. Thus we find among those simple forms of life, which are supposed to have been the first to make their appearance on our planet, and to which, if we accept the theory of evolution, even the most complex of existing organisms owe their origin—an agent which, from the very inception of life upon the earth, has continuously performed a function without which the successive generations of plants and animals could not have existed; and stupendous as is its work, it is an agent so minute that twenty million individuals of its class might be inclosed within a globe small enough to pass through the eye of a cambric needle.

ANCIENT JAPANESE BRONZE BELLS.*

By Prof. Edward S. Morse.

Mr. Morse described the so-called Japanese Bronze Bells which are dug up in Japan. These bells had been described and figured by Prof. Monroe in the Proceedings of the New York Academy of Sciences. Mr. Kanda, an eminent Japanese archæologist, had questioned their being bells from their peculiar structure.

Mr. Morse had seen a number of bells of different kinds, some of considerable antiquity, but none of them approached these so-called bronze bells. Mr. Kanda had suggested that they were the ornaments which were formerly hung from the corners of pagoda roofs, but the fact that none of them showed signs of wear at the point of support, rendered this supposition untenable. Mr. John Robinson, of Salem, the author of a work on Ferns, had given the first suggestion as to the possible use of these objects. He has asked why they may not have been covers to incense burners. Curiously enongh, old incense burners are dug up which have the same oval shape that a section of the bell shows. The bell has openings at the base and also at the sides and top, so that the smoke of burning incense might escape. It is quite evident that these objects are neither bells nor pagoda ornaments, and this suggestion of Mr. Robinson's may possibly lead to some clue regarding their origin.

Electric Motive Power for Omnibuses.—The Faure accumulators have been tried again by the Paris omnibus company on a tramway with a carriage arranged for the purpose. The experiment is said to have been highly successful.

*Read before the A. A. A. S., Cincinnati, 1881.

TIME SERVICE OF CARLETON COLLEGE OBSERVATORY, AT NORTHFIELD, MINNESOTA.*

WILLIAM W. PAYNE.

The observatory of Carleton College is located at Northfield, Minn., forty miles south of St. Paul, on one of the main lines of the Chicago, Milwaukee and St. Paul Railway. It was built in 1878. Its latitude was determined by Professor B. F. Thomas in 1879, by a series of observations made with a Würdemann zenith telescope of two-inch aperture loaned to the Observatory for that purpose by Lieut. Edward Maguier, Chief Engineer of the Department of Dakota. He used the Talcott method and found the latitude to be 44° 27′ 41″±. In August, 1880, the work was done a second time by myself, using the same instrument and method, and observing forty pairs of stars from Sofford's catalogue on three different nights. After the proper reductions the latitude was found to be 44° 27′ 40.″8.

In October, 1880, by the aid and courtesy of the officer just named, and Lieut. O. B. Wheeler, of the Lake Survey Corps, the longitude of the observatory was determined. The Coast Survey meridian of St. Paul was used as the base of operation. Observations were taken at both points on two different nights and telegraphic signals were exchanged. Independent reduction of the observations showed the longitude of the Observatory to be $1^h\ 4^m\ 23^s.85$ west of Washington and 14.3 seconds west of the meridian of St. Paul.

INSTRUMENTS.

The Observatory is furnished with the following instruments:

A Clark Equatorial, 8¼-inch aperture, 10½ feet, with complete mounting.

A Byrne Equatorial, 4.3-inch aperture, with portable mounting.

A Transit made by Fauth & Co., Washington; telescope of 3-inch aperture and 42-inch focal length with reversing apparatus.

Two Howard clocks with electric and magnetic attachments for use in regulating and sending time.

A Bond Siderial Chronometer with break-circuit and an ordinary Clark Chronograph.

TIME SERVICE.

The time service of the Observatory began October 23, 1878, immediately after the clock was set and regulated, the N. W. Telegraph Company (now Western Union) having previously asked for time, and having built a line to the Observatory and furnished it with a telegraph office.

The electrical time-signals are given by the mean time clock which has a break-circuit attachment operated by a small wheel on the shaft carrying the seconds hand. This wheel, which contains thirty-one teeth, spaced to represent two seconds except three which give continuous seconds to mark the close of each minute. This clock is placed in a local circuit with appliances for cutting it into the main telegraph lines for daily, noon signals. By arrangement with the railroad companies the clock is put into line before twelve daily and thus give *three* full minute signals, the last stroke of the third minute being the time of twelve exactly.

Until recently the distribution of the time has been effected in the following manner:

The principal officers of five of the seven different railroads centering in St. Paul and Minneapolis were connected with the main office of the Chicago, Milwaukee, and St. Paul Railway either directly or at some intersecting point, and in this way our central mean time clock has daily operated all the main lines of these companies. The branch lines use the same time, having it repeated by hand. When the main lines are thus connected the clock has given its break-circuit signal distinctly over 1285 of wire in six different States and territories and ranging from Kansas City to St. Paul, Winona and McGregor in Iowa.

For a few weeks recently, the signal has been modified by reversing the points of the relay in the local circuit for the purpose of a make circuit signal on the main line. A five minute signal attachment has also been applied to the clock that time balls may be dropped at noon daily in connection with our railroad time service. Arrangements are already made to drop a time ball in each of the cities of St. Paul and Minneapolis, apparatus for the same being already in hand.

The five-minute attachment, as it is called, that aids in dropping these time-balls, is a plain disk attached to the train of the clock so as to revolve once in five minutes; a portion of the circumference representing fourteen seconds is cut away. This disk is placed in the local circuit and serves to keep it closed, and hence main lines open during fourteen seconds preceding the *sixtieth one* before noon. The interval gives opportunity to connect time-balls with electrical apparatus for dropping the same by the single twelve o'clock stroke from the clock. The dropping apparatus that I use for these balls is manufactured by Prof. H. S. Pritchett, of St. Louis. It is neat, simple and effective.

DISTRIBUTION OF THE TIME.

The following railroad companies take the Northfield meridian time directly or indirectly, and use it over their lines without change.

	Miles.
1. C. M.and St. P. R'y, on its five divisions West of the Mississippi now embracing an aggregate length of....................................	2271
2. W. & St. Peter R'y, (branch of N. W. R'y,) uses both Northfield and Barabvo signals but runs on Northfield time West of the Missisippi........	484
3. St. P. M. & O. from Sioux city to Elroy Wis., on all its branches...........................	963
4. M. & St. L. R'y, from Minneapolis South....	260
5. Northern Pacific Railway to the end of its track....................................	680
6. St. P. M. & M. certainly to St. Vincent and (I think to Winnepeg)...........................	630
7. St. P. & Duluth..........................	153
Making a total of..........................	5541

The last two companies named do not take time directly from the observatory but from jewelers in the city of St. Paul who receive our daily signals.

It will be seen readily by inspecting a map that the territory traversed by these great railroads embraces all of Minnesota and forts of Iowa, Nebraska, Dakota, Wisconsin, Montana, and probably the Province of Manitoba.

CHANGES IN MYA AND LUNATIA SINCE THE DEPOSITION OF THE NEW ENGLAND SHELL HEAPS.*

BY PROF. EDWARD S. MORSE.

This communication embraced a comparison between the shells peculiar to the ancient deposits made by the Indians along the coast of New England, and similar species living on the coast at the present time. Mr. Morse referred to similar comparisons which he had made in Japan, wherein he had found marked changes to have taken place; changes which showed that the proportions of the shells had greatly altered.

He had made a large number of measurements of shells from a few shell heaps of Maine and Massachusetts, and had obtained very interesting results. The common

*Read before the A. A. A. S., Cincinnati, 1881.

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clam (Mya) from the shell heaps of Goose Island, Maine; Ipswich, Mass., and Marblehead, Mass., in comparison with recent forms of the same species collected in the immediate vicinity of these ancient deposits, showed that the ancient specimens were higher in comparison with their length than the recent specimens.

A comparison of the common beach cockle (Lunatia) from the shell heaps of Marblehead, Mass., showed that the present form had a more depressed spire than the recent forms living on the shore to-day, and this variation was in accordance with observations he had made on similar species in Japan.

AMERICAN COAL FIELDS.

The areas of the anthracite coal fields, confined to a few counties of our State, are so well defined that we need be in no doubt as to their extent; and this limited area admonishes us that we should carefully husband our inheritance, and not waste it. The fact is well established, that for every ton shipped to market, two are wasted. The loss in the operations of mining, the pillars left to support the roofs of the mines, the loss in prepaiation, each contributes to this great aggregate. How to prevent these losses, by use of improved machinery, and by more thorough methods of working the mines, should be the study of our mining superintendents and engineers. Several suggestions with a view to a partial remedy, present themselves.

First.—The owning of the land by the operators would make them careful to mine all the coals. As tenants for a limited term of years, their object is merely to take out that coal, and in such a manner as will cost them little, and bring them much.

Second.—If the lands are to be leased, the term should be long enough to enable them to mine all the coal beds covered by the lease.

Third.—The lease should contain clauses subjecting the methods of mining, ventilation and drainage to the supervision of the owner's mining engineers; limiting the lengths of "breasts" to seventy yards or less; forbidding the use of monkey rolls, or the rebreaking of the coal; providing for the dumping in separate heaps of the coal dirt and the slate and rock.

Fourth.—We need larger collieries, and fewer of them, with perfected machinery, for hoisting, pumping and breaking.

Fifth.—More capital is required to open the mines for extensive and exhaustive working, by driving the gangways to the extreme ends of the territory, and then mining towards the outlet, so as to obviate the necessity of retracing our steps and robbing the pillars.

In Schuylkill county we are specialists. We are dependent upon one substance: coal is king. There is no gold, silver, lead, copper, or other valuable metals. Though we have good iron ores, they are so disseminated as not to furnish us one workable bed. Yet we largely help Pennsylvania to furnish nearly halt the iron manufactured in the United States. We have a large farming area well cultivated by our industrious and frugal German farmers. Our convenient location to the great markets of the Atlantic seaboard, our canals and abundant railroad facilities, our great commodity, always give a promise and an attitude among the great countries of our grand old commonwealth, which we are ever proud to realize.—*Geology of Schuylkill County, by P. W. Scheafer. Pottsville, Pa.*

The latest addition to microscope stands is the swinging sub-stage. This American invention has been adopted by most of the English manufacturers. In the last number of the *Journal of the R. M. S.* we find the value of the swinging sub-stages disputed by Mr. Crouch, and that Mr. Stevenson concurred in this view, and described them as useless incumbrances and unsuitable for use with certain apparatus, which is essential to the display of some objects.

ASTRONOMY.

COMET C (SCHÄBERLE), 1881.

This comet has been observed here since the 16th of July. When first seen it was large, round and bright, and slightly condensed at the centre, being very plainly visible in a 1¼-inch telescope. On the morning of the 19th it had increased sensibly in brightness; a faint tail could be traced for a distance of fully 15′, pointing in a northwesterly direction; on the above date its position was obtainee from θ (*Theta*) *Aurigas* in the following manner: Thd comet and star were separated too far to be both seen in the field of the telescope together, the comet was also too far north of the star for both objects to be seen at once in the finder. One of the wires in the finder eye-piece was made parallel with the meridian, and then the star, which preceded the comet, was brought into the field and its passage of the wire obtained; the telescope was then carefully moved northward in declination until the comet, entered the field when its passage of the wire was observed; in this manner the difference of R. A. was obtained; the difference of declination was then estimated. From a mean of several passages of the star and comet its position on July 18th at 15h. 40m., Nashville mean time, was found to be R. A. 5h. 52m. 52sec., and Decl. 40° 15′. The R. A. will be very little out, but the declination may be over a minute in error.

Its position was obtained in the same manner on the 20th (A. M.), using the same star at 3h. 35m., R. A. 5h. 53m. 54sec., Decl. + 40° 42′, with probably several minutes of error in the declination. On July 24, at 15 hours, the comet was visible to the naked eye, appearing about as bright as a sixth magnitude star (Prof. Swift, of the Warner Observatory, saw it with the unaided eye as early as the morning of the 23d).

On the 28th a small star-like nucleus was visible with the telescope.

Aug. 3 (A. M.), it was very easily visible with the naked eye, traces of the tail being seen without a telescope. A naked eye comparison with comet B showed C to be the brighter. Comparing it with a six magnitude star it was of the same brightness, but, covering a larger area, it was more noticeable than the star. The tail, in the telescope, was long and slender and straight as a shaft.

Aug. 4 (A. M.), the comet was quite conspicuous with the unaided eye, the tail stretching out for some distance. In the telescope the nucleus was small, round and pale, and star-like in form, Turning the telescope from comet C to comet B, the two were identical in brightness, but B was slightly broader about the head and tail, and the nucleus was not so distinct; but considering the low altitude of C it must have been really much brighter than B.

On August 14 it was visible in the evening after sunset, being quite plainly visible to the naked eye, with its tail streaming upwards for several degrees. In the telescope it was many times brighter than comet B.

21 inst., in the evening, the comet was as bright to the eye as a 3½ mag. star. It appeared very graceful, straight and slender in the telescope. On this occasion I obtained its position with the aid of a ring micrometer, referring the comet to *Psi ursæ minoris.*

1881, August 21 ds., 14.1m. Washington, m. t. $\left\{ \begin{array}{l} \alpha = 11h.\ 08m.\ 08.5s. \\ \delta = +45^\circ\ 13'\ 42'' \end{array} \right.$
This was the *apparent position.*

22 inst., evening, its tail could be traced with the telescope for a distance of about 6°, and was visible to the naked eye for about the same distance. A faint lightish stripe was visible on this date, extending from near the head to a degree or so along the middle of the tail. The following side of the comet's head and tail were distinctly defined, the sky appearing quite dark up to the very body of the comet, but the preceding side was ill-defined and blended, the sky being whitish for some distance from the comet; there also appeared to be a diffused sort of short tail running out some 10′ or so from the n. p. side

of the head ; the nucleus was small and not very well defined.

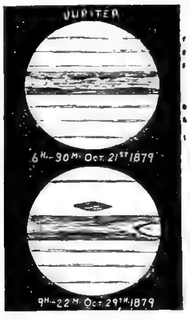
This comet differs considerably in general appearance from the comet now in *ursæ minor*. The head of B was large and broad, and its tail spread out greatly. Comet C has a small, narrow head with a very long slender shaft-like tail running from it in a straight line.

E. E. BARNARD.

NASHVILLE, TENN., *August* 26.

JUPITER.

The following cut represents the planet Jupiter on October 21st and October 29th, 1879, as seen with the 18½ inch Chicago refractor, with power 638.



The numbers on the right indicate the faint belts, which were systematically arranged on either side of the planet's equator.

The great Equatorial Belt, crossing the center of the disc, was composed of two separate belts, being divided by an irregular rift extending through the central portion. The color of this belt was reddish-brown-brick color, and the total width was 15,780 miles.

The great red spot shown in the center of the disc, on October 29th, was essentially of the same color as the equatorial belt, only more brilliant; it was about 30,000 miles in length and 8000 in breadth. Under fair atmospheric conditions, the equatorial belt was always visible up to the edge of the disc, with very slight diminution of color.

CORRESPONDENCE.

COMET *b*., 1881.

HARVARD COLLEGE OBSERVATORY,
CAMBRIDGE, U. S. *September* 13, 1881.

To the Editor of "SCIENCE."

SIR:—The spectrum of comet *b*, 1881, according to Dr. Konkoly (*Observatory*, 53, p. 257) contains five bright bands. From the mean of measures made with different spectroscopes on different nights, their wave-lengths in millionths of a millimetre were found to be 560, 545, 515, 472 and 468. The first, third and fourth of these bands are evidently due to carbon and, as Dr. Vogel has shown, are coincident with those of the banded stars of Secchi's fourth type. The other two bands appear to coincide with those of Ll. 13412. Last winter the spectrum of this star was found to consist mainly of bands having wave-lengths 545, 486 and 466 (*Nature*, xxiii, 604). The line 486 is probably due to hydrogen. The singular kinship of comets and banded stars is thus confirmed by a star whose spectrum seems to be quite unique.

EDWARD C. PICKERING.

To the Editor of "SCIENCE."

About two weeks ago, I found that one of the turtles which I keep for experimental purposes, a *Chrysemys picta* had laid eggs; all but one of these had been devoured whether by the turtle itself (as I have known to be the case with the same species, when kept in captivity) or by some alligators living in the same tank I could not discover. The perfect egg, I imbedded in moist sand, after carefully washing it, and finding yesterday, that it had not undergone development, I opened it and to my surprise found a living maggot, the larva probably of the *Musca vomitoria*, creeping around actively in the space between the half dessicated yolk and the shell membrane. It measured about four millimeters in length. As it crawled out of the aperture in the shell which I had made I threw the specimen away as it did not show the original anomoly.

Analogous observations have been made in the chick's egg. Cases are not infrequent where one egg has enclosed another or even several eggs, legs of beetles, wisps of straw and other foreign bodies. But this is I believe the first case where a living animal has been found in an egg. Of course the explanation of its presence is the same as in the case of the other substances referred to.

E. C. SPITZKA.

BOOKS RECEIVED.

ELEMENTS OF ALGEBRA, by G. A. WENTWORTH, A. M., PROFESSOR OF MATHEMATICS IN PHILLIPS EXETER ACADEMY, 8° BOSTON. Ginn & Heath, 1881; viii, 380 pp.

This addition to American algebraic literature is the sort of book that is to be expected from a live teacher. It bears the stamp of experience upon it and gives evidence throughout of the one end and aim of teaching beginners in algebra the art of algebraic manipulation. We say the art rather than the science, because the aim is clearly to familiarize the pupil with the *art*, to teach him *how* to manipulate rather than to lay stress upon the reasons for the processes, the author being evidently a disciple of Thomas Hill in his belief "*Facts before reasoning*." This is shown by such statements as "From these it may be assumed, etc."; "It may be verified that, etc."

The author has paid "particular attention to brevity and perspicuity in definitions," a thing which cannot be too highly commended, and without which any algebra, however good in other respects, will not succeed.

This matter of definitions is, as every teacher understands, a very important matter, if not for the algebra itself, then at least as a matter of right training and clear thinking. Definitions should be memorized, but memorization is not enough; they must be thoroughly understood. With those teachers who do not agree with this view we will have no disagreement, for the student trained to thoroughly comprehend is generally found by that very process to have secured that definition in his memory. In a text book, therefore, which aims at clearness and brevity in definition, a valuable training is afforded the student by leading him to carefully weigh the definitions; to consider whether the definition can be curtailed without loss of clearness, or whether it be not already too brief to be intelligible; to consider whether it is too restricted or too extended in its application, etc.

With the view of emphasizing this important matter we shall call attention to some of the definitions in this book, and at the outset let us premise that the definitions of mathematical terms must conform to the usage of mathematicians. It is a well-known fact that certain features of text books, faults as well as excellencies, are faithfully reproduced. Witness the statement concerning the rotation period of one of the major planets, erroneously given in one of the earlier editions of "*Herschell's Outlines*," and this error faithfully copied into astrono-

mical text books for nearly half a century. Witness also those mathematical tables "independently computed for this work" containing errors identical with older tables. The definitions given by a professional teacher, whose knowledge is gained from and chiefly confined to text books, will therefore be found to differ from those of a mathematician, astronomer or physicist, whose conceptions are drawn from memoirs and documents differing radically from text books. If a mathematician, not a teacher, should write an algebra he would probably reflect usage of mathematical terms by mathematicians better than the teacher; at the same time the teacher might express himself with more clear conciseness and in a manner better adapted to the class room.

The differences pointed out above are illustrated in the work before us. A co-efficient is defined as a *known* factor, in accordance with the usual custom of defining it; it is certain that this restriction is not kept up even in algebraic text-books, as they speak of indeterminate (meaning undetermined) co-efficients. That the leading letters of the alphabet usually stand for known quantities is something which the student has to *un*learn as soon as he gets out of the elements, and often before, as is the case in this work when Interest, Annuities, etc., is reached. The statement (p. 27) that "it is usual to prefix to the parenthesis the sign of the first term that is to be enclosed within it," may be questioned.

"An equation" according to this book "is a statement that two expressions are equal." Suppose we make this statement: "One pound is equal to sixteen ounces," will not this conform to the definition and at the same time will it not fail to represent the algebraist's conception of an equation? According to the definition of "Equation of Condition" $x^2 = my$ is not an equation of condition. "To solve an equation is, to find *the* value of the unknown quantity," thus implying that there is but one value that will satisfy the equation, an impression that will subsequently require correction. The terms *cancel* and *reduce* so much used are not defined. The usage of the first is in accordance with general use but not in accordance with the usual definition. In fact no definition of it in any algebra (I am ready to be corrected) conforms to mathematical usage.

The definition of fraction is purely the arithmetical one in which the numerator and denominator are supposed to be integers and hence fails as a general definition, just as the definition of *index* or *exponent* fails through too great limitation or from tacitly assuming that a general symbol will only have special values.

In spite, however, of the points to which we have called attention above we consider this algebra a useful one. The numerous examples afford the student ample resources for getting practically familiar with algebraic manipulation, and the conspicuous absence of set rules compels the work to be done thoughtfully rather than by rule of thumb. Factoring, that important branch of algebra is fully treated, though the same can hardly be said of radicals. The chapter on logarithms is well done, much better than is common, and to our mind is decidedly the best chapter in the book. The book is well printed and attractive in appearance in spite of the lines at the top of the page and is very free from typographical errors. We have only noticed one, p. 349, Ex. 20, where $10 should read $5.

MARCUS BAKER.

U. S. COAST AND GEODETIC SURVEY OFFICE,
WASHINGTON, D. C., *August* 11, 1881.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING SEPT. 10, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

SEPTEMBER.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 4	30.008	30.032	12 p. m.	29.950	0 a. m.	68.3	64.7	71	3 p. m.	66	3 p. m.	65	6 a. m.	62	6 a. m.	91.
Monday, 5	30.051	30.096	9 a. m.	30.008	6 p. m.	75.3	70.6	82	4 p. m.	75	4 p. m.	68	5 a. m.	66	5 a. m.	132.
Tuesday, 6	30.007	30.042	9 a. m.	29.988	4 p. m.	84.6	76.0	97	4 p. m.	81	4 p. m.	74	5 a. m.	72	5 a. m.	150.
Wednesday, 7	29.934	29.992	9 a. m.	29.894	5 p. m.	90.6	76.0	101	3 p. m.	83	6 p. m.	79	6 a. m.	73	6 a. m.	154.
Thursday, 8	30.031	30.088	9 p. m.	29.928	0 a. m.	79.0	71.3	89	2 p. m.	78	2 p. m.	69	12 p. m.	63	12 p. m.	133.
Friday, 9	30.003	30.082	0 a. m.	29.950	7 p. m.	73.3	68.0	78	4 p. m.	72	4 p. m.	68	5 a. m.	64	5 a. m.	96.
Saturday, 10	29.933	29.994	9 a. m.	29.900	12 p. m.	72.0	69.3	75	9 a. m.	71	9 a. m.	66	12 p. m.	65	12 p. m.	117.

Mean for the week .... 29.995 inches.
Maximum for the week at 9 a. m., Sept. 5th .... 30.096 "
Minimum " at 5 p. m., Sept. 7th .... 29.894 "
Range .... .202 "

	Dry.		Wet.	
Mean for the week	77.6 degrees		70.8 degrees.	
Maximum for the week, at 3 pm. 7th	101.	"	at 6 pm 7th, 83.	"
Minimum " " 6 am. 4th	65.	"	at 6 am 4th, 62.	"
Range " "	36.	"	21.	"

SEPTEMBER	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Begin-ing.	Time of End-ing.	Dura-tion. h. m.	Amount of water	OZONE. 0–10
Sunday, 4	s. e.	s.	s.	87	¾	11.30 pm	.516	.572	.599	83	75	84	8 cu.	9 cu.	9 cu.	-----	-----	-----	--	0
Monday, 5	s. w.	s.	s. s. w.	158	2½	0.50 am	.599	.717	.757	84	70	81	6 cu.	8 cu.	0	-----	-----	-----	--	0
Tuesday, 6	w. s. w.	n. w.	s. w.	81	¾	5.30 pm	.757	.741	.850	90	46	68	10	3 cir. cu.	1 cir. cu.	-----	-----	-----	--	1
Wednesday, 7	w. s. w.	w. n. w.	w.	122	2½	2.00 pm	.717	.559	.827	70	28	57	1 cu.	4 cir. cu.	1 cir.	-----	-----	-----	--	6
Thursday, 8	n. n. w.	e. n. e.	e.	117	1¼	3.20 pm	.690	.809	.529	70	59	74	0	10	10	-----	-----	-----	--	1
Friday, 9	e. s. e.	s. s. e.	s. e.	63	¾	4.00 pm	.529	.625	.693	74	65	85	10	10	10	-----	-----	-----	--	0
Saturday, 10	s. e.	n. e.	n. e.	104	7	8.40 pm	.706	.704	.635	90	81	89	10	8 cu.	10	10½ am 4.40 pm	11½ am 10 pm	1.00 5.20	.01 .08	9

Distance traveled during the week .... 732 miles.
Maximum force .... 7 lbs.

Total amount of water for the week .... .09 inch.
Duration of rain .... 6 hours, 20 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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SATURDAY, SEPTEMBER 24, 1881.

ENCKE'S COMET.

This comet is now visible in telescopes of moderate power, and will increase in brightness until November, when it may be visible to the naked eye. Tbe corrections to the ephemeris, computed by Dr. Backlund, of the Pulkowa Observatory, are as follows:

$$\Delta\alpha = -39^s.0 \quad : \quad \Delta\delta = -1'.4$$

These corrections may vary a little as the comet approaches the earth, but it can be found without trouble. If we consider the great care and labor that have been given to the calculation of the ephemeris; and the fact that the perturbations by nearly all the principal planets have been computed, as well as the effect of the resisting medium in space, the corrections to the ephemeris seem to be very large. This comet affords another example of what is now most needed in Astronomy, viz., complete and careful theoretical investigations. It will be comparatively easy to obtain a great number of observations of this comet during its present return, while a much smaller number of good observations is sufficient. The attention of astronomers should be given rather to a satisfactory determination of the motion of the comet, since the recent computers of its orbit do not have the success of Encke in predicting its returns.

THE WARNER-ASTRONOMICAL PRIZES.

We recently explained, in an editorial, the conditions on which Mr. H. H. Warner consents to present to each of the discoverers of comets during the year 1881, the sum of two hundred dollars. We also stated that applicants for the prize for Comet *b*, 1881, were presenting their claims at the rate of sixty *per diem*.

We now learn by a communication from Mr. C. S. Whittemore, secretary to the Rochester Astronomical Society, that nearly 3000 letters were received claiming priority in the discovery of this comet, all of which have been examined. As a result of such examination, Professor Lewis Swift reports that "no conclusion can be reached that would be scientific and satisfactory." In other words, the claims of the 3000 applicants are ignored, and the prize of $200 for this, the most important of the three comets, so far discovered in 1881, is withdrawn.

We cannot refrain from expressing our dissatisfaction with this decision, and the methods employed in arriving at it, which we believe to be neither "scientific nor satisfactory."

Mr. Warner, in a letter to the public dated September 5th, states, that two of the conditions on which he consented to give a prize of $200 to the discoverer of every comet appearing in 1881, were as follows: That it should be "telescopic" and "unexpected." He now claims that "Comet *b* was neither telescopic nor unexpected." Under these circumstances if Mr. Warner had simply announced that Comet *b* did not come within the meaning of his advertised prize, his course would have been intelligible and satisfactory. But he himself has stated, that in spite of these disqualifying circumstances, he "was anxious, could the first discoverer be found, to make a special reward of $200." In other words, a decision was arrived at to waive the disqualifications, and to proceed as if they did not exist, and the same letter admits that Professor Lewis Swift "examined" the 3000 applications on their merits. Such being the case, when Professor Swift found that he was unable to arrive at a "scientific and satisfactory conclusion," he should (under the terms of the contract between Mr. Warner and the public), have instantly referred the matter to Professor Asaph Hall, of Washington, and the other gentleman named as referee.

Under these circumstances we consider that Mr. Warner is under the moral obligation of carrying out his contract in regard to this matter, and insisting on Professor Swift taking the proper steps to arrive at some decision. Probably the mere perusal of the 3000 letters would instantly reduce the number to some half dozen applicants, whose cases could be submitted to Professor Hall, who would probably decide upon their merits within a week.

The second point in Mr. Warner's letter to which we would draw attention, is that in which he states, that to mitigate his disappointment in not being able to trace the "first discoverer" of Comet *b*, he proposes as a balm to the claimants, and to encourage astronomical study, to offer a prize of $200 to the person who shall prepare the best essay on "COMETS: *Their Composition, Purpose and Effect upon the Earth.*"

The conditions are as follows:

"1st. The essay must be written in plain language, each term to be defined in brackets immediately following, and must not exceed 3000 words.

"2nd. Each essay must be signed with a *nom de plume*, and a sealed envelope must accompany the essay superscribed with the *nom de plume*, and containing the real name of the author.

"3rd. All the essays must be filed with Dr. Lewis Swift, Director of the Warner Observatory, Rochester, N. Y., by November 1, 1881, and he will submit them to the judges.

"I hope that this prize will produce valuable additions to popular astronomical literature."

We regret that Mr. Warner was not better advised when he arranged the title of the essay and named the conditions. By "*plain*" language we presume he calls for "simple" language, but we are unable to explain his desire that "each technical term shall be defined in brackets immediately following." For whose benefit is all this defining to be offered? Is it to aid Professor Swift and the judges? If to aid readers, when the essay is subsequently published, would not a simple glossary of the scientific terms used, added at the end, be more appropriate? If "*each* technical term is to be defined," we fear that a large percentage of the 3000 words permitted will be used for this purpose.

Again, would it not be a more creditable arrangement, that the essays be filed with some independent person, instead of Professor Swift, who is at least a beneficiary of Mr. Warner, and is both a competitor and the judge in these prize gifts? (We think Professor Swift awarded the first Warner prize for comets to himself.)

Lastly, we find that no names are given of those who are to be judges of the value of the prize essays. This omission is very important, and seems to raise a doubt whether any judges whose opinion is worthy of respect can be secured to connect themselves with a scheme proposed under such conditions. Again, what disposition is to be made of the essays received by Mr. Warner? nothing is guaranteed in this respect; and will Professor Swift once more announce to the essayists that "no conclusion can be reached which is scientific and satisfactory?"

We do not wish our remarks to be interpreted in a sense which implies that either Mr. Warner or Professor Swift are desirous of acting improperly in this matter, although their behaviour may, in some quarters, be severely criticised; we rather lean to the view that their judgment and discretion is at fault, and that they require the counsel of some friend who can so advise them, that they arrive at "conclusions which *are* scientific and satisfactory." Mr. Warner hopes that his prize "will produce valuable additions to popular astronomical literature;" we fear that under the conditions he offers, he will be inundated with vulgar scientific trash.

In conclusion, we offer Mr. Warner one word of advice. If he honestly desires to encourage real scientific work and literature, let him permit such men as Hall, Newcomb, Pickering, Young, Stone, Holder or Draper to arrange the title of his prize astronomical essay, and request them to name the conditions, and be the judges, of the merits of the papers submitted. The decision of any two of the gentlemen we have named would be satisfactory to those who are likely to be competitors, provided they acted independently, and untramelled by Mr. Warner or any of his Rochester friends.

HYPERMNESIA OR EXALTATIONS OF MEMORY.*

[Translated from the French by the Marchioness CLARA LANZA.]

Until now our pathological study has been confined to destructive forms of memory. We have seen the latter diminished, sometimes completely destroyed. There are however, precisely contrary cases, in which the apparently abolished memory comes to life again as it were, and faint recollections become intensely vivid. Is this exaltation of the memory (called technically hypermnesia) a morbid phenomena? It is at least certainly an anomaly. When we remark further that it is always connected with some organic disorder or bizarre condition, we cannot deny that it comes within our province to discuss it. There are other subjects, amnesia for instance, which are more instructive, but we should not neglect it for that reason. We will see therefore what there is to learn about *persistence of recollections*.

Hypermnesia is divided into two classes—general and partial.

General exaltation of memory is difficult to determine, because the degree of excitation is quite relative. The force of this faculty varying to a great extent in different individuals we cannot measure it by any common standard. The amnesia of one person may possibly be the hypermnesia of another. It is, if we may employ the word, a change of *tone* in the memory, such as occurs in every other form of psychological activity, thought, imagination or sensibility. Moreover, when we say that the excitation is general, it is nothing more than a probable induction. Inasmuch as the memory is subject to the condition of our consciousness, and as consciousness is only produced in the form of succession, all that we can affirm is, that in the course of a period more or less extended a mass of recollections spring up in every direction.

General exaltation seems to depend exclusively upon physiological causes, particularly the rapidity of cerebral circulation. It is therefore apparent very often, in cases of acute fever. It is also produced in insanity, ecstasy, and hypnotism, sometimes in hysteria and in the beginning of certain mental diseases.

Besides these purely mental pathological instances there are others of a more wonderful nature which depend probably upon the same cause. Numbers of persons who narrowly escaped drowning have stated, that in the moment when asphyxia began, they seemed to see all at once their entire life in all its details, even the most

* See *Les Maladies de la Mémoire* by Th. Ribot, Paris 1881.

trifling incidents appear distinctly before them. One person in particular, declared that he saw his whole existence rolled out in retrograde succession, not like a mere indistinct sketch, but in precise details, making a panorama of his life, in which every act was accompanied by a feeling of pleasure or pain.

An analogous circumstance relates to a man of remarkable intelligence who happened to cross a railroad track just as an express train approached. He had just time to throw himself lengthwise between the rails. As the train passed over him, the sense of his danger caused every incident in his life to suddenly rise before him in memory.

Even allowing for possible exaggerations, these facts reveal a hyper-activity of the memory, of which in a normal state we can form no idea.

I will quote one more instance, due to stupefaction from opium, and at the same time I will beg the reader to observe how well it confirmed the explanation of the mechanism of recollection given in another chapter.

"It seems to me," says DeQuincy in his celebrated *Confessions*, "that I have lived seventy years or a whole century in one night. The most trivial events of my youth, forgotten scenes belonging to my early years, were constantly brought before me. It cannot be said that I remembered them, for had they been mentioned to me while awake, I should not have been capable of recalling a single one of them as forming a part of my previous existence. But placed before me as they were in a dream, like so many intuitions, made up of the most vague circumstances and their accompanying sentiments, I recognized them instantly."

All these general excitations are transitory. They endure no longer than the causes which produce them. Does permanent hypermnesia exist? If the word can be accepted in this rather forced sense, it must be applied to those singular developments of the memory which are the continuation of some chance accident. In ancient authors we find many cases of this kind related. We have no reason to doubt them, for modern investigators, Romberg among others, have noted a wonderful and permanent development of the memory, following small-pox, etc. The mechanism of this transformation being impenetrable, we have, however, no reason to insist upon it.

Partial exaltations are by their very nature limited. The ordinary tone of the memory being maintained in its generality, everything beyond this can be easily ascertained. These forms of hypermnesia are the necessary co-relatives of partial amnesia.

In the production of partial hypermnesia there is nothing resembling a fixed law. It presents itself under the form of isolated facts, that is to say, it is the result of a series of conditions which escape us. Why is a certain group of cells forming a particular dynamic association put in motion sooner than another? We can give no reason, neither a physiological nor a psychological one. The only cases wherein we can affirm the appearance of a fixed law, are those of which we shall speak later, where several languages return successively to the memory.

Partial exaltations generally spring from morbid causes. But sometimes they occur in a healthy state. Here are two examples:

"A lady in the last stages of a chronic malady was taken from London into the country. Her little daughter, who was a baby, not yet able to speak, was brought with her, and after a short interview, was taken back to the city. A few days later the lady died. The child grew up without having any recollection of her mother. When she had attained maturity, however, she had occasion to see the room in which her mother died. Although she was ignorant of this fact, she trembled as she crossed the threshold. On being asked the cause of her emotion, she replied: 'I have a distinct impression of having been in this room before. There was a lady in bed here in this corner. She seemed to be very ill, and she bent over me, weeping.' "[1]

"A man endowed with a highly artistic temperament (observe this point) accompanied some friends to a castle somewhere in Sussex. He had no recollection of ever having been there before. On approaching the entrance he had a sudden vivid impression of having already seen it, and, with the remembrance of the door came also a recollection of people above and beneath the portico, together with some donkeys standing by. As this conviction grew upon him more and more, he questioned his mother, thinking she, perhaps, might be able to enlighten him. She told him that when he was about sixteen months old he had been in the neighborhood with a large party of people, that he had been carried in a basket upon a donkey to the castle and left down stairs with the servants and their donkeys while the other members of the party had installed themselves above the portico to eat their dinner."[2]

The mechanism of recollection in these two cases does not admit of any ambiguity. It is a revival, by contiguity, after a long interval. They present that which happens throughout every instant of life in a striking and uncommon form. In order to recover a lost recollection have not many of us returned to the spot where the idea arose, endeavored to place ourselves as nearly as possible in the same material situation and thus see the remembrance spring to life?

As to hypermnesia arising from a morbid cause I will give one example.

"At the age of four years, a child submitted to trefining in consequence of fracturing its cranium. When his health was quite restored he had no recollection of the accident or the operation. But at the age of fifteen, while suffering from delirious fever, he described the operation to his mother, mentioning the physicians and those present, the details of their dress, and other minute particulars, with the utmost exactitude. Up to this time he had never spoken of the matter nor had he heard any one mention it."[3]

The revival of languages completely forgotten deserves to be spoken of more at length. The instance reported by Coleridge is so hackneyed that I shall not repeat it. There are many others of the same kind which are to be found in the writings of Abercrombie, Hamilton and Carpenter. Anæsthetic sleep produced by chloroform or ether can produce the same effects as febrile excitation. "An old forester lived during his youth on the frontier of Poland, and spoke scarcely anything but Polish. Later he removed to German territories. His children testified that for thirty to forty years he had neither heard nor pronounced a single Polish word. During an anæsthetic sleep of about two hours this man spoke, prayed and sang fluently in Polish.[4]

A still more curious thing is the *regressive* recollection of several languages. Unfortunately, the authors who have mentioned this use the term and note the fact without properly interpreting either.

The most interesting case was observed by Dr. Rush, of Philadelphia. An Italian named Dr. Scandella, a man of remarkable learning, resided in America. He was a complete master of the Italian, English and French languages. He took the yellow fever, and died of it in New York. In the beginning of his illness he spoke nothing but English. After that only French, and on the day of his death, Italian—his native language.

The same writer speaks in rather confused terms of a woman who was subject to transitory attacks of acute mania. When first seized she always spoke very poor

[1] Abercrombie. "*Essay on Intellectual Powers.*"

[2] Carpenter. *Mental Physiology.*

[3] Abercrombie. Work before quoted.

[4] M. Duval. HYPNOTISME *dans le Nouveau dict de Médicine*, p. 144.

Italian. When at the height of her insanity—French. During the first period of recovery, German; and while convalescent, her mother tongue, English.

If we throw aside this regression as far as it deals with several languages and content ourselves with more simple cases, we find abundant and very precise instances. A Frenchman living in England, and speaking English perfectly, received a violent blow upon the head. During his entire illness, he was unable to say a word in any language except his native French.

There is nothing, however, more instructive than the following fact, related also by Dr. Rush: "A Lutheran clergyman, of German origin, living in America, had in his congregation a great number of Germans and Swedes, who, before dying, repeated prayers in their mother tongue. Most of these, he said, he was sure had not spoken a word of German or Swedish for fifty or sixty years."

Winslow has stated that numbers of Catholics, converted to Protestantism, have prayed strictly in accordance with the Roman ritual, during attacks of delirium preceding death

This revival of languages, lost forms and ceremonies, seems to me only to be interpreted by a particular case of the law of regression. In consequence of a morbid action, which generally precedes death, the most recent impressions of the memory are destroyed, and this annihilation descends gradually until it reaches the oldest acquisitions and impressions. They acquire a temporary activity, and are produced in consciousness for a certain period before being wiped out forever. Hypermnesia, then, is merely the result of negative conditions. Regression does not follow from a normal return to consciousness, but from the suppression of more intense and vivid conditions. It resembles a weak, faltering voice, which cannot make itself heard until the loud speech of others has ceased. Impressions, certain habits belonging to childhood or youth, suddenly return, not because they are pushed forward by some cause, but because there is no longer anything to cover them. Recollections of this kind, are, strictly speaking, only a march backwards towards certain conditions of existence which seemed to have disappeared forever, but which a final working of the memory, before entire dissolution, brings once more to the surface. I will abstain, however, from further reflections which these facts naturally suggest, reserving them more properly to moralists. It could be shown, for example, that certain religious frenzies, which have overtaken people upon their death-bed, are, to psychology, merely the necessary effect of a general breaking up, for which there is no remedy.

Independent of this confirmation of our regressive law, is the surprising persistence of these latent conditions of memory which we have called residuum. Were it not for these disorders of the memory we could form no idea of it, for consciousness reduced to itself can only affirm the conservation of those conditions which constitute life and a few others dependent upon the will, inasmuch as they have become fixed by habit.

Must we draw, then, the conclusion that nothing is ever lost in the memory? Must we infer that an impression once formed there is indestructible, and that at any moment it is likely to be revived? Several writers, Maury particularly, have given striking examples to uphold this theory. However, there is no peremptory reason to deny that even without the existence of morbid causes, there are residuums which disappear. It is quite possible that certain cellular modifications and other dynamic associations are too unstable to last. In short we may say that the persistence agrees with a fixed rule, at least, in the majority of cases.

As to the manner in which these distant impressions are preserved and reproduced in memory, we do not know. I will tell you, however, how it can be conceived in the hypothesis which I have adopted throughout this work.

If we admit cellular modifications and dynamic associations to be the material substratum of recollection, there is no memory, however crowded with facts and impressions, which is unable to retain everything. For, if the cellular modifications are limited, the possible dynamic associations are innumerable. We may suppose that the old associations reappear when the new ones, being temporarily or effectually unorganized, leave the road free. The number of possible revivals being diminished the chances augment in proportion for the return of the most stable associations, that is to say, the oldest. I have no desire to insist upon a non-verifiable hypothesis. My aim is to keep closely within the bounds of certainty, and not wander off into doubtful paths.

It has been found impossible to place under the category of any of the preceding morbid types, a certain illusion of a peculiar nature, which occurs rarely, or rather, is seldom observed. There have only been three or four cases mentioned, and up to the present time no special term has been used to designate it. Wigan has called it very improperly, double consciousness. Sander, an illusion of the memory (*Errinnerungs-tauchung*). Other writers have termed it false memory. This latter name seems to me the most preferable. The condition consists in the belief that an entirely new state has been experienced before, so that, when it is produced in reality, for the first time, it seems to be a repetition.

Wigan, in his well known work upon the duality of the mind, says that while present at the funeral service of the Princess Charlotte in Windsor Chapel, he suddenly experienced the impression of having witnessed precisely the same spectacle sometime previous. The illusion was, however, but transitory. Many others of a more lasting nature have been recorded. Lewes associates the phenomenon with some which are more common. When we are in a strange country, for instance, it frequently happens that a sudden turn in a path or river brings us face to face with a view which we are certain we have contemplated before. Sometimes, on being introduced to a stranger, we feel sure that we have already seen him. While reading, new thoughts will often present themselves to the mind as being familiar[6].

This illusion, I think, can be easily explained. The impression received, evokes in our past, similar or analogous impressions, vague, confused and hardly perceptible, but nevertheless sufficiently defined to make us think the new condition is only a repetition of a former one. There is a basis of resemblance rapidly felt between two conditions of consciousness, which causes one to be identified with the other. It is, of course, an error, but it is only a partial one, because there really exists in the past, something which resembles an identical former state.

If, however, this explanation suffices for simple instances, here are others which do not admit of it.

A sick man, says Sander, on being told of the death of an acquaintance, was seized with an access of ungovernable terror because he thought he had already experienced the impression at some former time. "It seemed to me," he said afterwards, "that I was in the same bed on another occasion and X came to me, saying 'Müller is dead.' I answered, "he died long ago. How can he die twice?" "[7]

Dr. Arnold Pick reports the most complete case of false memory that I know of. It assumed the form of a chronic disorder. An educated man, of good reasoning powers, was suddenly attacked—about the age of thirty-two—with a most peculiar mental affection. If he was at an entertainment of any sort, or paying a visit, the

5 See an article by M. Delboeuf in the *Revue Philosophique*, February 1880.

6 Lewes, See *Problems of Life and Mind*, 3d series.

7 Sander, *Archiv für Psychiatrie*, 1873, IV.

event, with all its attendant circumstances, appeared so familiar to him that he was absolutely sure he had previously experienced the same impressions, surrounded by the same people or objects, with the same sky, weather, etc. If he ventured to undertake a new occupation of any kind whatever, he was certain he had done it before and under identical conditions. Sometimes the sensation would occur the same day, in the course of a few minutes or hours, sometimes it did not strike him until the day following, but it was always a distinct impression.

In this phenomenon of false memory, there is an anomaly of mental mechanism which escapes us, and which is difficult to comprehend in a healthy state. The person affected, no matter how acute an observer he might be, could only analyze the condition when he ceased to be deceived by it. From the examples given it would seem to me that the impression received is reproduced in the form of some image. To employ a physiological term, there is a repetition of the primative cerebral process. This is a very ordinary phenomenon. It occurs in every recollection which is not caused by the actual presence of the object. The only difficulty is to discover why this image which arises a minute, an hour or a day after the real condition, should appear to be a repetition of the latter. We may possibly admit that the mechanism of recollection acts in a distorted manner, for my part, however, the following explanation seems more explicit:

The image formed in this manner is very intense and partakes of the nature of an hallucination. It is, apparently, a reality, for nothing rectifies the illusion. Consequently, the real impression is forced back, as it were, and assumes the character of a recollection. It becomes realized in the past, erroneously if we consider the facts, objectively, properly, if we consider them subjectively. The hallucination, although very vivid, does not efface the actual impression, but as the latter is quite separate, and as the former is produced at a comparatively late period, the real occurrence appears to be a second experience. The hallucination assumes the place of the actual impression, it seems to be more recent, and this is really the case. Of course, to us who judge according to what we see externally, it is false to say that the impression was received twice. To the person afflicted, however, who determines solely as his consciousness may dictate, it is true that the impression was actually received twice.

To the support of this explanation, I would call attention to the fact that false memory is nearly always allied to some mental affection. The person mentioned by Pick suffered from a form of insanity. He was continually endeavoring to escape from people he supposed were his persecutors. Hallucinations in this instance would be perfectly natural. I do not, however, wish to assert that my theory is the only possible one. In regard to this isolated condition of false memory, much more numerous and concise observations than mine are probably required.

THE EXCAVATION OF THE GRAND CAÑON OF THE COLORADO RIVER.*

By Capt. C. E. Dutton, U. S. A. U. S. Geological Survey.

The Grand Cañon of the Colorado River is the longest, widest and deepest of the almost continuous chain of cañon valleys through which the upper half of that river flows. Its length is 218 miles, its width from 5 to 11 miles, and its depth from 4500 to 6000 feet. For convenience of discussion it may be arbitrarily divided into four divisions: 1st. The Kaibab division; 2d. The Kanab; 3d. The Uinkaret; 4th. The Sheavwits division. The upper or Kaibab division is the grandest, widest and most diversified, and a little deeper than the others

* Read before the A. A. A. S. Cincinnati, 1881.

The three others are simpler in form and much alike in their topographical features. Capt. Dutton first exhibited a view of the cañon in the Uinkaret division, showing its simplest and most typical form. It consists of an inner and an outer chasm, or a cañon within a cañon. The outer chasm is five to six miles wide, and is walled on either side with palisades 2000 feet high, of singularly noble and graceful profiles, which confront each other across a comparatively smooth plain. Within this plain is sunken the inner gorge, descending 3000 feet lower, and having a width a little greater than its depth. At the bottom of the inner gorge flows the Colorado River, a stream about as large as the Ohio between Pittsburg and Wheeling. The strata in which the chasm is cut are chiefly of carboniferous age. The summit of the outer cañon wall is very near the summit of that series. The chasm throughout the greater part of its extent cuts below the carboniferous and penetrates the Lower Silurian, and even the Archæan schists, revealing the fact that before the carboniferous was deposited the country had been extensively ravaged by an erosion which swept away heavy bodies of Silurian, and probably also of Devonian strata. The carboniferous now rests upon the beveled edges of the flexed older strata, and in many places rests upon the completely denuded Archæan.

The region adjoining the chasm and for 40 to 60 miles on either side is a nearly level platform presenting the summit beds of the carboniferous system patched over here and there with fading remnants of the Permian. The strata is very nearly, but not quite horizontal. There is a slight dip to the northward rarely exceeding one degree, but as the general course of the river is along the strike, the edges of the strata disclosed in the Cañon walls are to all appearances rigorously horizontal.

From 40 to 60 miles north of the river are found the principal masses of the later formations, including the Permian, Trias, Jurassic, Cretaceous and Lower Eocene. These form a series of terraces rising successively like the steps of a gigantic stairway as we move northward. Each formation is terminated southwardly by a great cliff and the strata are nearly horizontal, collectively they have been named the Southern Terraces of the High Plateaus. The latest formation which was deposited in this region was the Lower Eocene.

To the geologist it is obvious that the formations of the Terraces now terminated by gigantic cliffs once extended further out towards the southward and formerly covered regions from which they have been denuded. Captain Dutton is confident that all these terrace formations once reached entirely across the Grand Cañon platform in full volume, and that their ancient shore line is found in Central Arizona. The thickness of the strata thus denuded was a little more than 10,000 feet on an average, and the area from which they have been swept away is more than 13,000 square miles. It is through the heart of this denuded region that the course of the Grand Cañon is laid. The denudation began probably at an epoch not far from Middle Eocene time, since at that epoch took place the final emergence of the region from a marine condition (through the brackish water and lacustrine stages) to the condition of *terra firma*.

It is apparent that the cutting and development of the present Grand Cañon is only a closing episode of a long history of erosion, extending from Middle Eocene time down to the present. Before the river could begin its attack upon the summit beds of the carboniferous which now form the crests of its upper walls, it had to cut through more than 10,000 feet of superior strata. This would alone indicate that the beginning of the present cañon cannot date far back in Tertiary time, and Capt. Dutton thinks that the evidence points strongly to the conclusion that its excavation in the carboniferous began in Pliocene time. This evidence is cumulative and not direct, but is derived from a comparison of many groups

of facts which are too numerous and complex to be summarized very briefly. One group of facts bearing upon the question of age is found in the comparative study of the lateral drainage channels and their gradual extinction by the progressive development of the arid climate of the region which took place in Pliocene time. Nearly all the ancient tributaries of the Grand Cañon appear to have dried up at the beginning of its excavation or very soon after, and the whole work shows the influences upon arid climate.

The Grand Cañon district has also been subject to a great amount of uplifting, amounting in the aggregate according to locality, to 16,000 to 19,000 feet. The present elevation of its surface above the sea is the difference between the amount of uplift and the thickness of strata removed, and is from 7,000 to 9,000 feet. This great elevation is considerably surpassed in some other portions of the West. Obviously, it has been an important factor or essential condition in the process of cañon cutting.

The peculiar forms of the drainage channels of the Plateau country, and of which the chasms of the Colorado are extreme developments, are ascribed to the operation of two groups of processes acting under abnormal conditions. It is customary to say that the rivers have cut their cañons. This is but a partial truth, for the rivers cut passages no wider than their water surfaces. The first group of processes is termed corrasion, the result of which is the continuous sinking of the bed of the stream by the grinding action of flowing water charged with sand. Many factors enter into this result, and their mutual relations are highly complex. But in a general way it may be said that a river with a rapid descent, carrying a notable quantity of sediment, but not enough to overload it or overtax its transporting power, will continuously corrade or grind down and deepen its channel. If it is overloaded, a portion of its sediment will be deposited and form a protective covering to the bed-rock. Under special conditions it will actually build up its bed. Most rivers, along their middle and lower courses have their general conditions so adjusted that there is little or no tendency either to build up or corrade. To this equilibrium of adjustment all rivers are tending, and most rivers have nearly or quite reached it. The Colorado is exceptional in this respect, and its tendency is to corrade. Its waters, though carrying great quantities of sediment, are still under-loaded, and could carry more if they could get it. This tendency to corrade may be ascribed to the fact that the country through which it flows has been gradually rising in altitude through Tertiary and probably also Quaternary time, and this elevation produces and maintains a rapid declivity in the stream-bed, which in turn imparts a high velocity, and consequently great transporting power to its waters.

The widening of the cuts made by corrasion is the work of the second group of processes, viz., weathering. This is also a very complex action, and cannot be briefly summarized. To this action is due the remarkable sculpture of the cañon and cliff walls and all those surprising resemblances to architectural forms which are so abundantly displayed in the Plateau country, and most especially in the Grand Cañon.

The concluding portion of Captain Dutton's lecture was devoted to a description of the scenery in the Kaibab division of the cañon, which is declared by all who have seen it to be the most sublime and impressive spectacle in the world.

New Observatory.—A meteorological station is to be erected at Pavia, under the direction of Professor Cantoni. Investigations will be made at this station on the influence of heat, light, electricity, etc., on vegetation in general, and some cultivations in particular, and also the diurnal and annual variations of terrestrial magnetism.

MIXED SUGARS.*

By Professor H. W. Wiley.

Mixed sugars are made of cane sugar and *amylose* (starch sugar.) Within a few years the mixed sugar industry has advanced from a small beginning to a business of considerable importance. It is difficult to get accurate data of the amounts of this sugar made. Manufacturers and dealers are extremely reticent on the whole subject, and often refuse to talk about it at all. I have, however, after considerable trouble, been able to get at the figures which will give at least an approximate estimate.

The principle centers of the grape sugar industry are Brooklyn and New York, Buffalo and Peoria. From a careful comparison of the data which I have been able to collect, I place the daily product of mixed sugars at the several factories at 1,500 barrels. This will be found not far from the truth. It is rather under than over the true number. It is thus seen that the mixing of sugars is a fact which is altogether too large to be laughed at. It must be remembered, too, that the manufacture is rapidly increasing, and is only limited now by the quantity of dry white amylose that can be made.

Amylose costs 3½ to 4 cents a pound by wholesale. Uuntil the price of corn became so high it was half a cent less than this. It is, therefore, a very profitable business to mix it with cane sugar and sell the whole for the same price which the cane sugar would fetch alone. I have here on the table specimens of these mixed sugars. Here are eleven samples made by the Manhattan Refinery, of New York, also six samples from the Atlantic Refinery, of Buffalo, and six samples from Henry Hobart, of New York. These sugars are sold retail under various names. Of these I may mention "New Process Sugar," "Niagara A B C," "Harlem B," "Excelsior C." and various others. To the eye these sugars look very much like straight cane sugars, and are generally pure and wholesome. They differ from the pure cane sugars in being less soluble in water and in being less sweet to the taste.

It has been estimated that amylose is two and a half times less sweet than sucrose ; but this depends largely on the method of manufacture. Some samples of amylose will be found quite sweet, while others impart even a bitter taste.

In the manufacture of mixed sugars it is highly important that the amylose be dry. If hydrated amylose be used it is found almost impossible to pulverize it, and when ground it is pasty and sticky. Machines have been patented for obtaining finely granulated amylose from the well dried specimens. It is quite impracticable, however, to obtain amylose entirely dry, and it is capable of being worked very well when it still contains 8 to 10 per cent of water. This water is put in when sold at the same price as pure sucrose. In a commercial sense it is, therefore, not a disadvantage. The amylose which is used in mixing is generally made by high conversion under pressure. It, therefore, contains a high percentage of glucose, (dextrose) as compared with the maltose and dextrine present. It is, therefore, less sweet to the taste than the liquid amylose, where the percentage of maltose is larger.

Many schemes for the estimation of the different constituents of a mixed sugar have been proposed. For a discussion of the methods of analysis by reduction and fermentation, I refer to my paper read before this section last Saturday. I will content myself here with a brief outline of the method which I have employed. The water is estimated by heating two or three grammes in a flat platinum dish to 150° C. for two hours. The percentage of cane sugar I determine by Clerget's method. First get the total rotation in the polariscope then invert

* Read before the A. A. A. S., Cincinnati, 1881.

by heating to 68°, then polarizing again, carefully noting the temperature. From these readings the percentage of cane sugar present is calculated from the following formula:

$$x = \frac{a - a'}{144 - \frac{t}{2}}$$

Here a=first reading of polariscope.
a'=second reading of polariscope.
t=temperature of observation.
x=percentage cane sugar required.

In connection with the polariscope readings I also made reductions both before and after inversion, and thus obtained valuable data in regard to the nature of the amylose present, as well as securing a check on the optical results.

Following is a scheme of an analysis which will illustrate the method of procedure:

Reduction. Took 10 g. in 1000 c.c. Of this, to reduce 10 c.c. Fehling's Solution, took 27.8 c.c.

Then 1000 : 27.8 = x : .05 (.05 g = sugar corresponding to 10 c.c. copper solution.)

Whence x = 1.8 g. = 18 per cent reducing matter.

Polarization. 26.048 g. in 100 c.c. gave........ 97°.8+
After inversion at 21° " .... 2°.6—

Difference, = ... 100°.4

$$100^\circ.4 \div 144 - \frac{21}{2} = 75.2 = \text{per cent sucrose.}$$

Reduction after inversion.
For 10 c.c. copper solution took 5.35 c.c.
Then 1000 : 5.35 = x : .05.

x = 9.35 g. = ...	93.50	per cent.
Deduct 18 per cent due to amylose = ..	75.50	"
Due to invert sugar....................		"
Sucrose by polariscope................	75.2	"
Amylose, water and ash by difference...	24.8	"

Following are the results of twelve examinations of mixed sugar:

No.	Per cent Reducing Matter.	Per cent Sucrose by Polariscope.	Amylose, Water, Ash, etc., by difference.
1	*29.70	*71.4	28.6
2	24.6	64.35	35.65
3	25.64	68.2	31.80
4	25.00	64.72	35.28
5	22.52	66.80	33.20
6	24.4	60.34	39.66
7	26.88	60.7	39.30
8	25.00	68.6	31.40
9	30.5	59.9	40.10
10	25.8	71.6	28.40
11	26.6	61.0	39.00
12	18.0	75.4	24.60

The analysis of mixed sugars is at this time a matter of great public interest. It is important that the public be not defrauded by purchasing sugars under false names. It is true that the manufacturers, as far as I know, do not sell the mixed sugars as *straight*, but when they pass into the hands of the retail dealers they are usually disposed of as if they were genuine. I do not anticipate that mixed sugars will jeopardize the public health. When well made they are certainly palatable and harmless. For boiling with fruits, etc., as in making preserves, they are quite as efficient as cane sugars. Nevertheless a "mixed sugar" should be bought, sold, and consumed as a mixed sugar, and thus all "winking" at fraud be prevented.

* One of these numbers is evidently incorrect. On looking over my notes I cannot find the mistake, and I have no more of the sugar with which to repeat the analysis. I think the error is in the per cent of reducing matter.—H. W. W.

COAL DUST AS AN ELEMENT OF DANGER IN MINING; SHOWN BY THE LATE EXPLOSION IN THE ALBION MINES IN NOVA SCOTIA.*

By H. C. Hovey.

My object, in this communication, is to lay before the public by permission of Mr. Edward Gilpin, Inspector of Mines for Nova Scotia, the results of his investigations into the part played by coal dust in spreading and augmenting the terrible explosion that took place in the Albion mines, near Stellarton, on the East river, N. S., on the 12th of November, 1880.

Explosions are frequent in the coal mines of England and Belgium, causing loss of many lives and the destruction of much property. But in our own country, as a rule, we are fortunately exempt from such calamities. On the other side of the Atlantic there are special causes leading to these explosions; the thinness of the seams, the depth of the workings, the gaseous nature of the coals most prized for their coke and illuminating power, all combining to render difficult the great problem of producing ventilating currents sufficiently powerful and searching to insure the safety of the workmen.

Within the last few years men of scientific and practical knowledge have studied into these disasters and their causes, hoping that some remedy might be found that could remove the dreadful uncertainty hanging over the lives of those who help to sustain the fabric of modern civilisation. It was discovered by a rigid inspection of all available accounts of explosions, that many of them had occurred in pits known to be, as a rule, not dangerous from explosive gas, or declared to be free from it shortly before the moment of the accident. Then the fact gradually became apparent that a seemingly innocuous constituent of the mines, namely, *coal dust*, played an important part in spreading and augmenting the blasts. It was as if the wadding of a gun was composed of an inflammable material which, on ignition of the charge, doubled its effect.

It is well-known that chemical action is often induced in heaps of slack, such as exist in thick coal workings, and that the heat evolved may be enough to cause ignition. The danger is increased when the broken coal is comminuted, and floats in the air, as in this form, under various conditions, it may undergo rapid oxidation. Galloway's experiments show that when the particles of dust are so fine as to pass through the gauze shield of a safety lamp, an explosion may be caused by their ignition. Bauerman states that, in the Franco-Belgian collieries, "several fatal explosions have been traced to the firing of coal dust from the flame of a shot, even in cases where no fire-damp was present in the workings."

A brief glance at the history of the Albion Mines will not be misplaced. The main seam, which has been continuously worked since 1807, attains the remarkable thickness of 37 feet 6 inches, and is a highly bituminous coal, well adapted for gas and coke-making. It contains several layers of iron stone up to one foot in thickness, but may be considered as an unbroken mass of coal.

The earliest workings, now known as "the Burnt Mines," furnished large quantities of coal, until they were abandoned in 1839 on account of fire, which blazed so fiercely from the shafts that the chains used in raising the coal tubs, were melted. A new opening, the Bye Pit, was worked till 1863, when a fire occurred from a shot lighting gas. This was extinguished; but a short time after the gas ignited at the boilers, which were within a few yards of the top of the pit, and the works had to be closed up. The Foster Pit was next opened, but in 1869 spontaneous combustion caused a fire, which necessitated its abandonment before it was fairly under way.

* Read before the A. A. A. S., Cincinnati, 1881.

At length the Foord Pit was won out, and, with its great pumps and engines, formed one of the finest coal-mining establishments in America. The ventilation was effected by a large Guibal fan, similar to those used in the Pennsylvania Mines, and capable of circulating 120,000 cubic feet of air per minute through the ramifications of the mine. The workings of the Foord Pit, which is nearly 1,000 feet deep, extend about 1,800 yards to the north, and 1,700 yards to the south, having an average breadth of 350 yards. The galleries varied in height from 9 to 15 feet, being driven in the upper part of the seam, the lower part being left for later operations. The ventilation through the south side was maintained at the average rate of 25,000 cubic feet per minute. Shortly before the accident referred to, I went entirely through the colliery, in company with Mr. Gilpin and the overman, and we remarked the perfection of the ventilation, and the consequent absence of deleterious gases, even in the remotest bords.

On the morning of the disaster the night watchmen reported the mine to be free from gas, except in small and harmless quantities. From what source, then, originated the series of explosions that began within an hour from the time this report of entire safety was made, and continued at intervals until the mine became a furnace, whose flames could be subdued only by emptying into its burning chambers the waters of the adjacent river? Was there some sudden exudation of gas from the solid coal, or was this explosion due to the firing of coal dust from a safety-lamp or the flame of a blast?

None of the forty-four men who witnessed the beginning of the catastrophe escaped to tell the story or explain the mystery, and those rescued from more distant galleries had but conjectures to offer. The workmen on receiving the assurance that the mine was free from gas, received their orders, descended the drawing shaft, received their safety-lamps at the lamp cabin and a part of them went into the north side workings and the rest went into the south side dips and waited for their tools to be sent in for distribution. At this moment the explosion took place that was first noticed at the fan-shaft, where it blew the cover of the fan-drift off, and about one minute later it was apparent at the drawing shaft, having traveled in the one case *with* and in the other *against* the ventilating current.

The only additional facts definitely ascertained were gathered by an exploring part led by Mr. Gilpin, who, shortly after the original explosion and at the risk of life, descended into the pit and penetrated as far as the after-damp would allow them to go. The locality where the unfortunate workmen whom they tried to save were known to be was 1,200 yards south of the shaft; and the point reached by the party was only about 600 yards in that direction.

It was evident that the flame of the explosion had not reached as far as this, for there were no marks of fire on the dead bodies of men and horses found, nor was the splintered wood-work charred. They carried two corpses to the surface for examination, and it was found that one of these died of the after-damp, and the other from being dashed against some timber.

The walls of the galleries had been swept clear of timber, and presented the appearance of having been brushed with a broom. This was due to the passage of great volumes of dust which lay on the floor of the level in waves and drifts, into which the party often sank up to their knees. A similar effect was visible in the mine level, but not to so great a degree; as it was damper about the floor, and from the effects observed it would appear that, while the explosion passed along each level simultaneously, it had greater power in the lower one, as the doors were blown toward the upper, or main level. Clouds of the finer particles were carried up the shaft, and were swept on into the North-side levels.

At the lamp-cabin, where the safety lamps were cleaned and given to the men after being examined by the shot-firers, an open light had been kept burning for years, as it was considered a safe place, being within a few feet of the bottom of the shaft. But here a *secondary* explosion took place, demolishing the cabin, burning the horses between the shaft and the cabin, and fatally injuring the lamp-man by igniting his oil-soaked clothing, so that he died in a few days. The effect of the explosion did not extend far into the north side, and some of the men there were ignorant of the disaster until warned by the over-man to leave the pit.

Secondary explosions caused by extracted, or generated gas, are nearly always in the vicinity of the primary one; but here is a case where the second was half a mile from the first, with an intervening space of at least a quarter of a mile known to be free from gas, because men were in it with lamps which showed no indications of its presence.

The ignition of these volumes of dust would, no doubt, have done serious injury to the shafting, had not the latter been wet and indeed saturated with water oozing under pressure through the upper strata into the shaft, and then falling to the bottom; so that, although elsewhere the mine was a very dry one, it was here in such a condition that the flame would be extinguished as soon as it touched the damp walls. The necessity of watering dusty mines has been pointed out by Inspector Gilpin, and this is said to be practised in some of the Belgian collieries. The present instance shows that such a precaution would tend to reduce the range of the explosion of the dust.

Attempts were made to restore the ventilation of the workings in the Albion Mines, when the presence of a large fire was discovered, and this made it necessary to flood the galleries. In about 48 hours after the explosion, a trench had been cut through to East River, and the water was let in at the rate of 15,000 gallons per minute, until, within a week, all the workings were filled. This, of course, made further investigations impossible, and nothing will be known beyond what has here been told.

The subject is one of acknowledged importance. There have been frequent explosions in flouring mills, said to be attributable to the ignition of flour dust. At a late meeting of the Manchester Geological Society, (in England), experiments were made to show that even finely powdered slate will spread the flame of gas explosions. Since the preparation of the present paper, a report has been made before the royal commission on accidents in mines, by Mr. F. A. Abel, Chemist to the British War Department, in which it is claimed, as demonstrated, that coal-dust is not only a fiercely burning agent, but when suspended in air currents may operate as an exploding agent. It operates, aside from its inflammability, as a finely divided solid, in "determining the ignition of only small proportions of fire-damp and air, and consequently in developing explosive effects," *i. e.*, under circumstances which, in the absence of the dust, would be attended by no danger.

HISTORY OF ALHAZEN'S PROBLEM.

Abstract of a paper read before the American Association for the Advancement of Science, Cincinnati, August, 1881, by MARCUS BAKER, U. S. COAST AND GEODETIC SURVEY, Washington, D. C.

Alhazen's problem is an optical one and was thus stated by the Arabian Alhazen for whom the problem is named. "*Given a luminous point and a point of vision unequally distant from the center of a convex spherical mirror, determine the point of reflexion.*" The solution of this problem involves the solution of the following geometrical problem now generally known among mathematicians as Alhazen's problem. *From two given points in the plane of a given circle draw lines meeting in the circumference and making equal angles with the tangent drawn at that point.*

This problem was first solved by Alhazen, a learned Arabian of the 11th century, and published at Basle, in Latin, in 1572. Since that time it has been studied by several distinguished mathematicians and a variety of solutions given. The paper presented contained a collection of these solutions aiming to be complete. Eleven solutions were contained in this collection, beginning with Alhazen and ending with a solution by E. B. Seitz, in 1881.

The first five solutions are by geometrical constructions, in which the points sought are determined by the intersections of a circle and hyperbola. The sixth solution, also a geometrical construction, is by means of the intersection of a circle and parabola. The seventh, eighth, ninth and eleventh solutions are by analytical or algebraical methods, while the tenth is a trigonometrical solution.

Among the people who have studied and solved the problem are Alhazen, Barrow, Hutton, Huyghens, Kaestner, Leybourn, L'Hospital, Robins, Seitz, Sluse and Wales. A complete list of bibliographical references was appended to the paper.

The paper contained further, an extension of the problem, *first*, to the surface of a sphere, and *second*, to an ellipse. The first case was illustrated by the following practical example:

The great circle track between San Francisco, Cal., and Yokohama, Japan, reaches nearly to latitude 52° N. The Pacific mail steamers plying between these ports usually avoid going north of latitude 45° N. Now, if the 45th parallel of latitude be designated as one north of which the steamer is not to go, in what longitude must this parallel be reached in order that the steamers' path between the ports shall be the shortest possible? The extension of Alhazen's problem to the surface of the sphere solves this problem and the longitude required is 168° W. from Greenwich.

The extension of the problem to the case in which an ellipse replaces the circle gives rise to a very complex equation of no special value.

WASHINGTON, D. C., Sept. 13, 1881.

ROTATION OF REDUCING POWER, AS MEASURED BY FEHLING'S SOLUTION TO THE ROTATORY POWER OF COMMERCIAL AMYLOSE (GLUCOSE AND GRAPE SUGAR).

SECOND PAPER.*

BY PROF. H. W. WILEY, Lafayette, Ind.

In a paper read at the Boston meeting of this Association[1] I called attention to the fact that the reducing power of Amylose, measured by Fehling's Sol., could be readily determined by the polariscope. Since that time I have extended the series of observation then reported, and with such results as justify the conclusions at which I arrived.

In commercial Amyloses, whose specific gravities do not vary much from 1.410, the reducing power is reliably calculated from the reading of the polariscope. The average percentage of water in these Amyloses is nearly thirteen. If we allow one per cent for optically inactive substances present, we may safely place the optically active matter at 86 per cent. By prolonged boiling with acids, even if they be quite concentrated, only about 82 per cent of reducing matter is obtained.[2] Further boiling causes the mass to turn brown, and may even cause a decrease in the amount of reducing matter found. Since there is so much difference of opinion respecting the reliability of Fehling's solution, and since there is no other reducing mixture that works as well, it would, perhaps, be better to use the polariscope for the determination of the amount of substances present in an Amylose capable of reducing the various solutions used for grape sugar measurements.

In the following table the calculation of the reducing power was made by the formulæ,[3] which I have already explained. Although, in a few cases, the specific gravity varied by a few thousandths from 1.410, the difference has not been of sufficient importance to make any correction[4].

Since the ordinary Amyloses, called grape sugars, of commerce differ from those called glucoses only in having the processes of conversion carried further, it is found that the same rule applies to them also. In fact, I believe it will be found true with all varieties of Amylose made by use of sulphuric acid, provided 8.6 grammes of the anhydrous substance be used in each 100 c.c. of the mixture to be examined.

Following are the results of my observations:

TABLE I.

No.	Sp. gravity.	Reducing matter by Fehling's Solution.	Rotation of 10 g. in 100 c.c. cone sugar scale.	Reducing matter calculated from Polariscope.	Difference.		Date of Manufacture.
					+	—	1880.
1	1.414	52.1	53.04	52.05	0.04	...	September 15
2	1.419	52.2	53.00	53.00	0.08	...	" 14
3	1.410	53.8	51.00	55.05	1.07	...	" 15
4	.....	53.2	55.05	49.09	....	3.3	October 12
5	1.412	51.0	54.01	51.06	0.06	...	" 18
6	1.413	51.1	53.02	52.75	1.65	...	" 19
7	1.417	51.6	53.45	52.44	0.84	...	" 19
8	1.417	49.7	55.02	50.03	0.06	...	" 20
9	1.408	49.0	55.05	49.09	0.09	...	" 21
10	1.413	49.5	55.04	50.00	0.05	...	" 21
11	1.411	48.1	56.06	48.05	0.04	...	" 17
12	1.421	48.8	56.04	48.08	0.00	0.0	" 16
13	1.417	50.0	57.00	48.00	....	2.0	" 16
14	1.413	46.4	56.07	48.04	2.00	...	" 14
15	1.417	48 1	56.05	48.06	0.05	...	" 14
16	1.418	46.3	58.02	46.05	0.02	...	" 13
17	1.412	47.2	57.00	48.00	0.08	...	" 12
18	.....	72.0	37.03	72.63	0.63	...	Unknown.

The above analyses were of samples sent by the manufacturers, the Peoria Grape Sugar Company. They represent the whole number of samples examined and in the order in which the analyses were made. Seventeen of them were of syrups, and the eighteenth of a solid sugar. Only four out of the eighteen show discordant results. In one of these the specific gravity was not determined. It was my intention to make these four analyses in duplicate, but a press of other business prevented. In general, it appears that the results given by the polariscope, by the above method of calculation, are a little too high. If they were diminished by 5 the agreement would be better. That the reducing power of Amylose can be correctly calculated from its rotatory power is certainly established from the thirty-eight unselected instances which have been presented.

ELECTRIC LIGHT FOR LIGHTHOUSES.—The first of the series of lighthouses round the French coast which are to be henceforth illuminated by electricity, has, with all its necessary machinery, been completed. It is called the "Phare de Planier," and is situated at the mouth of the Rhone, near Marseilles.

* Read before A. A. A. S., Cincinnati, 1881.

[1] Proceedings of this Association, 1880, p. 308; Journal Am. Chem. Soc., Vol. II., p. 387.

[2] Proceedings A. A. A. S., 1880, p. 320. Journal Am. Chem. Soc., Vol. II., p. 399.

[3] Proceedings A. A. A. S., 1880, p. 313. Journal Am. Chem. Soc. Vol. II., p. 393.

[4] Proceedings A. A. A. S., 1880, p. 316. Journal Am. Chem. Soc., Vol. II., p. 395.

ON THE INTERIOR CONDITION OF THE TERRESTRIAL GLOBE.

By M. E. Roche.

Generally we admit that the earth is entirely fluid in its interior, with the exception of a thin crust, and most of the mathematical studies on the figure and the constitution of the earth assume this fluidity. Thus by attributing to this fluid a certain law of compression, Laplace has deduced a corresponding law of densities, which Legendre had already examined before him, and which permits the calculation of the flattening of the different surfaces *de niveau* of the terrestrial mass. I have myself proposed another law of compressibility, which conducts to a very simple formula for the increment of the density. The conditions which every hypothesis must satisfy, on the distribution of the mass in the interior of the earth, are that it must accord with the value of the superficial flattening, and also with a certain constant depending on the phenomenon of precession. These conditions are very approximately satisfied in the hypothesis of fluidity, if we admit that the terrestrial flattening is nearly $\frac{1}{300}$; but if this flattening is greater than $\frac{1}{295}$, as it seems to result from the most recent determinations, the agreement no longer exists.

There is need, therefore, of a new examination of these researches under a different hypothesis, for example, in considering the globe as formed of a nucleus or solid mass very nearly homogeneous, covered with a lighter shell whose density, from geological considerations, can be estimated as 3 with respect to water. This constitution of the globe being supposed, I find that it is possible to conciliate the actual values of the precession and of the flattening, if we take account of this, that the interior nucleus of the globe solidified and has taken its definitive form under the influence of a rotation less rapid than that with which the earth is now animated.

In every case the contraction due to this cooling of the globe must lead to a progressive acceleration of its angular velocity. But if this globe is fluid the figure of the different strata adapts itself continually to the rotation which has place at each instant, in such a way that finally there remains no trace of the successive variations which their flattening have undergone since the origin. If, on the contrary, at a certain epoch of the cooling the interior strata have passed to a solid state, these strata have taken and preserved a flattening very different from that which would be attributed to them by the general equation of hydrostatics applied to a mass entirely fluid and possessing a rotation common to all its parts. The formulæ calculated in the hypothesis of a solid nucleus contains at the same time the constant q, the actual ratio of the centrifugal force to the equatorial gravity, and value q_0, of the same ratio at the epoch of the solidification of the central mass. This last element, not being determined, we can give to it a value such that the superficial flattening accords with the coefficient of precession. It is necessary for this to suppose q_0 less than q, whence it results that the terrestrial rotation has undergone an acceleration since the consolidation of the interior nucleus.

The physical and astronomical conditions of the problem permit also the determination, with some precision, of the dimensions and specific weight of this central mass. If we leave out of consideration the crust purely superficial, as also a slight condensation towards the centre where the heavier materials would be collected, the constitution of the globe will be as follows: a nucleus, of which the density is nearly 7, covered with a shell of density 3, whose thickness does not attain one-sixth of the entire radius.

The central terrestrial mass is therefore in specific weight analogous to meteoric iron, while the stratum that envelops it is comparable to aerolites of a stony nature, where iron enters only in a small proportion.

BOOKS RECEIVED.

The Microscope and its relation to Medicine and Pharmacy. Edited by Chas. H. Stowell, M. D., and Louisa Reed Stowell. Published monthly. Ann Arbor, Michigan, 1881.

There are already two journals published monthly which are devoted to microscopy, it is therefore with some surprise that we find a third journal of the same description appealing to the patronage of Microscopists.

"*The Microscope*" claims to supply a want in offering physicians a journal which treats exclusively of Medical and Pharmaceutical Microscopy, thus differing from the two former microscopical journals which cover the whole field of Microscopy.

We believe that the success of "*The Microscope*" will depend upon that journal being conducted strictly within the limits of its own programme. Undoubtedly the majority of American microscopists are members of the medical profession and, therefore, "*The Microscope*" may look for a numerous constituency.

"*The Microscope*" has been produced in an excellent form, is well printed, and illustrated with good illustrations, and if the editors will confine the columns to *Microscopy*, to the exclusion of facetious "items" clipped from their exchanges, they may hope to place their journal on a firm basis.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "Science."

Permit me to suggest a few questions that should be answered by the author of "The Great Primordial Force." (See "Science," p. 405.)

Let me introduce my questions by announcing my *belief* in the conservation of energy and in the unity of force and by expressing a *hope* that the time will come when the phenomena peculiar to the different *manifestations* of force may be comprehended and their identity demonstrated.

The following questions are respectfully suggested:

1. When, and by whom, has it been demonstrated that gravitation is resolvable into light or heat?

2. If all force "is substance," or matter, (see "Science," p. 405, last paragraph), then, at least some matter is force. What then is the distinction between matter and force?

3. On p. 406, it is asserted that the two elements, "*Motion* and *Magnetism*," "develop all known forces of the universe." How can motion exist without a *prior* force, and what *is* magnetism?

4. What produces the revolution of the revolving celestial armatures?

The foregoing questions may be sufficient for the present, but it is due to science that statements claiming to be scientific should be *truths*. Let us examine some of the author's statements.

It is asserted that "if gravity acts inversely as the square of the distance, then the earth at aphelion could not, without the aid of some other force, return to perihelion." It is to be feared that the author of the above question is not aware that the inertia of matter is an experimental fact, and is entirely sufficient to bring back the planet from aphelion to perihelion.

As to the law of inertia, all matter, if not acted upon by some external force, will continue in its present state, whether of motion or rest, and because while a planet is passing from perihelion to aphelion the tangent to its orbit makes an obtuse angle with its radius vector; therefore its inertia counteracts, and to a certain extent,

opposes its centripetal force, from perihelion to aphelion, and because at aphelion, the tangent of its orbit, is at right angles with its radius vector, and from aphelion to perihelion the angle between the tangent and radius vector is acute, therefore, the inertia of the planet co-operates with the centrifugal force from aphelion to perihelion.

Hence, *inertia*, in conjunction with the centripetal force, is sufficient in all cases of elliptic motion, to bring back the planet from aphelion to perihelion.

Again, the writer says, (p. 407) "the degree of ellipticity of each planetary orbit is due to the inclination of its axis." That the reader may judge how much weight should be allowed the author's statement, let this statement be compared with known facts.

The eccentricity of the earth's orbit is .0167922; that of Venus is .0068618. The inclination of the earth's axis is about 23½°; that of Venus 75°. That is, the inclination of the earth's axis to its orbit is less than one-third that of Venus; whereas, the eccentricity of the earth's orbit is more than twice that of Venus.

The author's statement, therefore, is not only not supported by the facts, but is in conflict with the facts.

J. E. HENDRICKS.

DES MOINES, Sept. 3, 1881.

To the Editor of "SCIENCE":—

The prevailing scientific ideas of any period are regarded by the common people, and even by the scientists of that period as indubitable facts, without due examination of their origin or their foundation. But further thought and observation often compel a reluctant retreat therefrom. Thus present conceptions have all the weight of perfect truth. We call them "*facts.*"

The rapid discoveries of the present age, and the unprecedented freedom of thought are disturbing all theories that are not well founded. The new law of Conservation of Force will cause the final destruction of every theory that is not in harmony with it. The paper upon the "Great Primordial Force" was but an effort to bring the explanation of the physical forces into consistency with that all-governing law.

That the paper should give rise to such questions as those proposed by our critic was most natural, and we shall endeavor to answer them in the same candid spirit in which they were asked.

1. Bodies fall by force of gravitation. Resistance to such fall of course produces light and heat, precisely as resistance to the motion of the electrical current produces the same. If we admit that the electrical current is convertible into these forms, by parity of reasoning, so is gravity.

2. The relations of matter and force seem adequately set forth in the paragraph referred to. (See "SCIENCE," p. 405.) Electricity, which in its tenuity pervades all matter, is abundantly demonstrated to be itself matter in varied form, as in the thunderbolt, the fire ball, and in the St. Elnios fire.

3 and 4. The only rational explanation, whether to us satisfactory or unsatisfactory, of the origin of force, is found in the hypothesis and admission of already existing force, the *primum mobile*. HELMHOLTZ says, that a body set in motion around the sun in vacuous space, and with a certain velocity will continue to move with the same velocity to all eternity. It is sufficient for us to know that the motion *is*, and the magnetism *is*, and thus we have the "celestial armatures" already in revolution, —the effects of which it is for us to observe. To us the effects are, and are called, the "physical forces."

Our critic is disturbed by our questioning of the dogma that "gravity acts inversely as the square of the distance," —on the ground that if that force is weakened by the earth's being removed to aphelion, it could not again bring back the body to perihelion. We re-affirm FARADAY'S position; "The received idea of gravity appears to me to ignore entirely the principle of the conservation of force, and by the terms of its definition, if taken in an absolute sense, 'varying inversely as the square of the distance' to be in direct opposition to it." But we would not rest the assertion upon any great name. It is evident that inertia can "bring back" nothing, that inertia, or momentum, or centrifugal force, or whatever other expression is used, may effect only motion in a straight line. Momentum, (which evidently is what the critic means by "inertia,") has no tendency towards circular motion. It is attraction, gravity, centripetal force alone that draws, or "brings back," and if that force is *weakened*, how can it make itself stronger? If once diminished (as the principle, "gravity acts inversely as the square of the distance" necessitates,) then the opposite force has the balance of power, has destroyed the equilibrium, and except some favoring force steps in to restore the lost ground, momentum, ("inertia") must forever send it farther and farther into space.

Finally, our critic in order to show that the electrical theory, which makes the inclination of a planet's axis to govern the ellipticity of its orbit, is not true to fact, adduces the instance of Venus. This asserted "*fact*" (?) that Venus' axis has an inclination of 75°, is wholly unfounded, Astronomers to-day are not so ready to assert it. The dazzling brilliancy of this planet prevents any positive disclosures as to the period of its daily revolution, to say nothing of the more delicate and difficult determination of the inclination of its axis.

Our distinguished astronomer NEWCOMB says:—The latest physical observations on Venus, with which I am acquainted are those of Dr. VOGEL, "Bothkamp Observations, 1873." The result to which these observations point is that the atmosphere of Venus is filled with clouds so dense that the solid portion of the planet cannot be seen, and no time of rotation can be determined." HERSCHEL said that he was never able to see any permanent markings on Venus,—but it is only by such markings that these determinations are made.

H. RAYMOND ROGERS, M. D.

DUNKIRK, N. Y.

MEDICAL CONGRESS NOTES.

(London, 1881.)

At the close of Professor Huxley's address, Mr. MacCormac followed with a statement, the most important items of which were that the number of members amounted to 3,210; that the sections had held 11 meetings, extending over 293 hours; that there had been delivered 464 written papers and 360 oral addresses. The attendance at the sections had been large, and had not shown signs of falling off even quite to the close. The museum was referred to as a great success, and the demonstrations of living patients had been attended by crowds each morning.

ENDORSING VIVISECTION.

Sir James Paget then presented the following resolution forwarded by Professor M. Foster, from the Physiological Section: "That this Congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable for its future progress; that, accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion that, in the interest of men and animals, it is not desirable to restrict competent persons in the performance of such experiments." Pointing out that it was impossible to discuss such a resolution then, the President asked those who were opposed to it to record their names and votes at the close of the meeting. The resolution was then adopted with loud cheers, and no hand was held up in opposition to it.

NOTES.

THE PLANTÉ AND FAURE BATTERIES.—Speaking of the relative merits of the two batteries, M. Faure says in a letter to "*The Electrician*," M. Planté has the merit of being clear in the exposition of his ideas and researches, and in his remarkable work, "Recherches sur l'Electricité," he tells us exactly how his battery is made, how it is "formed," and what it does when so made. Referring to this last point we read that a battery can furnish a constant current through fifty metres of copper wire one millimetre in diameter, say one ohm during one hour. Now, if we take the electromotive force at 2.20 volts we find for the work so given out $\frac{2.20^2}{1 \times 9.81} = .5$ kilogrammetres per second during one hour, or an absolute total of 1,800 kilogrammetres. And for the sake of comparison we may also say that the above battery would furnish a current of 2.2 webers for an hour. As Planté batteries may not be in the hands of everyone of your readers, and as I was fortunate enough to obtain an assorted supply before the scarcity set in, I will give a few figures which are the results of my experiments, and somewhat corroborate the above statements. The best cell that I could procure, and which had been nearly two years in formation at the makers in Paris, gave me, when properly charged, a current of twenty webers during five minutes. The two lead electrodes are each one millimetre in thickness and 65 millimetres long, by 20 centimetres deep. The amount of suboxide of lead which had been formed upon the positive electrode I found by drying and weighing to be 75 grammes. I will at once here make a comparison. In some of my round cells, having electrodes of the same size as the above, that is 65 x 20, I have placed upon the positive electrode 2,000 grammes of red lead (a similar quantity being also placed upon the *negative electrode*). The current which this arrangement furnished me was about equal to 20 webers during two hours and a half, or nearly proportionate to that furnished by the Planté battery, taking into account the relation $\frac{2000}{75}$ of lead oxide brought into action in both batteries. The least perfect of my Planté cells, which had been "formed" during three months only, gave me only about one-fourth of the above work. I state simply facts, but it is said that the above mentioned perfect Planté battery might have been made in three months instead of two years. Let it be so, and let us suppose that the Faure battery has no greater capacity of storage than three or four times that of some of the old Planté batteries in existence, still I beg to say that it exists, and is perfectly well covered by valid patents, and as such will be of great value to the electric industry. Upwards of twenty-five tons of Faure batteries have been made, and experiments on a commensurate scale carried out during a year of silence, and from trustworthy experimental work I have acquired the certitude that there are great things in it.

TESTS FOR COLOR-BLINDNESS.

A resolution received from the Ophthalmological Section, on the subject of the tests most applicable to be employed in working and observing signals by land or sea, where the lives of others are involved, was similarly carried unanimously, and the recommendations of the section ordered to be forwarded by the Hon. Secretary-General as the opinion of the Congress to the Foreign Secretary, the first Lord of the Admirality, and the President of the Board of Trade.

(Medical Congress, London, 1881.)

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING SEPT. 17, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

SEPTEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 11	29.891	29.912	9 a. m.	29.848	5 p. m.	71.0	67.3	78	4 p. m.	71	6 p. m.	64	7 a. m.	63	7 a. m.	131.
Monday, 12	29.901	29.958	12 p. m.	29.838	4 a. m.	67.6	61.0	77	4 p. m.	63	4 p. m.	61	6 a. m.	60	6 a. m.	141.
Tuesday, 13	29.979	30.032	12 p. m.	29.938	4 p. m.	70.3	63.0	79	3 p. m.	69	5 p. m.	58	6 a. m.	57	6 a. m.	134.
Wednesday, 14	30.113	30.154	12 p. m.	30.032	0 a. m.	66.3	61.6	76	3 p. m.	67	5 p. m.	58	6 a. m.	57	6 a. m.	132.
Thursday, 15	30.179	30.198	9 a. m.	30.144	3 p. m.	68.3	65.0	74	2 p. m.	69	2 p. m.	63	4 a. m.	60	4 a. m.	130.
Friday, 16	30.228	30.288	10 p. m.	30.162	4 a. m.	65.3	62.7	70	2 p. m.	65	2 p. m.	61	12 p. m.	60	12 p. m.	90.
Saturday, 17	30.275	30.292	9 a. m.	30.228	12 p. m.	63.3	60.3	70	4 p. m.	64	5 p. m.	56	12 p. m.	56	12 p. m.	131.

Mean for the week .... 30.080 inches.
Maximum for the week at 9 a. m., Sept. 17th .... 30.292 "
Minimum " at 4 a. m., Sept. 12th .... 29.838 "
Range .... .454 "

	Dry.		Wet.
Mean for the week	67.4 degrees		62.9 degrees.
Maximum for the week, at 3 pm. 13th	79. "	at 6 pm 11th,	71. "
Minimum " " 12 pm. 17th	56. "	at 12 pm 17th,	56. "
Range " "	23. "		15. "

SEPTEMBER	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE.
Sunday, 11	n. n. e.	w. n.w.	s.	129	4¼	0.30 am	.562	.614	.693	94	63	85	10	4 cu.	10	10½ pm	12 pm	1.30	.04	8
Monday, 12	n. n. e.	w. n.w.	n.	111	1½	2.30 pm	.505	.516	.457	94	83	69	10	1 cir.	0	0 am	5.30 am	5.30	.62	1
Tuesday, 13	n. n. w.	s. s. w	n. e.	82	1⅓	11.20 pm	.416	.443	.586	72	46	80	0	3 cir. cu.	0	-----	-----	-----	--	0
Wednesday, 14	n. e.	e.	e.	147	1¼	3.50 pm	.426	.497	.549	82	59	89	0	1 cir. s.	0	-----	-----	-----	--	7
Thursday, 15	e.	s. e.	e.	252	8	2.30 pm	.516	.641	.569	83	76	89	1 cir.	2 cir. cu.	10	-----	-----	-----	--	6
Friday, 16	e.	e. s. e.	e.	249	5½	1.30 pm	.542	.550	.510	94	75	88	10	9 cu.	9 cu.	0 am 11 am	8 am 0.15 pm	8.00 1.15	.12 .01	5
Saturday, 17	n. n. e.	s. e.	e. s. e.	153	1½	0.00 am	.473	.495	.487	88	70	94	7 cu.	8 cu.	9 cu.	-----	-----	-----	--	0

Distance traveled during the week .... 1123 miles.
Maximum force .... 8 lbs.

Total amount of water for the week .... .79 inch.
Duration of rain .... 16 hours, 15 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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JOHN MICHELS, Editor.

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TO OUR ENGLISH READERS.

We have received from Messrs. Deacon & Co., of 150 Leadenhall street, London, England, a standing order for a large supply of "Science," which will be forwarded weekly. We shall be obliged if our English readers will make this fact known to their friends.

The death of President James A. Garfield is regretted by the nation as a great national loss; but all friends of progress and those who desire to elevate the indifferent and ignorant to a higher grade of civilization, will mourn his sudden death as a calamity; for he was a living example of the wonderful power of education to raise a man from a humble position in society to a post of high honor and usefulness, developing powers which not only opened up a bright and brilliant career, but brought a peaceful and hopefnl serenity to his mind which was evident to all who enjoyed his society.

A NEW COMET.

Mr. E. E. Barnard, of Nashville, Tennessee, announced to the Smithsonian Institute, on the 21st instant, the discovery of a comet by him on the 20th, at two o'clock A. M., Washington mean time, in seven hours forty-six minutes right ascension, and thirteen degrees twenty-eight minutes north declension, with a daily motion of three degrees northeast.

On the 23d instant Professor Lewis Swift, of Rochester, made the following announcement in regard to this comet:

The position of Barnard's comet, as telegraphed from Washington, is so widely erroneous that nobody would be able to find it. Instead of being in cancer and having been discovered at two o'clock in the morning, it was near zeta virginis, low down in the Western horizon, and can be seen but a few minutes. It was discovered on the evening of the 19th, and at 7h. 46m., Washington mean time, of the 20th, was in right ascension 13h. 28m. 2s., declination north 3 deg. 47 min., with a daily motion of 3 degrees northeast.

In consequence of smoke I have not been able to find it.

We trust in our next issue to offer some explanation of these contradictory statements.

One of the most interesting and valuable reports that has been issued by the Board of Education at Washington, is that recently printed, which describes the opportunities for instruction in Chemistry and Physics which at present exists in the United States, together with statistical tables relating to this subject.*

The Department was fortunate in securing the services of Professor F. W. Clarke, Professor of Chemistry and Physics in the University of Cincinnati, to draw up this report, based on the mass of facts and figures bearing on this matter, which had been collected in reply to circulars issued by the Commissioner of Education towards the close of the year 1878. Professor Clarke appears to possess both executive and literary ability of a high order, and being himself a chemist and a teacher of science, was clearly in a position to do justice to the excellent intentions of Commissioner Eaton. We congratulate Professor Clarke on his success in compiling the technical part of his report, and we propose, on this occasion, to refer to some of his critical remarks aud suggestions, which, in scientific circles, will be considered the most valuable result of this investigation.

Before discussing the condition of scientific instruction in public schools, it may be well to consider first, at what age such instruction shall be commenced, and whether it should be considered as a part of primary education, or be reserved for high schools and universities, where special courses of training in the various branches can be advantageously advanced.

Professor Clarke claims that oral instruction in chemistry and physics can be made intelligible to children of ten years of age. He admits, however, that there is a tendency towards over-cramming the lower schools with a too great variety of subjects, which lead to results which are undesirable. He therefore suggests a compromise, and proposes, that in primary schools a taste for science should be cultivated among children "through the medium of the reading books, which might properly contain some short extracts relating to natural science." This plan Professor Clarke considers would be beneficial, and could not be injurious.

We can find no objection to such a course, provided a suitable reading book be written for the purpose, but before any discussion can be made as to the propriety of teaching the sciences in any form in the primary schools, a more thorough reform in the

*Circulars of Information of the Bureau of Education No. 6. 1881.

A report on the teaching of Chemistry and Physics in the United States, by Frank Wigglesworth Clarke, S. B., Professor of Chemistry and Physics in the University of Cincinnati. Washington, 1881.

whole system of public school education must be made. It appears superfluous to urge the teaching of science to children of ten years of age in public schools, where the elder scholars of fourteen and fifteen years are unable to read aloud intelligently or write an ordinary letter legibly. And yet, to our knowledge, such is the situation of too many of the scholars in our public schools in regard to these fundamental branches of education.

Professor Clarke's report shows that in high schools and academies the teaching of Chemistry and Physics varies between widely separated limits; in the majority of cases mere text book work is done, and only a few experiments performed by the teacher.

In our opinion the report offers excellent advice in regard to the teaching of Chemistry and Physics to such classes of students.

The following extract will explain Professor Clarke's views:

"That chemistry and physics are desirable branches to teach in schools of the grade now under discussion is pretty generally admitted, although a few educators still hold that such studies are fit only for technological institutes and colleges. But the greater number of pupils cannot go out into these higher grades, and must therefore either study the sciences now or do without them altogether. The latter alternative is clearly the wrong one to choose; at least, if we admit that education is anything other than a mere system of mental gymnastics. If subjects are to be learned quite independently of their relations to active life, then there is no ground for present argument; but if culture and utility are both to be considered we must recognize that some scientific training is indispensable. Nearly every pupil goes out of school into one of the great industries; and, whether he becomes a mechanic, manufacturer, railroad man, telegraph operator, farmer, miner, or tradesman, he is likely to encounter practical applications of the two sciences. In every avocation some knowledge of either physics or chemistry is almost certain to be directly useful; and this utility is often so great that the schools can better afford to err on the side of over-thorough teaching than in the opposite direction."

In answer to the inquiry how far these sciences can be carried in such schools without detriment to other interests, the report states:

"One high school has three years and another four years in its total course of study; the latter is plainly able to give more time to any particular subject than the former, Every variation in the character of a school must involve corresponding variations in the treatment of these two sciences. It may be safe to put half an academic year as the minimum time assignable to either subject. A year can usually be given to each without difficulty."

While as to the detail of such instruction Professor Clarke says:

"Instruction should be general rather than special. The attempt is too often made to teach applied science when there are no foundations of science to apply. Such foundations should be thoroughly laid in the high schools and academies, so that the pupil who passes on to a university or polytechnic course may have a genuine preparation for advanced work. Fundamental ideas, like those of the conservation of energy, the correlation of forces, the conceptions of atoms and molecules, &c., ought to be clearly inculcated. The scholar should be made to realize that each science is a coherent whole with definite relations to other sciences, that all its parts are vitally connected, and that certain general principles are universally applicable in all of its branches. In chemistry it is better to concentrate all efforts upon the inorganic portion of the science, leaving the complicated organic side for more advanced study. Along with the merely descriptive work should go a solid drill in chemical problems and chemical notation. Experiments made before classes ought to bear as far as possible upon main questions, and unavoidable details should be handled so as to illustrate clearly the great central ideas. When these have been fairly grasped, the scholar has gained something of both practical and intellectual value. His studies will have brought him not knowledge only, but also increased power."

For success, much depends on the teacher.

"He must have a vivid sense of what needs to be accomplished and enough special knowledge to render him in a measure independent of text books."

Text books, the report says:

"May be useful or injurious, according to circumstances. If they have been chosen by an average school committee, influenced by some publisher more energetic than his rivals, they are likely to be worthless, and the teacher must be prepared to make good their omissions and correct their blunders. No text book can be taken as sole guide and followed without variation; but a good treatise upon either science, prepared, not by a professional school-book maker, but by a trained specialist, may be of great help to teacher and pupils."

Professor Clarke wisely urges the value of laboratory work:

"In addition to classroom drill, laboratory practice should be an essential and prominent feature of every chemical or physical course. In the recitation or lecture, general principles are taught; in the laboratory, the student becomes familiar with methods and details. Three months of laboratory work will give more real insight into any science than a whole year's study of the printed page. To study chemistry from books alone is like learning a language from its grammar only, without attempting to translate or to write exercises. The pupil must learn to observe and to experiment for himself, in order to acquire any clear scientific knowledge."

One recommendation of the report is strongly in accord with our own views, and that relates to the advice to teachers and pupils, to construct as far as is practicable all apparatus used in the laboratory.

"The apparatus which a teacher contrives for himself

with the aid of his scholars is oftentimes the most useful for purposes of instruction. Many and many a school has invested in trifling electrical playthings a sum of money which would have gone far towards the establishment of a simple working laboratory.

"In physics the laboratory practice must needs be somewhat limited. The pupils may handle whatever apparatus happens to be available, learn its manipulation, and assist the teacher in the construction of simple appliances. The magnetization of needles, the electrolysis of liquids, the verification of the fixed points upon a thermometer, and rough determinations of specific gravity, boiling point, and melting point are among the many experiments which ought always to be possible.

"In the chemical laboratory a much greater variety of work is easily attainable. There are the ordinary experiments in manipulation, such as the bending of glass tubes, filtration, precipitation, distillation, &c., the preparation of the commoner gases, acids, and salts; the verification of the more obvious properties of the chemical elements; and lastly, the simpler reactions of qualitative analysis. To the last named subject some time may always be profitably assigned. No other class of exercises will do so much towards impressing the average beginner or towards making him realize the nature of chemical reasoning. At every step it calls his powers of judgment into play. It involves the use of no costly apparatus, and enough can be done for all school purposes with a very moderate supply of the cheaper chemicals. At an expense of a hundred dollars a year a great deal can be accomplished; and an outlay of only one-fifth of that sum may yield results which are by no means to be reckoned as trivial. Again let it be said that success depends upon the teacher and not upon the cost of materials."

We shall in our next issue continue our notice of this interesting report.

ON COMETARY APPEARANCES.[1]

By M. Jamin.

[Translated from the French by the Marchioness Clara Lanza.]

The question of comets seems at present to occupy the attention of all *savants*, and as M. Faye has prevailed upon physicists to take up the subject also, I have decided to enter into the discussion, not with the intention of creating any novel hypothesis, but rather to oppose that which M. Faye imagines to be the correct one. In the first place, it appears to me unnecessary. It contradicts in my opinion, the theory of the vibration of the ether. Besides, it deprives the law of gravitation of its generality and simplicity. In his first work, M. Roche determined, by means of calculation, the form of the horizontal strata of cometary atmospheres subject to the sun's attraction, but he omitted to note the variations of temperature occasioned by the solar rays on the two sides of the comet. In this way he was led to think that the latter must have two tails, one turned towards the sun, the other away from it, which supposition is contrary to reality, as it should be, in fact, inasmuch as it overlooks the cause which manifestly determines the unsymmetrical forms of the two sides. In a second paper, however, he makes a correction, by supposing the existence of a repulsive force diminishing the solar attraction about 1—to 1. ϕ, φ, being a force acting unequally upon different substances, and which is in reverse ratio to their density. This hypothesis admits of the calculation being achieved with facility, but it has no physical actuality. It is confined to replacing the warmth of the cometary atmosphere, which should be included in the calculations, but which has been forgotten, with a wholly imaginary action whose existence no experiments have ever confirmed. I shall endeavor to re-establish the effect due to the unequal warmth of the two sides by referring to analogous conditions which should exist between the Earth and comets.

Upon the Earth, every day throughout the year, the solar rays one after the other in regular succession strike normally all the points of a circle perpendicular to the axis of rotation and near the equator. These points on all portions of the globe are those that receive the maximum of heat at noon. They constitute what is termed the *ring of aspiration*. The air there really becomes rarified and ascends, advancing towards the north or the south, as the case may be, determining two gaseous currents called trade winds. These currents are permanent and regular; they come from temperate climates, grow warmed progressively in their course, carry with them an intense evaporation, are slightly deviated towards the west in consequence of the Earth's rotation, and finally meet obliquely upon the ring, to rise to the highest atmospheric limits. There they spread, then taking a contrary course, return, one towards the north, the other towards the south. These are the *counter trade winds*. There are, therefore, on the two sides of the ring of aspiration, two closed atmospheric currents completely enveloping the globe, coming cold from the poles, grazing the Earth, and then returning warm, by a higher route. There is no occasion to dwell upon the chief *rôle* played by this circulation. It is sufficient to merely demonstrate its necessity, its constancy and its extent, besides recalling the theory due to the famous Halley, which has never been contested.

This circulation would still exist although under changed conditions, if the Earth instead of turning on its axis always presented the same side to the Sun. The ring of aspiration would be reduced to a single point, the trade winds would converge in all directions, while the counter winds would diverge in the same way. All points of the Earth would send to this summit cold air which would grow warm there, *rise in the form of a cone toward the Sun, spread, become bent upon the edges like the chalice of a cyrathiform flower*, leave the Sun by a high route and after a more or less prolonged journey would return to the point of departure grazing the Earth's surface. It is very evident that this double movement would possess an increase of force proportionate to the Earth's approach to the Sun; that its atmosphere would be more extended, and that there would be a greater mass of water to be evaporated. This does not imply any particular repulsive force.

But let us get to the comets. In the long journey which they perform slowly until they are beyond the Solar world, they have plenty of time to lose all the heat received from the Sun, and to efface all traces of perturbation. The tail disappears, the matter is knit together by its own attraction and assumes a nebulous, spherical form. In the centre are the dense, solid substances, the nucleus, then the liquids and finally the gases. An enormous atmosphere and a very small nucleus. In Donati's comet the nucleus measured 1600 km., while the atmosphere was 20,000 km. The comet of 1881 was still more extraordinary. Its aureole measured 2,000,000 km., the nucleus was reduced to 680. This is just contrary to the Earth whose diameter amounts to 12,000 km., while its atmosphere is merely a thin pellicle of 18 or 20 leagues. Comets are so constituted that the most tremendous atmospheric movements are developed be-

[1] See *Comptes Rendus*, August 16, 1881.

neath the Solar action incomparably more accentuated than those exhibited by the Earth.

As no rotatory motion has ever been observed in comets or their atmospheres, we feel authorized in saying that if it does exist it is exceedingly slow, without taking into account the fact that comets always present the same side to the Sun. The second method of heating should therefore be produced. In every plain passing through the centre of the Sun and the nucleus, there will be a double atmospheric circulation. On the interior, the comets advance towards the Sun as though the gravitation there was intensified. On the exterior, they deviate as if the gravitation was diminished, or rather as though there existed some repulsive force emanating from the Sun, affecting the exterior surface of the cometary atmosphere, and acting solely upon it. In reality, this repulsive force does not exist. It seems as though it did, however, and under conditions analagous to those inferred by M. Faye. All the consequences therefore, which he deduced in order to explain the formation of the tails, are developed naturally. There is nothing here to be altered.

I do not think, however, that this theory is sufficient to account for cometary appearances. On the contrary, it is my opinion that electricity has a great deal to do with them. But before entering upon this let us first return to terrestial phenomena.

It has been satisfactorily proved that considerable electricity exists in atmospheric altitudes, and that it increases according to the height. It is admitted generally that atmospheric motion results; that it is developed by evaporation at the ring of aspiration; that it moves from the time it leaves this ring until it reaches the poles under the form of two currents in the rarified air which it illumines. Towards the sun it is the zodiacal light, invisible when close to this planet, but extending a sufficient distance to be perceived, especially near the equator. Close to the poles it is the aurora borealis, which we see obliquely and which appears more luminous than at the zenith, because it has greater density and is more concentrated.

Upon a comet the warmth occurs at the point where the trade winds come together opposite to the sun. But analogous electric actions should be manifested, illuminate the head and produce the appearance of effluviums succeeding each other like the stratifications in a Geissler tube, accompany the counter trade winds to the opposite side to illumine the tail, and be prolonged to a great distance like the luminous rays in Mr. Crookes's apparatuses. No doubt, matter would be contained in the tail, but rarified to an extreme degree and made visible by both the solar light and the electric current.

M. Flammarion would be quite right then to attribute this shining to electricity. On the other hand, M. Berthelot's observation would be justified, and the development of this electricity would be due to the phenomena of evaporation and movement situated in the atmosphere. We must insist upon this point.

The recent study of cometary spectra has shown us beyond the possibility of a doubt that the interior aureole and the tail contain carburetted gases which emit a light of their own. Now, they can only become luminous in two ways; either by combustion or by an electric effluvium. If by combustion, we have yet to explain how they take fire and how they continue to burn indefinitely, which seems very difficult. For in this case, all the materials of which the comet is composed would be red, and the spectrum would contain the bright spectral rays of the metals as we see them in the electric arc burning n mid air. Nothing of this kind occurs. The light is absolutely like that of the arc when the vaporous carbon is transported to the torpid gases without burning. It shows no brilliant metallic bands, any more than this arc. The light, therefore, cannot be the result of fire, but is due to illumination made by the currents.

I think that the Sun determines gaseous currents in cometary atmospheres analogous to terrestrial trade winds and counter trade winds; that this circulation produces near the Sun effluviums arising from the head of the nucleus and transports to the opposite side the substances which are on the exterior, producing upon these substances the effect of a repulsive force emanating from the Sun, a force which has absolutely no ***raison d'ètre***. Besides this, I think this circulation is accompanied by an electric movement which illumines the gases either towards the head or tail, as the case may be, making them visible to us notwithstanding the feebleness of their density, and precisely on account of this feebleness.

AMYLOSE: ITS CONSTITUENTS AND METHODS FOR THEIR ESTIMATION.

By H. W. Wiley, Lafayette, Indiana.

I propose the name Amylose for all the varieties of sugar and sugar-like substances derived from starch.

These substances are now known by many different appellations, and often the indiscriminate use of these terms gives rise to a great deal of misunderstanding and confusion. Among them I may mention grape sugar, starch sugar, dextrose, dextrine, glucose, maltose, fruit sugar, etc. These names do not always have the same signification in different localities. For instance, glucose and dextrose, in Europe, signify the same product, while in this country they embrace many other substances besides.

If we designate the starch sugar in general by amylose then the terms glucose, dextrose and maltose can be used to designate certain definite constituents of amylose.

Amylose is composed of three principal ingredients.

1st. Dextrine. Pure dextrine is very difficult to obtain. It is obtained almost pure by the dry roasting of starch. The temperature during torrefaction must not be carried too high, 210°–275°. Starch itself has a specific rotatory power of 214° (1). Bondonneau (*loc. cit.*), asserts that there are three dextrines, (*a*), in which aj (2) = 186°; (*β*), in which aj = 176°; (*γ*), in which aj = 164°. According to Musculus and Grubber (3), there are five dextrines; viz.: (*a*), soluble starch colored wine red by iodine aj = 218; (*b*), Erythro dextrine, red color with iodine; rotating power not given.

(*c*), *a* Achroodextrine, not colored by iodine, j = 210.

(*d*), *β* Achroodextrine, aj = 190.

(*e*), *γ* Achroodextrine, aj = 150.

Of these varieties the first and second do not reduce the alkaline copper solutions while the others do. If the reducing power of dextrose be taken at 100, that of the third of the above dextrines will be 12; the fourth 12 and the fifth 28.

O'Sullivan admits the existence of but one dextrine with a = 214.

Thomson (4) tries to show by history of multiples in the rotating power of the carbo-hydrates that there are at least three dextrines in which the value of aj is 186, 176 and 164 respectively.

I will not multiply authorities concerning the rotating power of dextrine. I have quoted enough to show the highly chaotic state of our knowledge on the subject.

The chemical properties of dextrine are equally as undeterminable.

Gentele (5) is quite confident that dextrine will reduce the alkaline copper solutions and on this he bases his method of separating dextrine from other reducing substances by ferricyanide of potassium.

Stommer (6), Bondonneau (7) and Rumpff (8), are equally

(1) Bondonneau, Ber. d. Deu. Chem. Gesel., IX, 69.
(2) a = specific rotatory power.
(3) Comptes Rendus, LXXXVI, 1459.
(4) Ber. d. Deutsch. Chem. Gesel., 14—2—158.
(5) Ding. Journal, CLII, p. 139.
(6) " " CLVIII, p. 40.
(7) Bull. de la Soc. Chim., 1874, XXI, p. 50.
(8) Zeit. fur Anal. Chem., 1870, p. 358.

positive that dextrin does not reduce the copper solution except on prolonged boiling.

My own experience is that dextrine does not reduce Fehling's solution except on prolonged boiling and then by the dextrine being slowly converted into dextrose.

To secure the reduction of the dextrose and the reducible portion of maltose it is not necessary to boil the solution more than two minutes, and during this time the amount of dextrine reduced is wholly inappreciable.

Color with Iodine. Bondonneau ([9]) says, dextrine colors a solution of iodine a dark red, but that it is without effect on caustic soda. Musculus & Gruber ([9]), found that some varieties of dextrine colored iodine solution red, while others were without action on it.

In general it will be found that most substances purporting to be dextrine give a reddish color with iodine. For my part I do not know of any method by which absolutely pure dextrine can be prepared.

Fermentability. Respecting this property the most contradictory statements are found. The weight of authority leans to the non-fermentible doctrine. It is probable that before dextrine ferments it is first converted into dextrose.

Formation. In addition to the method by roasting, dextrine is always formed when diastase or dilute acids act on starch.

Some authorities maintain that dextrine in such cases is always the first transformation product and that the others are derived from it.

According to Musculus ([10]), dextrose and dextrin are formed synchronously by the action of sulphuric acid on starch and in the proportions of one part of the former to two of the latter. This proportion is maintained until all the starch disappears. The further action of the acid then tends to convert the dextrin into dextrose. O'Sullivan ([11]) states that by the action of diastase, both dextrine and maltose, and nothing else, are first produced from starch under certain conditions of temperature (below 63°), and in the proportions of one part of the former to two of the latter. The proportion, however, only obtains when the specific rotatory power of the mixed bodies is equal to 171°.

From a careful study of all the data I could obtain I concluded that very little is yet known of the real proportions of dextrine which amyloses formed by heating starch with dilute acids contain.

MALTOSE. Dubranfaut ([12]) first pointed out the probable existence of a third transformation product of starch in addition to the two which had long been known. But we must accord to O'Sullivan ([13]) the merit of having first isolated and studied the properties of this body. He has given in the *Journal of the Chemical Society*, within the last decade several papers on the properties of this important saccharide. He gives its specific rotatory power (a)= 150°. The same value is also given by Musculus and Gruber, ([14]),

Joshida[15] found a slightly higher number. For (a)j this would give 135.°36.

Maltose is formed chiefly by the action of diastase on starch at a temperature not exceeding 75°. But it is also formed, but in smaller quantities by the action of dilute acids.

It is well established that maltose has the power of reducing the alkaline copper solution and in the proportion of 65 to 100 compared with dextrose. We here find an easy explanation of the fact that so many chemists have affirmed that dextrine acted on copper solutions. In all these cases the dextrine doubtless contained maltose. Without citing further from the literature of maltose, which is all recent, I wish to call your attention again to the numbers representing its rotating and reducing power, viz. 150 and 65.

3d. DEXTROSE. This substance is the final product of a complete saccharification of starch. It has a slightly bitter taste, which is probably due to the development of a bitter principle on long boiling with acids. This bitter taste is not noticed in the products less perfectly converted.

Dextrose possesses in the highest degree the power of reducing the copper solutions. One gramme reduces 2.205 g. weighed as cupric oxide. Its specific rotatory power has been the subject of much controversy.

O'Sullivan gives 57°.6 = (a)
Tollens " 56 = "
" " 53.17 = (a) j. ([16]).

The differences in results which the above numbers show do not indicate so much errors of observation as they do the impurities which the purest dextrose is likely to contain. As a mean of these numbers, we may take (a) = 56 and (a) j. = 50.5 (by calculation).

Pure dextrose is almost insoluble in absolute alcohol, while it is very soluble in water. According to Anthon ([17]) 100 parts water dissolve 81.68 parts dextrose. To dissolve one part, 50 parts of alcohol, .83 sp. gr. are required. When subjected to fermentation, dextrose affords 48 per cent alcohol ([18]), while cane sugar and maltose each gives 51 per cent and dextrine none at all.

I have thought it useful to give the above brief *resume* of the literature of amylose, because the conclusions to be drawn from it will go far to explain the anomalies of the numbers which the following analyses and methods of analysis will show. The whole subject is a matter of considerable scientific interest on account of the immense production of amylose in this country and the laws which some of the States have passed to regulate its sale. In the present state of our knowledge I am at a loss to see how the real constitution of an amylose, or mixed sugar, can be established before a court of justice. To show this I will give synopses of a few of the

METHODS OF ANALYSIS

which have been proposed.

I shall not attempt to give an out-line of all the methods which have been proposed for determining the amount of dextrine and dextrose in amylose. Until within a few years they were all based upon the assumption that these were the only transformation products of starch—an assumption which we know to be false. Reduced to first principles, all the methods may be comprised under three heads.

1st. The reduction of certain metallic salts by the dextrose, and estimation of dextrine by difference.

2d. Fermentation of dextrose and estimation of dextrine by difference.

3rd. Precipitation of the dextrine by strong alcohol and estimation of the dextrose by difference.

Remembering the facts established in the first part of this paper, it will not be hard to show the fallacies of these several methods.

1st. Metallic salts, especially the compounds of copper, mercury and ferro-cyonigen, are all reduced by maltose as well as by dextrose, and on prolonged boiling in a slight degree by dextrine also. Thus the total reducing effect does not measure the amount of dextrose present only in case maltose is completely absent. In commercial amyloses this is never the case unless it be in rare instances of high pressure conversion.

2d. Fermentation not only converts dextrose into alcohol and carbonic dioxide, but acts in the same manner on maltose. For a given weight maltose gives even

([9]) Loc. cit.
([10]) Annal. de Chem. et Phys., p. 203.
([11]) *Chemical News*, 1876, 861-218.
(12). Anall. de Chem. et Phys. (3) XXI, p. 168.
(13). Jour. Chem. Soc. Vol. X, 2d series.
(14). Loc. cit.
(15). Chem. News, 1881.

(16). Ber. d. Deutsh Chem. Gesel., 1876, p. 420.
(17). (*Ding. Journal*, CLV, p. 41).
(18). O'Sullivan loc. cit.

a higher percentage of alcohol than dextrose, and in the proportion of 51 to 48. Whether dextrine is fermentible or not has long been a subject of bitter discussion. The weight of authority seems to be in favor of its fermentability by a slow conversion into dextrose. Thus by fermentation we are not sure of getting the amount of dextrose present even when maltose is absent. From this it appears that the process of fermentation is likely to give less reliable results than reduction affords.

3d. Precipitation with alcohol is even less reliable than the method just given. Alcohol of 90 per cent ceases to give a precipitate long before the dextrine is all converted into dextrose, as every grape sugar maker well knows. If absolute alcohol is used, dextrose is also precipitated.

Anthon has shown[19] that fifty parts of alcohol of .83 sp. gr. will only dissolve 1 part of dextrose. Anything like an accurate separation, therefore, by this method is impossible.

As an illustration of the variations in the composition of different specimens of amylose I will cite the following analyses by Steiner[20]:

	I.	II.	III.	IV.
Water	15.50	6.00	13.30	7.60
Ash	.30	2.50	.40	1.10
Dextrose	45.40	26.50	76.00	...
Dextrine	9.30	15.90	....	39 80
Maltose	28.00	40.30	5.00	42.60
Carbo-hydrates	1.50	7.00	5.30	8.90

My own analyses have given results quite as puzzling as the above, although I have never been able to satisfy myself with such exact expressions of percents.

I have heretofore been glad when, by hard work and liberal guessing, I could come any way near the truth. The author of the above table leaves us in charming ignorance of the methods by which such accurate percents were obtained; at least, of methods which would stand the test of criticisms, while the bunching of all the unknowables as *carbo hydrates* is quite worthy of the reasoning of the Concord School of Philosophy. I have given enough, I think, to show the untrustworthiness of methods now in use, and of the results obtained by them. I am sorry that I have nothing very much, if any better, to propose as a substitute. What I have been able to accomplish I will now briefly describe.

THE ANALYSIS OF AMYLOSE.

Water. I have estimated water in a flat platinum dish. Only two or three grammes should be taken, though in many of my analyses I have used more. This dish is placed in a second one, and this in a paraffine bath heated to 150°–170°. The object of the second dish is to keep the wax from touching the dish which is to be weighed. After two hours the weight is sensibly constant, and the whole mass is quite brown. I believe the method will give the water to within one-half of one per cent.

Ash. There is only one method of determining the ash, *i. e.*, by incineration in a shallow platinum dish in a muffle. The per cent of ash in a strait amylose is extremely small. In most cases its quantity may be neglected as far as practical purposes are concerned. The determination of the ash is chiefly useful to furnish a clue to the purity of the sample.

Reducing Matter. I determine by Fehling's solution. It will be found most convenient to take 10 *g.* in 1000 *c. c.* In all cases the volumes of the solutions employed should be as nearly the same as possible.

Rotating Power is determined by using 10 *g.* of the amylose in 100 *c. c.* If 26,048 *g.* are taken the weight of cane sugar, which gives 100 divisions on most polariscopes, the rotation is so great that the neutral point is thrown entirely beyond the graduation. In many cases of high conversion, however, this would not be the case. If the solution is turbid it must be clarified by blood charcoal, or plumbic acetate. These substances, as I have shown ([21]), tend to diminish the rotating power. The clearer definition, however, of the neutral point in part compensates for this loss in gyratory power.

In the light of the foregoing *resumé* it is possible to explain the results of my own work, although I am far from thinking that anything better than approximate per cents can yet be obtained.

We have seen that an ordinary amylose, whether liquid or solid, contains about 86 per cent of material not water. One per cent, nearly, of this is ash and optically non-active matter. Ten grammes of amylose, therefore, will contain an average of 8.5 grammes optically active matter. If this were all dextrose it would give in 100 *c.c.* an angular rotation of 8°.67. This is obtained by the formula $a = \frac{\theta \times v}{\lambda \times w.}$

Here a_j. = sp. rot. power for yellow ray.
θ = angular rotation.
V = volume of solution in *c. c.*
λ = length of observation tube.
W = weight of substance in grammes.

In a large number of cases where I heated the amylose with dilute sulphuric acid from 4 to 6 hours, I obtained an average value of θ= 8°.85. This shows that prolonged boiling does not convert all the amylose into homogeneous dextrose.

If the substance under examination were all maltose the value of a_j would be 135.36 and θ would become 23° nearly. The highest number I ever obtained for an amylose was 22° .24. This shows that even this specimen, with such a high rotating power contained some matter in a weaker degree of optical activity.

Finally, if all the snbstances present were dextrine, I would not be able to tell theoretically what its rotating power would be, since we have just seen that dextrine is assigned different degrees of activity by different authors. As a mean of these I think we may place dextrine a_j =176°, although I do not wish to be understood as stating its real value.

This would give a total angular deflection of 29° nearly.

The problem of analysis is therefore at the present time in the following *status*:

1. In every amylose there are present at least three kinds of optically active matter, viz., dextrose, maltose and dextrine.
2. There are present in every amylose two kinds of reducing matter, viz., dextrose and maltose.
3. A high reducing power shows a high percentage of dextrose present.
4. A high rotating power, which is always shown when the reducing power is low, indicates a large percentage of maltose and dextrine.
5. From a very extended series of analyses, I will say that there is no method known which will give reliable, or rather exact, numbers for the percentage of the different constituents.
6. I propose to attempt the accomplishment of this very desirable result by first polarizing and then reducing the sample, and then repolarizing the residue. The difficulties of preserving a standard volume and of getting a solution sufficiently clear for polarization have prevented me hitherto from obtaining any results. I hope to overcome these troubles and to establish thereby a reliable optical method of determining the percentages of dextrose, maltose and dextrine in amylose.

Prof. Cantoni has been appointed director of the meteorological observatory, to be erected at Pavia. Observations are to be made on the influences of light, heat, and electricity upon vegatable growth, in addition to the ordinary meteorological and magnetical work.

[19] Ding. Jour., CLV., p. 41.

[20] Zeitsch. f. d. Gesch. Brauwesen, 1879, No. 11, p. 339.

(21). (These proceedings Vol. 28, p. 317.)

A REMARKABLE INVASION OF NORTHERN NEW YORK, BY A PYRALID INSECT:*

Crambus vulgivagellus.

(Abstract.)

By J. A. Lintner.

About the middle of May, of the present year (1881), a serious invasion of St. Lawrence county, N. Y., and several of the adjoining counties, by the "Army Worm," was announced by the newspapers, and by letters addressed to me. It was stated that many pastures had been completely ruined, and the entire destruction of the pastures and meadows was feared. I had never witnessed the operation of the Army Worm—comparatively rare in the State of New York—and I at once visited the infested locality for personal observations.

The reports had not been exaggerated. The ravages were widespread and serious, already extending over eight of the northern counties. Hundreds of acres of grass presented a brown appearance, as if the grass had been winter-killed. A pasture lot of fifty acres, examined by me, which ten days before offered good pasturage, was now entirely brown from the complete destruction of the grass—so thorough, that in places not a blade could be discerned in an area of a square yard, by careful search. Numerous dead caterpillars were adhering to the dried stems of last year's grass, which, it is believed, had fallen victims to starvation.

Several interesting features characterized this attack. It appeared first on upland pastures, differing in this respect from the invasions of the Army Worm, *Leucania unipuncta.*

The progress was remarkably rapid. A browned patch would rapidly extend its area, until it overspread the entire field in ten or twelve days. It could not be ascertained if this was by the spread of the larvæ from certain points, or from the unequal hatching of the eggs, uniformly distributed over the field, as influenced by various conditions. The secresy of the depredation was unusual. The larvæ had seldom been seen and never observed in active feeding. It was believed that they fed at night, by drawing in the blades of grass to their subterranean retreats.

In two instances the larvæ were observed in immense numbers, collected on trunks of trees—so numerous that they could have been scooped up by handsfull. One of the reported localities was visited by me where the assemblage had been noticed three days before. The tree-trunk, at its base, was found to be enveloped by a web of silk, as was also an adjoining stump, of so firm a consistence that it could be lifted up in a sheet, like a piece of woven silk. The cause of the congregation at this point could only be conjectured, but it was thought it might have been for shade, after the complete destruction of the surrounding pastures. It was not for feeding on the foliage, for the grasses were alone eaten by the larvæ.

It was generally accepted throughout the entire region as an army-worm invasion, and the most disastrous consequences apprehended. The papers abounded with notices of it. Farmers commenced to dispose of their cattle, in the prospect of their ruined pastures and meadows. It became the one topic of village conversation, and general alarm prevailed.

The caterpillars observed and collected by me in Morley and Potsdam, by digging in the soil, and occasionally finding one on the surface, were slender, cylindrical, sixteen-footed, of a sordid or obscure greenish color, with a shiny black head. They were destitute of lines or other ornamentation than some verrucose spots on the dorsal portion. The average length was three-fourths of an inch.

I was unable to identify these with *Luecania unipuncta,* for they were quite unlike the mature form of that species which I had alone seen. Yet it was possible that they had additional moltings to undergo, which might result in a material change of appearance. Their habits seemed to be quite different from those of the army-worm, and it was nearly two months too early for an invasion of that species.

Of the larvæ which I had brought from Potsdam for rearing and ascertaining the species, nearly all died shortly thereafter. Only a single one developed, giving me a small Pyralid moth—*Crambus exsiccatus.* Additional ones were sent me, at my request, from Potsdam. They were quite different from those previously collected and observed by me, but it was believed by my correspondent, as the result of observations made, that the molting through which they had just passed had produced this change.

I suspected that two species were associated in the attack, but other pressing duties at the time prevented a decision upon this subject. Some of the examples received were submitted to Prof. Riley, who was able to identify them as the larvæ of a rather rare Noctuid—*Nephelodes violans,* which he had known in Missouri. The occurrence of the species in such numbers—more than a dozen in lifting a small piece of a rail—was an interesting discovery. In some communications contributed by me to some of the newspapers of Albany and Northern New York, I ascribed the above ravages to *Nephelodes violans.* Farther study led me to believe that I had been hasty in my reference.

Early in July, Mr. J. Q. Adams, of Watertown, N. Y., where the ravages of the same insect had also been observed, furnished me with information and material that convinced me that *N. violans* was only chargeable with a small portion of the above injuries, and that the principal depredator was the smaller larva observed and collected by me, which, from the cocoons forwarded to me at this time, undoubtedly belonged to the Pyralidæ. The cocoons were taken from the infested fields at Watertown, from just below the surface of the ground, where they were so numerous that a half dozen could be taken from a sod the size of a man's hand. On opening the cocoons, the larvæ were found lying within them, still unchanged, although they had been made over a month before, and they were identical with my Potsdam collections. Additional cocoons were opened by me early in July, when the larvæ were still in their untransformed state, in which they had at this time been remaining for from a month to a month and a half.

The delayed pupation is an interesting item in their history. It is known to occur in some of the Bombycidæ, among the Notodontas for example, when it extends over the winter, and the pupa state is assumed in the spring, a short time before the emergence of the perfect insect; but it was new to me at this season of the year.

Dr. Hagen, to whom I communicated the fact, was unable to find any record in the extensive library at Cambridge of such delayed pupation among the Pyralidæ, although Prof. Riley informs me that he had known of its occurrence in some of the species.

On the 6th of August, the first moth from the Watertown cocoons was disclosed, and it proved to be *Crambus vulgivagellus.* The interesting question as to which of our insect depredators was chargeable the ravages in Northern New York—more injurious in the extent of territory embraced than in an army worm invasion—was decided. The new enemy, the latest addition to our list of formidable insect pests, was found to be a modest, inconspicuous, hitherto unobtrusive little Crambus. It had long been known in our cabinets, but had never before presented itself as an injurious insect.

It is probable that several accounts of injuries to pasture lands, in New England States, during the last three or four years, which have been ascribed, either to the army-worm or an unknown depredator, are due to this

*Read before the A. A. A. S., Cincinnati, 1881.

species. In its subsequent appearance, hereafter, it may now be recognized.

The Crambidæ are small moths with narrow front wings, often marked with metallic spots and lines, which are frequently driven up for short staccato flights in our pastures and meadows during the fall months.

The paper concluded with a resumé of the history of the species, so far as known at present, which is omitted as not of general interest.

CAÑONS—THEIR CHARACTER AND ORIGIN.*

By Hon. William Bross.

To the professional geologist it may seem an impertinence for a layman to offer any opinions as to the character and the origin of cañons. He may, however, it is hoped, use his eyes without offense, and form such conclusions as the facts which he has observed, may appear to warrant. If they should not agree with the recognized principles of the science as now understood, he will be no worse off than scores of learned Professors in the past, for in this, as in almost every other science, nearly every conceivable absurdity was exhausted before theories were made to agree with acknowledged facts. And here, at the commencement, the conclusion to which the observations to be presented somewhat in detail have led, may as well be stated—viz.: that cañons were formed by some great convulsion of the earth's surface, or by the contraction of mountain chains from their igneous condition in the early history of the planet. Take, for instance, the cañon of the Saguenay—a vast fissure in the mountain chain that lies on the north side of and nearly parallel with the St. Lawrence. The fissure or cañon is some fifty or sixty miles long and lies nearly at right angles to the river. Something like a mile apart, the perpendicular rocks on the north side are, at some points, about 1,500 feet high, the water at their base being several hundred feet deep. No man in his senses, it seems to me, could possibly conceive that this gorge through the granite mountain could have been formed by the action of the insignificant river that empties into Ha-Ha Bay at the northern end of the cañon. The surface of the water, for the whole distance of sixty miles, is on a level with the St. Lawrence, in some places it is several hundred feet deep and the cañon is about a mile wide, through the solid granite rocks. And here another general principle may as well be stated, that, with a single exception, the width of this and the other cañons hereafter to be noticed, is scarcely ever more than a fraction of a mile; seldom a single mile—a fact that strongly indicates uniformity in their origin. And besides, the mountains on both sides are generally nearly of the same height.

TAKE THE CAÑON OF THE HUDSON,

where it passes through the Blue Ridge, above and below West Point. The channel is deep, the tide ebbing and flowing far upwards towards Albany; the mountains on both sides, though rounded off towards their summits, doubtless during the glacier period, are about of the same height, and there is a general correspondence in the dip and thickness of the vast strata of rocks on both sides of the river. With the exception that the cañon is far above tide water, the same general facts are witnessed in that of the Delaware at the water-gap through the same spur of the Allegheny Mountains. In this case there are two well-defined ledges corresponding with each other on both sides of the river; the water is deep and sluggish while passing through the gorge, and all the facts seem to point, with unerring certainty, to some great convulsion in Nature as the origin of the cañon. With the exception that the current of the Potomac is swift at Harper's Ferry, the break in the mountain there, so graphically described by Jefferson, is very similar to that of the Delaware. This gorge may not have been relatively as deep at its formation as those of the Hudson and the Delaware.

THE CAÑON OF THE NIAGARA

was confessedly formed by the action of the river; but, if the structure of the rocks forming the cañon between the falls and Lewistown be considered, the exception in this case, it is believed, will prove the rule enunciated at the beginning of this paper. The rocks underlying the country between Lewistown and Buffalo are nearly horizontal, and are, in round numbers, as indicated by the gorge below the falls, some 200 feet thick. The upper strata, for say half the distance, are solid limestone, underlaid for perhaps an unknown depth, by soft sandstone, scooped out with comparative ease by the great cataract. Hence, the support of the upper stratum of lime-rock is gradually worn away, and it falls into the gulf below. On the American side of Goat Island, where only a fraction of the river falls over the precipice, the lime-rock lies below in vast blocks, and a rapid is gradually forming, while on the Canada side, the immense river scoops out the sand-rock to a great depth, and the falling sections of the lime-rock are buried out of sight forever. Below the railway bridge, for a long distance, there is a terrible rapid, showing that some other rock at the bottom of the river was harder than the sandstone, or that the stream is partially damned up by the lime-rocks thrown down between the bridge and the present fall, forced to the position they now occupy by the water, débris, and ice pressing down from above as the river gradually receded towards Lake Erie. This recession will doubtless continue even back to Lake Erie, unless the sandstone dips deeper down into the earth, and the limestone strata become thicker or some other hard rock fills the entire face of the cataract. Then the fall would gradually wear away at the top and become a rapid of gigantic proportions. Now, if the Niagara River, with its vast volume of water at first falling over a lime-rock ledge, at Lewistown, underlaid by a friable sandstone base—a condition of things found, it is believed, in no other cañon upon the continent—has required untold ages to work its way up to its present location, how is it possible for the comparatively small rivers heretofore named, and those to follow, to wear away a pathway to the sea through great mountain ledges of the hardest rock? Such a conclusion would be absurd.

THE CAÑON OF THE MISSISSIPPI

extending, say from Dubuque to the head of Lake Pepin, some 200 miles or more, is an exception to the rule above proposed, mainly in its width, which is some five to seven miles. The sandstone bluffs on either side are generally perpendicular from the top downwards from 200 to 300 feet, when the débris slopes down to the bottom lands or to the majestic river as it sweeps through the alluvium from one side of this broad cañon to the other. There are doubtless good reasons for the opinion that the waters which now find their way from Lake Winnipeg to Hudson's Bay once flowed south and filled full the broad space between the beautiful bluffs of the Upper Mississippi.

THE GORGE OF THE UPPER MISSOURI,

situated about 100 miles below Fort Fenton, is one of the most marked, as it is one of the most beautiful, cañons on the continent. The walls are perpendicular, of white sandstone, scarcely a mile apart, and some eighty feet high. On the top of these walls there is a layer of clay, perhaps of the same thickness, rounded off gracefully by the winds and storms, while in some places it has been all worn away, and the tops of the white sandstone ledges appear as castellated forms, reminding one of the Milan Cathedral, or some of the old ruins

* Read before the A. A. A. S., Cincinnati, 1881.

scattered all over Europe. Between these sandstone walls the river flows smoothly, without giving the least suspicion that the cañon was formed by it. Only some great convulsion could have torn apart this immense sandstone deposit for some twenty or twenty-five miles. It will well repay a visit to the Upper Missouri to see it.

THE GRAND CAÑONS OF THE ARKANSAS,

the South Platte, Clear Creek and the Bowlder strongly resemble each other, and may, therefore, be disposed of in the same paragraph. Through the three first, in spite of the tremenduous obstacles they presented, railways have been built, and the saucy little locomotive rings out the echoes from their perpendicular granite walls on either side some 2000 to 3000 feet high. Small rivers—for they are small here—rush through them with angry roar; but it would be worse than idiotic to suppose that thev wore down the vast granite walls through which they run to the bed they now occupy. Only Nature's reserved forces, such as the world sees in earthquakes, could rend these granite mountains asunder, and, with perpendicular walls half a mile high, make a pathway for the tiny streams that surge and brawl between them.

THE CAÑON OF THE COLUMBIA

is, in some respects, one of the grandest upon the continent. From Cape Horn to perhaps some twenty miles or more above Celilo, at the head of the Dalles, a distance of sixty or seventy miles, the great river finds its way to the ocean through a gorge, the walls of which are from 500 to 4000 feet high. Even a cursory inspection will convince a practiced eye that for the entire height, and most of the distance, it is composed of nearly perpendicular basaltic rocks. No one series of columns where present reaches from the base to the top of the mountain; but at the foot of the cascades, on the south side of the river, their development is truly wonderful. Suppose before you there is a row of them 500 feet high and twice as long on the face of the mountain; at either end of that thousand feet another row, with their bases on a line with the tops of the first, shoots up another 500 feet, and so on from the base to the top, one row of columns above another, will convince the beholder that the entire mountain is composed of basalt. From the time of the Cæsars to the present all the world has been wondering at, or gazing with admiration at the Giant's Causeway, on the coast of Ireland. It, too, is composed of basaltic columns, and they are actually 300 feet high. Thus America furnishes to Great Britain a ratio in basalt of 3300 to 300; figures which I was wicked enough to write in 1879 would probably represent the influence of the two nations on the affairs of the world 100 years hence.

The Cascade Range, in Oregon and Washington Territory, corresponds with, and is virtually an extension of, the Sierra Nevada, in California. Near the western end of the cañon of the Columbia the Cascades form a splendid rapid, and the river falls thirty-five feet in two miles. From the head of the Cascades steamers run on the smoothly-flowing river for forty-five miles through the splendid cañon to the foot of the Dalles. Here, as the tourist glides along, Mount Hood, clad in a mantle of snow old as creation, peers down upon him through the lateral cañons, while the dark, frowning walls of basalt on either side almost make him shudder and forget for the moment how he can escape from this gloomy prison to the cheerful abodes of mankind. These stupendous basaltic walls, with the river flowing smoothly and beautifully between them, would never for a moment suggest the thought that this grand gorge was formed by the river. Only Nature herself, shaking as a reed this vast mountain-chain, could have rent it asunder and given us the sublime cañon of the Columbia. Only one other,

THE YOSEMITE VALLEY,

can be compared with it, and to that, as in some respects the grandest of them all, let us now turn our attention. A description of it will be most easily remembered by saying it is a gorge in one of the spurs of the Sierra Nevada Mountains, about twelve miles long, a mile wide, and a mile deep. As many, perhaps most of the members, have visited this grandest wonder of the world, only a brief description of it will be attempted. At El Capitan—or Tu-toc-a-nu-la, the granite wall—they are, on both sides, of the same material—is 3,300 feet high and very nearly perpendicular. At the grand arches, the height is about the same and the south dome is 6,000 feet—a very considerable fraction more than a mile—one-half of which is perpendicular. From either side the waterfalls are splendid. The Bridal Vail is 900 feet; the Yosemite, 2,634, more than half a mile; The Vernal Fall of the Merced River, at the head of the cañon, is 350 feet; and Nevada Fall is 700. The question is how was this vast gorge made through this mountain of granite? Prof. Whitney, if correctly reported, ascribes it to the dropping down towards the centre of the earth of a section of the mountain a mile wide. From this opinion, of this master of geological science, with all possible respect, I beg leave to differ. The facts of its structure, in my judgment, warrant the belief that, like all the other cañons above referred to—that of the Niagara alone excepted—it was formed by an upheaval of the mountain, at that particular point, sufficient to break it apart to the extent of a mile—the more probable cause; or the mountain, while intensely heated, contracted enough to do it. A few of my reasons for this opinion are as follows:

These solid granite mountains were once torn apart—on a smaller scale, it is true—for there are immense seams, perhaps two feet thick, of cream-colored feldspar, running through the walls of this valley; and it is believed that a correspondence can be observed in these seams on both sides of the gorge. If rent asunder to admit the injection of these seams of feldspar, why not on a larger scale? When this vast fissure was first made it was undoubtedly very deep, perhaps half a dozen miles or more. Where the break was in the line of the cleavage there the wall stands up perpendicularly, as at El Capitan, and the arches, and a few other points. Where it was not in the cleavage line, immense masses of rock were thrown into the abyss, and from this source and the débris brought down by the Merced river, the gorge gradually filled up to its present level. At El Capitan and the arches, the granite wall stands unbroken to the top, and you can ride right up to it and, from your saddle, put your hand on that wall rising sheer above you for more than three-fifths of a mile. Your horse stands on the fine disintegrated granite, the last contribution of the snowy range to the eastward. But after the valley was filled up to its present general level, at points where the cleavage was not in the line of the upheaval, as in the rear of Mr. Hutchins' hotel and some other places, the frost and perhaps earthquakes continued to throw down immense blocks, and hence there is at this point, a gradual slope to the top on the south side of the valley, with trees growing wherever they can find a ledge or a crevice to get root in. Another instance, showing how water, frost and other causes have broken the symmetry of the valley, may be seen at the Yosemite Fall. Both the height and the front of the escarpment, east and west of the Fall, are in the same line, while the ice and the stream have worn the wall back at the Fall perhaps a quarter of a mile from the front line. And yet the first perpendicular fall is 1,600 feet, or ten times the height of Niagara.

Such facts might be multiplied almost indefinitely, but

enough for this paper. This general remark, however, should be carefully weighed. The cañon of the Columbia, the Yosemite Valley, the Charquinez Strait connecting the Suisun and San Pablo Bays, and the Golden Gate itself, through which the waters of the Sacramento and the San Joaquin, draining the great Valley of California, find their way to the ocean, are all about a mile wide. With the exception of the cañon of the Mississippi, the same is true, it is here repeated, of all the cañons above referred to in the Rocky Mountains and east of them, noticed in this paper. It is submitted, therefore, that the main facts in regard to them, point almost unmistakably to a similar origin for them all. All these cañons I have myself visited, many of them several times. Several of them are splendid, even sublime, beyond the power of the most accomplished pen to describe. I dared not to attempt it, and have, therefore, simply stated what I have myself seen and drawn such conclusions as the facts seemed to warrant.

Let me add a very few words in conclusion upon a paper on the geological history of the Colorado River and the plateau of it, read at the St. Louis meeting by Col. E. C. Dutton, of Washington. This cañon, as described by Maj. Powell, who has the honor of braving almost incredible dangers to explore it and to give the world their first knowledge of its wonders, is some 1,500 miles long; the perpendicular walls are a mile or a fraction of it apart, and are from 1,000 to 5,000 feet high. They are composed of nearly all the series in the geological catalogue, from the granite all the way up to the highest igneous stratified rocks. Now this, by far the longest, and in some respects the most wonderful cañon in the world, Col. Dutton described as having been worn by the Colorado River. In view of the facts herein presented that conclusions seems supremely fanciful and absurd. Like all the others, it could only have been formed by some great convulsion of the earth's crust, and through it the drainage of nearly a thousand miles along the western slopes of the Rocky Mountains finds its way to the Gulf of California.

ADDRESS OF COL. GARRICK MALLERY, U. S. ARMY.

Chairman of the Subsection of Anthropology of the A. A. A. S. at the Opening of that Subsection.

THE GESTURE SPEECH OF MAN.

Anthropology tells the march of mankind out of savagery in which different people have advanced in varying degrees, but all started in progress to civilization from a point lower than that now occupied by the lowest of the tribes now found on earth. The marks of their rude origin, retained by all, are of the same number and kind, though differing in distinctness, showing a common origin to all intellectual and social development, notwithstanding present diversities. The most notable criterion of difference is in the copiousness and precision of oral speech, and connected with that, both as to origin and structure, is the unequal survival of gesture signs, which it is believed once universally prevailed. Where sign language survives it is, therefore, an instructive vestige of the prehistoric epoch, and its study may solve problems in philology and psychology. That study is best pursued by comparing the pre-eminent gesture system of the North American Indians with the more degenerate or less developed systems of other people.

EXAMINATION OF THE INDIAN SYSTEM.

The conditions and circumstances attending the prevalence, and sometimes the disuse, of sign language in North America were explained. The report of travelers that among Indians, as well as other tribes of men, some were unable to converse in the dark, because they could not gesture, is false. It is the old story of Βάρβαρος and ἀγλασσος applied by the Greeks to all who did not speak Greek, repeated by Isaiah of the "stammering" Assyrians, and now appearing in the term *slav* (speaker arrogated to themselves by a large division of the Aryan family), as contradistinguished by the Russians from the Germans, whom they stigmatize as *Njemez* (speechless.)

The theory that sign language was the original utterance of mankind does not depend upon such tales or prejudices. After the immeasurable period during which man has been upon the earth it is not probable that any existing peoples can be found among whom speech has not obviated the absolute necessity for gesture in communication between themselves.

The assertions made that the sign language of Indians originated from one definite tribe or region supposes its comparatively recent origin, whereas the conditions favorable to its development existed very long ago and were co-extensive with the territory of North America occupied by any of the tribes. Numerous evidences were presented as to its antiquity and generality. But the signs are not now, and from the nature of their formation never were, identical and uniform. The process is the same as among uninstructed deaf mutes when associated together, which was explained.

A comparison sometimes made of the diversities of the sign language of the Indians with the dialects and provincialisms of the English language is incorrect, as there is so small a proportion of the sign-using tribes which make identically the same signs to express the same ideas, and also because the signs are not absolute and arbitrary as are the words of English.

ARE SIGNS CONVENTIONAL OR INSTINCTIVE?

Sign language, as a product of evolution, has been developed rather than invented, but each of the separate signs had a definite origin arising out of some appropriate occasion, and the same sign may thus have had many different origins due to identity in the circumstances. No signs in common use were at first conventional. What may appear to be convention largely consists in the differing forms of abbreviation which have been adopted. Yet, while all Indians, as well as all gesturing men, have many signs in common, they use many others which have become conventional in the sense that their etymology and conception are not now known or regarded by those using them. The conventions by which such signs were established occurred during the long periods and under many differing circumstances. Our Indians, far from being a homogeneous race and possessing uniformity in their language, religions and customs, differ from each other more than do the several nations of Europe, and their semiotic conceptions have correspondingly differed.

PERMANENCE OF SIGNS.

Instances were presented of the ascertained permanence of some Indian signs, and of those of foreign peoples and deaf mutes. Though they, as well as words, animals and plants, have had their growth, development and change, those which are general among Indian tribes, and are also found in other parts of the world, must be of great antiquity. Many signs but little differentiated were unstable, while others that have proved to be the best modes of expression have survived as definite and established.

IS THE INDIAN SYSTEM SPECIAL AND PECULIAR?

The Indian system as a whole was compared with those of foreign peoples—the ancient Greeks and Romans, the modern Italians, the Turks, Armenians and Koords, the Bushmen of Africa, the Redjangs and Lelongs of Sumatra, the Fijians, the Chinese, Japanese, and the Australians. The result is that the so-called sign language of Indians is not, properly speaking, one language, but that it and the gesture systems of deaf-mutes and of all

peoples constitute together one language, the gesture language of mankind, of which each system is a dialect. The generic conformity is obvious, while the occasion of specific varieties can be readily understood.

ARCHÆOLOGIC RELATIONS.

The most interesting light in which Indians, as other lower tribes of men, are to be regarded, is in their present representation of the stage of evolution once passed through by our ancestors. Their signs, as well as their myths and customs, form a part of the paleontology of humanity. Their picture writings are now translated by working on the hypothesis that their rude form of graphic representation, when at the same time a system of ideographic gesture signs prevailed, would probably have been connected with the latter. Traces of the signs now used by the Indians are also found in the ideographic pictures of the Egyptian, Chinese and Aztec characters.

HISTORY OF THE GESTURE LANGUAGE.

From the records of the ancient classic authors, and also from the figures on Etruscan vases and Herculanean bronzes and other forms of Archaic art, it is certain that a system of gesture language is of great antiquity. Later, Quintilian gave elaborate rules for gesture, which are specially noticeable for the significant disposition of the fingers still prevailing in Naples. The ancient and modern pantomimes were discussed, and also the gestures of speaking actors in the theatres, the latter being seldom actually significant or self-interpreting even, in the expression of strong emotion. The same scenic gesture must apply to many diverse conditions of fact. Its fitness consists in being the same which the hearer of the expository words would spontaneously assume, if yielding to the same emotions, and which, therefore, by association, tends to induce sympathetic yielding. But the communication of the facts themselves depends upon the words uttered. A true sign language would express the exact circumstances, with or without any exhibition of the general emotion appropriate to them.

PRACTICAL APPLICATION OF SIGN LANGUAGE.

This was shown to be in successful use in cases cited by travelers skilled in it, and its powers were compared with those of speech. It finds actually in nature an image by which any person can express his thoughts and wishes on the most needful subjects to any other person. Merely emotional sounds may correspond with merely emotional gestures, but whether with or without them would be useless for the explicit communication of facts and opinions of which signs themselves are capable. Notwithstanding frequent denials, they are able to express abstract ideas. The rapidity of their communication is very great, and can approach to that of thought. Oral speech is now conventional, and with the similar development of sign language, conventional expressions could be made with hands and body more quickly than with the vocal organs, because more organs could be worked at once.

But such rapidity is only obtained by a system of preconcerted abbreviations and by the adoption of absolute forms, thus sacrificing self-interpretation and naturalness, as has been the case with all oral languages in the degree of their copiousness and precision.

RELATIONS IN PHILOLOGY.

Signs often gave to spoken words their first significance, and many primordial roots of language are found in bodily actions. Examples are given of English, Indian, Greek and Latin words in connection with gesture signs for the same meaning, and the structure of the sign-language was compared with the tongues of this continent, with reference also to old Asiatic and African languages, showing similar operations of conditions in the same psychologic horizon.

ORIGIN OF SPEECH.

It is necessary to be free from the vague popular impression that some oral language of the general character of that now used by man is "natural" to man. There is no more necessary connection between ideas and sounds, the mere signs of words that strike the ear, than there is between the same ideas and signs for them which are addressed only to the eye. Early concepts of thought were of a direct and material character. This is shown by what has been ascertained of the radicals of language, and there does not seem to be any difficulty in expressing by gesture all that could have been expressed by those radicals.

CONCLUSIONS.

It may be conceded that after man had all his present faculties, he did not choose between the adoption of voice and gesture, and never with those faculties, was in a state where the one was used, to the absolute exclusion of the other. The epoch, however, to which our speculations relate is that in which he had not reached the present symmetric development of his intellect and of his bodily organs, and the inquiry is: Which mode of communication was earliest adopted to his single wants and informed intelligence? With the voice he could imitate distinctively but few sounds of nature, while with gesture he could exhibit actions, motions, positions, forms, dimensions, directions and distances, with their derivations and analogues. It would seem from this unequal division of capacity that oral speech remained rudimentary long after gesture had become an efficient mode of communication. With due allowance for all purely imitative sounds, and for the spontaneous action of vocal organs under excitement, it appears that the connection between ideas and words is only to be explained by a compact between speaker and hearer which supposes the existence of a prior mode of communication. This was probably by gesture. At least we may accept it as a clew leading out of the labyrinth of philological confusion, and regulating the immemorial quest of man's primitive speech.

TRICHINÆ CYSTS.

The mode of formation of the cyst of trichina has been studied by M. Chatin and described in a communication to the Académie de Sciences. It was formerly said to be formed partly from the contractile tissue, and partly by a secretion from the nematoid, but this opinion was based only on some apparent differences in the thickness or aspect of the cyst wall, and not on any careful study of its formation, which necessitates the examination of animals dying or killed in different states of the affection. When it arrives in the muscles the worm forms adhesions with the interfascicular tissue in which rapid changes occur. The elements increase in size, and during the growth of the protoplasm it assumes the appearance of an amorphous mass, in which, however, nuclei and vacuoles can be seen, which seem to indicate that the mass consists really of aggregated cells. By the growth of this the primitive fibres are compressed. In the new protoplasm fine proteoid granulations are first observed, and then other granulations which present all the reactions of glycogen. Then follow important changes in the periphery of the granular mass, containing the trichina, now curled up in the interior; the outer surface becomes distinctly thickened and indurated, and may then become lamellated or present granulations or folds. The sarcolemma takes no part in the formation of the cyst except occasionally furnishing it with a purely adventitious layer. Moreover, when the nematoid contracts its first adhesions to sarcolemma, and not to the interfascicular tissue. it rapidly dies without determining a new formation.

BEFORE leaving New York, King Kalakaua called on Mr. Edison. He was accompanied on his visit by the Attorney-General of his island kingdom, Mr. Armstrong, and by an intimate friend residing in this city, whose acquaintance he made in Vienna. Punctually at nine o'clock in the evening his majesty alighted from the carriage of his friend in front of the Fifth-avenue mansion. He was introduced to Mr. Edison, who escorted him through the building, and by means of models, maps, drawings, and the 55 lamps in operation, explained the theory of the conversion of steam-power into electricity and the generation of light in the carbon loop.

Escorting his distinguished visitor to the library, Mr. Edison first explained the science of the light, and then, by reference to maps of the district that his engineers are preparing for the experiment, the application of his system to the practical requirements of a city. The region to be lighted will require 22,000 lights, all of which are to be supplied from a central station in Pearl street, where 12 engines of 185 horse power are to be placed. Ten of these will be in constant operation, the other two being held in reserve to meet the emergencies of accident. The engines will be run at a rate of speed equal to that of a locomotive at 60 miles an hour, and a new feature of the system is that no belts are employed to transmit the power to the dynamo-electric generators, and the power is applied directly, avoiding the irregularity and vibration arising from the slipping of the belt which seems inseparable from the old practice. The mains consist of large iron pipes, in which the crescent-shaped positive and negative conductors are carried, being insulated from such by means of a non-conducting material with which the pipes are filled when in a pasty condition, induced by heat, but which hardens like a concrete pavement in the process of cooling. These mains in their passage through the streets are all connected with each other by means of ingenious connection boxes, the whole forming a subterranean net-work of electrical conductors comparable to the capillary circulation in the skin of an animal body.

His Majesty listened with intense but almost silent interest, and examined the cross-sections of the electrical mains and the interior arrangement of the connection boxes with critical closeness, now and then asking a question in the purest English imaginable, and with a voice that was strikingly low, mellow, and muscial, and yet so sharply defined in the articulation of the consonants as to impress the ear at an unusual distance. He seemed particularly interested in the statement that after steam power had been transformed into electricity and carried to a great distance in that form it could again be converted into motive power by means of an electrical motor, and sold to customers for the purpose of running elevators or operating hoist-ways. His eyes lighted when he was told that one of the most profitable departments of the business of the company would be the sale of power to manufactories and business firms in quantities as small as a single horse-power, costing, under circumstances of ordinary use, not more than eight cents a day.

From the library Mr. Edison led the way to the front parlor, brilliantly lighted. Pressing the toe of his shoe upon a knob projecting from the floor, every lamp was instantaneously extinguished and as suddenly blazed out again. The inventor next turned the stop-cock of a single lamp among the group and extinguished it. The party then ascended to the upper floor, where more wonders were in store, and then descended two flights beneath the street level, where, in a low-ceiled vault, a small engine was operating, with nearly absolute silence, a generator whose cylinder performed 1200 revolutions per minute. After inspecting every detail, his Majesty took leave of the inventor, and repaired to his carriage. One of the points that appeared to impress him most was the steadiness of the light, and its freedom from vibration.—*N. Y. Times*.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING SEPT. 24, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

SEPTEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 18	30.094	30.228	0 a. m.	29.964	12 p. m.	68.3	60.6	78	3 p. m.	64	5 p. m.	53	6 a. m.	53	6 a. m.	142.
Monday, 19	29.831	29.964	0 a. m.	29.788	4 p. m.	75.0	65.3	84	3 p. m.	70	6 p. m.	64	6 a. m.	60	6 a. m.	140.
Tuesday, 20	29.869	29.898	9 a. m.	29.810	5 p. m.	73.3	63.3	83	4 p. m.	69	4 p. m.	63	6 a. m.	58	7 a. m.	134.
Wednesday, 21	29.999	30.068	12 p. m.	29.892	0 a. m.	60.7	59.3	73	4 p. m.	64	4 p. m.	62	12 p. m.	56	7 a. m.	130.
Thursday, 22	30.069	30.108	10 a. m.	30.012	5 p. m.	66.6	62.3	70	4 p. m.	65	4 p. m.	60	5 a. m.	59	5 a. m.	87.
Friday, 23	29.040	30.046	0 a. m.	29.882	4 p. m.	74.6	67.3	85	4 p. m.	70	2 p. m.	67	6 a. m.	64	6 a. m.	139.
Saturday, 24	29.985	30.004	9 a. m.	29.942	0 a. m.	77.3	68.3	85	4 p. m.	73	4 p. m.	68	6 a. m.	64	6 a. m.	141.

Mean for the week .... 29.969 inches.
Maximum for the week at 0 a. m., Sept. 18th .... 30.228 "
Minimum " at 4 p. m., Sept. 19th .... 29.788 "
Range .... .440 "

	Dry.		Wet.	
Mean for the week	71.7	degrees	63.7	degrees.
Maximum for the week, at 4 pm. 24th	85.	" at 4 pm 24th,	73.	"
Minimum " " 6 am. 18th	53.	" at 6 am 18th,	53.	"
Range " "	32.	"	20.	"

SEPTEMBER	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0 10
Sunday, 18	n. n. e.	n.	n. e.	60	¼	9.00 am	.380	.436	.482	76	48	66	0	1 cir. s.	4 cir. cu.	-----	-----	-----	--	2
Monday, 19	n.	n. n. e.	n.	130	1½	12.00 m	.425	.447	.628	64	39	72	2 cir. cu.	2 cir. s.	5 cu.	-----	-----	-----	--	8
Tuesday, 20	n. w.	n. n. w.	w.	90	1¾	2.00 am	.403	.438	.519	67	41	60	0	2 cir. cu.	3 cir. cu.	-----	-----	-----	--	8
Wednesday, 21	n. e.	e. s. e.	s. s. e.	152	1¼	7.30 am	.356	.390	.483	62	50	78	0	0	0	-----	-----	-----	--	0
Thursday, 22	s. s. e.	s.	s. s. w	153	2¼	7.20 pm	.447	.529	.543	77	74	79	3 cir. cu.	10	10	-----	-----	-----	--	0
Friday, 23	w. s. w.	s. w.	w. s. w.	236	7	1.40 pm	.556	.558	.604	84	49	72	10	6 cir. cu.	0	-----	-----	-----	--	0
Saturday, 24	w.	s.	s. s. w.	129	¾	3.30 pm	.529	.583	.612	75	50	62	1 cir.	4 cir. cu.	0	-----	-----	-----	--	0

Distance traveled during the week .... 950 miles.
Maximum force .... 7 lbs.

Total amount of water for the week .... .00 inch.
Duration of rain .... 00 hours, 00 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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TEACHING OF CHEMISTRY AND PHYSICS IN THE UNITED STATES.*

II.

In normal schools, the time which can be assigned to work in chemistry and physics is necessarily limited; it becomes then all-important that it should be of the right sort. As Professor Clarke points out, it is not the purpose of such schools to train specialists in any one department of learning, neither should they attempt to give a broad general education. The sole function of a normal school is to fit students for the profession of teaching.

The Bureau of Education has taken pains to enquire how far the scientific work in normal schools has complied with the plan which was originally formed to preserve them within their original functions.

On this point the report states that:

"An examination of the evidence presented in this report will show a great diversity among the various normal schools with respect to chemistry and physics. By far the larger number of them treat these sciences exactly as they are treated in secondary institutions and the smaller colleges; that is, they teach the elements of both subjects, partly by text books and partly by lectures; a few experiments are exhibited, and laboratory work on the part of the students is entirely ignored. In other words, the practice of these schools with reference to the sciences does not accord with the theory upon which they were originally founded."

A small number of normal schools, however,

"Adopt a more rational policy. Recognizing the fact that their students may be called upon to teach chemistry and physics, they endeavor to train them intelligently in methods of instruction."

Respecting instruction in chemistry and physics in universities, colleges and schools of science, much interesting matter is presented, giving in detail the actual work done in these branches of science at the most important institutions of this character.

The general conclusion drawn by Professor Clarke on the character of scientific instruction in universities and colleges is not favorable to such establishments. He says:

"Many high schools are actually doing more and better work with these sciences than is done in a very considerable number of colleges bearing good reputations."

The low standard of scientific work in universities and colleges is attributed by the report to persistent use "of the old-fashioned plan of a fix curriculum."

"Clearly these colleges could, if they would, build upon the work of the preparatory schools as a foundation, and, with no more cost of time, carry their pupils much further than they do now. The present subordinate position of scientific studies is undoubtedly due to the continuation in so many localities of the old-fashioned plan of a fixed curriculum. Given a college in which the latter still holds its own and in which the classics and mathematics have been for many years the dominant subjects of study, and we have an institution wherein but little time can be given to any one of the sciences. One term, from a third to half an academic year in length, is all that is usually allowed to chemistry. This is absurdly inadequate as one term in Latin or one term in mathematics, with no previous preparation, would be. By this system the sciences are not only underrated, but smattering is directly encouraged. The student trained in it can have no definite idea of scientific methods, scientific reasoning, or the scientific spirit. Even the professor in charge of the sciences may be himself a smatterer, teaching several branches without ever having received a systematic training in any one of them. Such teachers, who keep ahead of their classes by only a few lessons, are unfortunately very common, and with them the modern laboratory methods are simply impossible."

Professor Clarke may be correct in these general conclusions, but it is agreeable to refer to the many honorable exceptions, colleges where scientific instruction is offered on the most liberal and enlightened basis.

It would be difficult to take exception to the courses of study in Chemistry and Physics at Columbia College, New York City, where the collection of physical apparatus is the finest in the country, and three laboratories provided for the use of students.

The instruction in Physics and Chemistry at the school of mines of this college is thus described in the report:—

Physics.—Professor, O. N. Rood; mechanics is taught by Professor William G. Peck. The first year students, in the first term, take up the subject of heat, including the steam engine, and acoustics. In the second term they study optics, electricity and magnetism. The courses are illustrated by experiments and problems and are pre-

* Circulars of Information of the Bureau of Education No. 6. 1881.
A report on the teaching of Chemistry and Physics in the United States, by Frank Wigglesworth Clarke, S. B., Professor of Chemistry and Physics in the University of Cincinnati. Washington, 1881.

scribed for all students. To the third year class, lectures are delivered upon electro-statics, the mechanical theory of heat, mathematical optics, and the undulatory theory of light. Some of the lectures are accompanied by experimental demonstrations. This course is required of all students except those in chemistry, with whom it is optional.

Mechanics is taught in the third year to the students in mining engineering, civil engineering, and metallurgy. The mechanics of solids is studied in the first term and the mechanics of fluids in the second.

No physical laboratory work is mentioned in the handbook of information.

Chemistry.—Professor, C. F. Chandler; instructors, Elwyn Waller, Pierre De Peyster Ricketts, Alexis A. Julien, James S. C. Wells, Henry C. Bowen, Francis N. Holbrook, and Louis H. Laudy. General inorganic chemistry, stoichiometry, qualitative analysis, quantitative analysis, and blowpiping are required studies in all the courses. Assaying is taught to students in mining, metallurgy, and chemistry. In the geological and chemical courses, organic chemistry is studied. The chemical students have also a large amount of work in applied chemistry. Quantitative blowpipe analysis is an optional study in all of the courses.

In general chemistry the first year students attend three exercises a week throughout the year. This course is preliminary to practical instruction in the laboratory. The students are drilled upon the lectures, with free use of the best text books, and take notes which must be submitted to the professor. At the end of the year there is a rigid examination. The second class also attend three times a week during the year, and receive instruction in theoretical chemistry adapted to the needs of special scientific students.

For analytical chemistry there are three laboratories, one for qualitative analysis, one for quantitative analysis, and a third for assaying. Each of these is thoroughly equipped and is in the special charge of an instructor with an assistant. Every student is provided with a convenient table containing drawers and cupboards, and is supplied with a complete outfit of apparatus and reagents. The laboratories are open daily, except Saturdays, Sundays, holidays, and vacations, from 10 A. M. to 4 P. M.

During the second year, qualitative analysis is taught by lectures, blackboard exercises, and constant laboratory practice. The spectroscope is freely used. When the student shows, by written and experimental examination, that he is sufficiently familiar with qualitative work, he is allowed to enter the quantitative laboratory. In the third and fourth years, quantitative analysis is taught, the laboratory exercises being accompanied still by lectures and blackboard work. The laboratory course is graded after the usual manner, the student beginning with comparatively simple substances of known composition and passing on by degrees to the analysis of more complex bodies, such as coals, pig iron, various ores, slags, mattes, and so on. Both volumetric and gravimetric methods are employed. In the fourth year the student is admitted to the assay laboratory, where he is furnished with a suitable table and a set of assaying apparatus. Here he has access to crucible and muffle furnaces and to volumetric apparatus for the assay of alloys of gold and silver. The general principles and special methods of assaying are described in the lecture-room, and at the same time the ores of the various metals and their appropriate fluxes are exhibited and described. The student is then supplied with different ores and is required to assay each ore in duplicate under the supervision of the instructor.

Stoichiometry is taught, by lectures and blackboard exercises, as a part of the course in general chemistry, through the first and second years; and its practical applications are developed in lectures upon quantitative analysis and assaying.

In applied chemistry, the instruction extends through the third and fourth years and consists of lectures illustrated by experiments, diagrams, and specimens. The cabinet of industrial chemistry is very large and complete, containing several thousand specimens and materials and products.

It will be noticed that the course of study at this college is thorough, practical and technical, "the design being to train analysts and technologists." Professor Chandler has brought to bear in this work the full weight of his well-known administrative abilities, and the School of Mines of the City of New York may well be taken as a model for all future establishments of the same class on this continent.

A perusal of this report will make the fact evident, that in this country ample facilities exist for the most thorough instruction in both Physics and Chemistry, and the record shows that since the year 1865 the course of instruction in these departments of science has been one of continuous progress.

Of Columbia College, New York, we have spoken, but it might appear that we made an invidious selection if we did not refer to other prominent centres ot physical and chemical research. Among many of such we may name the Massachusetts Institute of Technology; the Stevens Institute, Hoboken; the Universities of Pennsylvania, Virginia and Cincinnati; Yale, Harvard and the Johns Hopkins University. To-day the higher chemistry can be studied in a score of places where twenty years ago no adequate facilities were offered, and the modern physics, with its mathematical methods and its laboratories, is rapidly coming into vogue.

One other feature of the new movement remains to be mentioned, namely, the spread of scientific teaching downward into the secondary schools. These, too, are organizing laboratories, teaching young scholars to see and experiment for themselves, preparing the way for higher work, and rendering the latter more easily possible. The "summer schools" of chemistry at Harvard and elsewhere, the Woman's

Laboratory at the Massachusetts Institute of Technology, and such-like enterprises are doing much in this direction. To-day Chemistry and Physics are taught in nearly all the academies and high schools of the land; so that the larger colleges, whenever they see fit, may easily require from the candidate for admission a wider knowledge of these sciences than they themselves taught a dozen years ago. When and in what manner the present scientific movement shall culminate, no one can say; but the fact of growth is evident everywhere. This report is an attempt to catch the present aspect of affairs and fix it in a permanent record.

ON THE SOURCES OF ENERGY IN NATURE AVAILABLE TO MAN FOR THE PRODUCTION OF MECHANICAL EFFECT.*

By Sir William Thomson, F. R. S.

During the fifty years' life of the British Association, the advancement of Science for which it has lived and worked so well has not been more marked in any department than in one which belongs very decidedly to the Mathematical and Physical Section—the science of Energy. The very name energy, though first used in its present sense by Dr. Thomas Young about the beginning of this century, has only come into use practically after the doctrine which defines it had, during the first half of the British Association's life, been raised from a mere formula of mathematical dynamics to the position it now holds of a principle pervading all nature and guiding the investigator in every field of science.

A little article communicated to the Royal Society of Edinburgh a short time before the commencement of the epoch of energy under the title "On the Sources Available to Man for the Production of Mechanical Effect,"† contained the following:

"Men can obtain mechanical effect for their own purposes by working mechanically themselves, and directing other animals to work for them, or by using natural heat, the gravitation of descending solid masses, the natural motions of water and air, and the heat, or galvanic currents, or other mechanical effects produced by chemical combination, but in no other way at present known. Hence the stores from which mechanical effect may be drawn by man belong to one or other of the following classes:

"I. The food of animals.

"II. Natural heat.

"III. Solid matter found in elevated positions.

"IV. The natural motions of water and air.

"V. Natural combustibles (as wood, coal, coal-gas, oils, marsh-gas, diamond, native sulphur, native metals, metoric iron.)

"VI. Artificial combustibles (as smelted or electrically-deposited metals, hydrogen, phosphorus).

"In the present communication, known facts in natural history and physical science, with reference to the sources from which these stores have derived their mechanical energies, are adduced to establish the following general conclusions:

"1. *Heat radiated from the sun* (sunlight being included in this term) *is the principal source of mechanical effect available to man.*‡ From it is derived the whole mechanical effect obtained by means of animals working, water-wheels worked by rivers, steam-engines, galvanic engines, wind-mills, and the sails of ships.

"2. The motions of the earth, moon, and sun, and their mutual attractions, constitute an important source of available mechanical effect. From them all, but chiefly no doubt from the earth's motion of rotation, is derived the mechanical effect of water-wheels driven by the tides.

"3. The other known sources of mechanical effect available to man are either terrestrial—that is, belonging to the earth, and available without the influence of any external body—or meteoric—that is, belonging to bodies deposited on the earth from external space. The terrestrial sources, including mountain quarries and mines, the heat of hot springs, and the combustion of native sulphur, perhaps also the combustion of inorganic native combustibles, are actually used; but the mechanical effect obtained from them is very inconsiderable, compared with that which is obtained from sources belonging to the two classes mentioned above. Meteoric sources, including only the heat of newly-fallen meteoric bodies, and the combustion of meteoric iron, need not be reckoned among those available to man for practical purposes."

Thus we may summarize the natural sources of energy as Tides, Food, Fuel, Wind and Rain.

Among the practical sources of energy thus exhaustively enumerated, there is only one not derived from sun-heat—that is the tides. Consider it first. I have called it *practical*, because tide mills exist, but the places where they can work usefully are very rare, and the whole amount of work actually done by them is a drop to the ocean of work done by other motors. A tide of two meters' rise and fall, if we imagine it utilized to the utmost by means of ideal water wheels doing, with perfect economy, the whole work of filling and emptying a dock basin in infinitely short times, at the moments of high and low water, would give just one metre-ton per square metre of area. This work done four times in the twenty-four hours, amounts to 1.1620th of the work of a horse-power. Parenthetically, in explanation, I may say that the French metrical equivalent (to which in all scientific and practical measurements we are irresistibly drawn, notwithstanding a dense barrier of insular prejudice most detrimental to the islanders),—the French metrical equivalent of James Watt's "horse-power" of 550 foot-pounds per second, or 33,000 foot-pounds per minute, or nearly 2,000,000 foot-pounds per hour, is 75 metre-kilogrammes per second, or 4½ metre-tons per minute, or 270 metre-tons per hour. The French ton of 1000 kilos, used in this reckoning, is 0.984 of the British ton.

Returning to the question of utilizing tidal energy, we find a dock area of 162,000 square metres (which is little more than 400 metres square) required for 100-horse power. This, considering the vast costliness of dock construction, is obviously prohibitory of every scheme for economizing tidal energy by means of artificial dock basins, however near to the ideal perfection might be the realized tide-mill, and however convenient and non-wasteful the acumulator—whether Faure's electric accumulator, or other accumulators of energy hitherto invented, or to be invented,—which might be used to store up the energy yielded by the tide mill during its short harvests about the times of high and low water, and to give it out when wanted at other times of six hours. There may, however, be a dozen places possible in the world where it could be advantageous to build a sea-wall across the mouth of a natural basin or estuary, and to utilize the tidal energy of filling it and emptying it by means of sluices and water-wheels. But if *so* much could be done, it would in many cases take only a little more to keep the water out altogether, and make fertile land of the whole basin. Thus we are led up to the interest-

* British Association, 1881.

† Read at the Royal Society of Edinburgh on February 2, 1852. (*Proceedings* of that date.)

‡ A general conclusion equivalent to this was published by Sir John Herschel in 1833. See his "Astronomy," edit. 1849, § (399.)

ing economical question, whether is 40 acres (the British *agricultural* measure for the area of 162,000 square metres) or 100 horse-power more valuable? The annual cost of 100 horse-power night and day for 365 days of the year, obtained through steam from coals, may be about ten times the rental of forty acres, at £2 or £3 per acre. But the value of land is essentially much more than its rental, and the rental of land is apt to be much more than £2 or £3 per acre in places where 100 horse-power could be taken with advantage from coal through steam. Thus the question remains unsolved, with the possibility that in one place the answer may be *one hundred horse power*, and in another *forty acres*. But, indeed, the question is hardly worth answering, considering the rarity of the cases, if they exist at all, where embankments for the utilization of tidal energy are practicable.

Turning now to sources of energy derived from sun-heat, let us take the wind first. When we look at the register of British shipping, and see 40,000 vessels, of which about 10,000 are steamers and 30,000 sailing ships, and when we think how vast an absolute amount of horse-power is developed by the engines of those steamers, and how considerable a proportion it forms of the whole horse-power taken from coal annually in the whole world at the present time, and when we consider the sailing ships of other nations, which must be reckoned in the account, and throw in the little item of windmills, we find that, even in the present days of steam ascendancy, old-fashioned wind still supplies a large part of all the energy used by man. But however much we may regret the time when Hood's young lady, visiting the fens of Lincolnshire, at Christmas, and writing to her dearest friend in London (both sixty years old if they are now alive), describes the delight of sitting in a bower and looking over the wintry plain, not desolate, because "windmills lend revolving animation to the scene," we cannot shut our eyes to the fact of a lamentable decadence of wind-power. Is this decadence permanent, or may we hope that it is only temporary? The subterranean coal stores of the world are becoming exhausted surely, and not slowly, and the price of coal is upward bound—upward bound on the whole, though no doubt it will have ups and downs in the future as it has had in the past, and as must be the case in respect to every marketable commodity. When the coal is all burned, or long before it is all burned—when there is so little of it left, and the coal mines from which that little is to be excavated are so distant and deep and hot that its price to the consumer is greatly higher than at present, it is most probable that wind-mills or wind motors in some form, will again be in the ascendant, and that wind will do man's mechanical work on land at least, in proportion comparable to its present doing of work at sea.

Even now, it is not utterly chimerical to think of wind superseding coal in some places for a very important part of its present duty—that of giving light. Indeed, now that we have dynamos and Faure's accumulator, the little want to let the thing be done is cheap windmills. A Faure cell containing 20 kilos. of lead and minium charged and employed to excite incandescent vacuum-lamps has a light-giving capacity of 60 candle hours (I have found considerably more in experiments made by myself; but I take sixty as a safe estimate). The charging may be done uninjuriously, and with good dynamical economy in any time from six to twelve hours or more. The drawing off of the charge for use may be done safely, but somewhat wastefully, in two hours, and very economically in any time of from five hours to a week, or more. Calms do not last often longer than three or four days at a time. Suppose, then, that a five-days storage capacity, suffices (there may be a little steam engine ready to set to work at any time after a four days' calm, or the user of the light may have a few candles or oil lamps in reserve and be satisfied with them when the wind fails for more than five days.) One of the 20-kilo. cells charged when the windmill works, for five or six hours at any time and left with its 60 candle-hours' capacity to be used six hours a day for five days, gives a 2-candle light Thus thirty-two such accumulator cells soused would give as much light as four burners of London 16-candle gas. The probable cost of dynamo and accumulator does not seem fatal to the plan, if the windmill could be had for something comparable with the prime cost of a steam engine capable of working at the same horse power as the wind mill when in good action. But wind mills as hitherto made are very costly machines; and it does not seem probable that without inventions not yet made, wind can be economically used to give light in any considerable class of cases, or to put energy into store for other kinds of work.

Consider, lastly, rain-power. When it is to be had in places where power is wanted for mills and factories of any kind, water-power is thoroughly appreciated. From time immemorial, water-motors have been made in large variety for utilizing rain-power in the various conditions, in which it is presented, whether in rapdidly-flowing rivers in natural waterfalls, or stored at heights in natural lakes or artificial reservoirs. Improvements and fresh inventions of machines of this class still go on; and some of the finest principles of mathematical hydrodynamics have, in the lifetime of the British Association, and, to a considerable degree with its assistance, been put in requisition for perfecting the theory of hydraulic mechanism and extending its practical applications.

A first question occurs: Are we necessarily limited to such natural sources of water-power as are supplied by rain falling on hill-country, or may we look to the collection of rain-water in tanks placed artificially at sufficient heights over flat country to supply motive power economically by driving water-wheels? To answer it: Suppose a height of 100 metres, which is very large for any practicable building, or for columns erected to support tanks; and suppose the annual rainfall to be three-quarters of a metre (30 inches). The annual yield of energy would be 75 metre-tons per square metre of the tank. Now one horse-power for 365 times 24 hours is 236,500 foot-tons; and therefore, dividing this by 75, we find 3153 sq. metres as the area of our supposed tank required for a continuous supply of one horse-power. The prime cost of any such structure, not to speak of the value of the land which it would cover, is utterly prohibirory of any such plan for utilizing the motive power of rain. We may or may not look forward hopefully to the time when windmills will again "lend revolving animation" to a dull flat country; but we certainly need not be afraid that the scene will be marred by forests of iron columns taking the place of natural trees, and gigantic tanks overshadowing the fields and blackening the horizon.

To use rain-power economically on any considerable scale we must look to the natural drainage of hill country, and take the water where we find it either actually falling or stored up and ready to fall when a short artificial channel or pipe can be provided for it at moderate cost. The expense of acqueducts, or of underground water-pipes, to carry water to any great distance—any distance of more than a few miles or a few hundred yards—is much too great for economy when the yield to be provided for is *power*; and such works can only be undertaken when the *water itself* is what is wanted. Incidentally, in connection with the water supply of towns, some part of the energy due to the head at which it is supplied may be used for power. There are, however, but few cases (I know of none except Greenock) in which the energy to spare over and above that devoted to bringing the water to where it is wanted, and causing it to flow fast enough for convenience at every opened tap in every house or factory, is enough to make it worth while to make arrangements for letting the water-power be used without wasting the water-substance. The cases in which water-power

is taken from a town supply are generally very small, such as working the bellows of an organ, or "hair-brushing by machinery," and involve simply throwing away the used water. The cost of energy thus obtained must be something enormous in proportion to the actual quantity of the energy, and it is only the smallness of the quantity that allows the convenience of having it when wanted at any moment, to be so dearly bought.

For anything of great work by rain-power, the water-wheels must be in the place where the water supply with natural fall is found. Such places are generally far from great towns, and the time is not yet come when great towns grow by natural selection beside waterfalls for power; as they grow beside navigable rivers, for shipping. Thus hitherto the use of water-power has been confined chiefly to isolated factories which can be conveniently placed and economically worked in the neighborhood of natural waterfalls. But the splendid suggestion made about three years ago by Mr. Siemens in his presidential address to the institution of Mechanical Engineers, that the power of Niagara might be utilized, by transmitting it electrically to great distances, has given quite a fresh departure for design in respect to economy of rain-power. From the time of Joule's experimental electro-magnetic engines developing 90 per cent of the energy of a Voltaic battery in the form of weights raised, and the theory of the electro-magnetic transmission of energy completed thirty years ago on the foundation afforded by the train of experimental and theoretical investigations by which he established his dynamical equivalent of heat in mechanical, electric, electro-chemical, chemical, electro-magnetic, and thermoclastic phenomena, it had been known that potential energy from any available source can be transmitted electro-magnetically by means of an electric current through a wire, and directed to raise weights at a distance, with unlimitedly perfect economy. The first large-scale practical application of electro-magnetic machines was proposed by Holmes in 1854, to produce the electric light for lighthouses, and persevered in by him till he proved the availibility of his machine to the satisfaction of the Trinity House and the delight of Faraday in trials at Blackwall in April, 1857, and it was applied to light the South Foreland lighthouse on December 8, 1858. This gave the impulse to invention; by which the electro-magnetic machine has been brought from the physical laboratory into the province of engineering, and has sent back to the realm of pure science a beautiful discovery—that of the fundamental principle of the dynamo, made triply and independently, and as nearly as may be simultaneously, in 1867 by Dr. Werner Siemens, Mr. S. A. Varley, and Sir Charles Wheatstone; a discovery which constitutes an electro-magnetic analogue to the fundamental electrostatic principle of Nicholson's revolving doubler, resuscitated by Mr. C. F. Varley in his instrument "for generating electricity;" patented in 1860; and by Holtz in his celebrated electric machine; and by myself in my "replenisher" for multiplying and maintaining charges in Leyden jars for heterostatic electrometers, and in the electrifier for the siphon of my recorder for submarine cables.

The dynamos of Gramme and Siemens, invented and made in the course of these fourteen years since the discovery of the fundamental principle, give now a ready means of realizing economically on a large scale, for many important practical applications, the old thermo-dynamics of Joule in electro-magnetism; and, what particularly concerns us now in connection with my present subject, they make it possible to transmit electro-magnetically the work of waterfalls through long insulated conducting wires, and use it at distances of fifties or hundreds of miles from the source, with excellent economy—better economy, indeed, in respect to proportion of energy used to energy dissipated than almost anything known in ordinary mechanics and hydraulics for distances of hundreds of yards instead of hundreds of miles.

In answer to questions put to me in May, 1879,* by the Parliamentary Committee on Electric Lighting, I gave a formula for calculating the amount of energy transmitted, and the amount dissipated by being converted into heat on the way, through an insulated copper conductor of any length, with any given electro-motive force applied to produce the current. Taking Niagara as example, and with the idea of bringing its energy usefully to Montreal, Boston, New York, and Philadelphia, I calculated the formula for the distance of 300 British statute miles (which is greater than the distance of any of those four cities from Niagara, and is the radius of a circle covering a large and very important part of the United States and British North America), I found almost to my surprise that, even with so great a distance to be provided for, the conditions are thoroughly practicable with good economy, all aspects of the case carefully considered. The formula itself will be the subject of a technical communication to Section A in the course of the meeting on which we are now entering. I therefore at present restrict myself to a slight statement of results.

1. Apply dynamos driven by Niagara to produce a difference of potential of 80,000 volts between a good earth connection and the near end of a solid copper wire of half an inch (1.27 centimetre) diameter, and 300 statute miles (483 kilometres) length.

2. Let resistance by driven dynamos doing work, or by electric lights, or, as I can now say, by a Faure battery taking in a charge, be applied to keep the remote end at a potential differing by 64,000 volts from a good earth-plate there.

3. The result will be a current of 240 webers through the wire taking energy from the Niagara end at the rate of 26,250 horse-power, losing 5250 (or 20 per cent) of this by the generation and dissipation of heat through the conductor and 21,000 horse-power (or 80 per cent of the whole) on the recipients at the far end.

4. The elevation of temperature above the surrounding atmosphere, to allow the heat generated in it to escape by radiation and be carried away by convection is only about 20° Centigrade; the wire being hung freely exposed to air like an ordinary telegraph wire supported on posts.

5. The striking distance between flat metallic surfaces with difference of potentials of 80,000 volts (or 75,000 Daniell's) is (Thomson's "Electrostatics and Magnetism." § 340) only 18 millimetres, and therefore there is no difficulty about the insulation.

6. The cost of the copper wire, reckoned at 8d. per lb., is £37,000, the interest on which at 5 per cent is £1900 a year, If 5250 horse-power at the Niagara end costs more than £1900 a year, it would be better economy to put more copper into the conductor; if less, less. I say no more on this point at present, as the economy of copper for electric conduction will be the subject of a special communication to the Section.

I shall only say, in conclusion, that one great difficulty in the way of economizing the electrical transmitting power to great distances, or even to moderate distances of a few kiloms., is now overcome by Faure's splendid invention. High potential—as Siemens, I believe, first pointed out—is the essential for good dynamical economy in the electric transmission of power. But what are we to do with 80,000 volts when we have them at the civilized end of the wire? Imagine a domestic servant going to dust an electric lamp with 80,000 volts on one of its metals? Nothing above 200 volts ought on any account ever to be admitted into a house or ship or other place where safeguard against accident cannot be made absolutely and forever trustworthy against all possibility of accident. In an electric workshop 80,000 volts is no more dangerous than a circular saw. Till I learned Faure's invention I could but think of step-down dynamos, at a main receiving station to take energy direct from the electic main

* Printed in the Parliamentary Blue-book Report of the Committee on Electric Lighting, 1879.

with its 80,000 volts, and supply it by secondary 200-volt dynamos or 100-volt dynamos, through proper distributing wires, to the houses and factories and shops where it is to be used for electric lighting, and sewing machines, and lathes, and lifts, or whatever other mechanism wants driving power. Now the thing is to be done much more economically, I hope, and certainly with much greater simplicity and regularity, by keeping a Faure battery of 40,000 cells always being charged direct from the electric main, and applying a methodical system of removing sets of 50, and placing them on the town-supply circuits, while other sets of 50 are being regularly introduced into the great battery that is being charged, so as to keep its number always within 50 of the proper number, which would be about 40,000 if the potential at the emitting end of the main is 80,000 volts.

ON THE ARRESTATION OF INFUSORIAL LIFE.*

By Prof. Tyndall.

Three years ago I brought with me to the Alps a number of flasks charged with animal and vegetable infusions. The flasks had been boiled from three to five minutes in London, and hermetically sealed during ebullition. Two years ago I had sent to me to Switzerland a batch of similar flasks containing other infusions. On my arrival here this year 120 of these flasks lay upon the shelves in my little library. Though eminently putrescible, the animal and vegetable juices had remained as sweet and clear as when they were prepared in London. Still an expert taking up one of the flasks containing an infusion of beef or mutton would infallibly pronounce it to be charged with organisms. He would find it more or less turbid throughout, with massive flocculi moving heavily in the liquid. Exposure of the flask for a minute or two to lukewarm water would cause both turbitity and flocculi to disappear, and render the infusion as clear as the purest distilled water. The turbidity and flocculi are simply due to the coagulation of the liquid to a jelly. This fact is some guarantee for the strength of the infusions. I took advantage of the clear weather this year to investigate the action of solar light on the development of life in these infusions, being prompted thereto by the interesting observations brought before the Royal Society by Dr. Downs and Mr. Blunt, in 1877. The sealed ends of the flasks being broken off, they were infected in part by the water of an adjacent brook, and in part by an infusion well charged with organisms. Hung up in rows upon a board, half the flasks of each row were securely shaded from the sun, the other half being exposed to the light. In some cases, moreover, flasks were placed in a darkened room within the house, while their companions were exposed in the sunshine outside. The clear result of these experiments, of which a considerable number were made, is that by some constituent or constuents of the solar radiation an influence is exercised inimical to the development of the lowest infusoria. Twenty-four hours usually sufficed to cause the shaded flasks to pass from clearness to turbidity, while thrice this time left the exposed ones without sensible damage to their transparency. This result is not due to mere differences of temperature between the infusions. On many occasions the temperature of the exposed flasks was far more favorable to the development of life than that of the shaded ones. The energy which in the cases here referred to prevented putrefaction was energy in the radiant form. In no case have I found the flasks sterilized by insolation, for on removing the exposed ones from the open air to a warm kitchen they infallibly changed from cleaness to turbidity. Four and twenty hours were in most cases sufficient to produce this change. Life is, therefore, prevented from developing itself in the infusions as long as they are exposed to the solar light, and the paralysis thus produced enables them to pass through the night time without alteration. It is, however, a suspension, not a destruction, of the germinal power, for, as before stated, when placed in a warm room life was invariably developed. Had I had the requisite materials I should like to have determined by means of colored media, or otherwise, the particular constituents of the solar radiation which are concerned in this result. The rays, moreover, which thus interfere with life must be absorbed by the liquid or by its germinal matter. It would therefore be interesting to ascertain whether, after transmission through a layer of any infusion, the radiation still possessed the power of arresting the development of life in the same infusion. It would also be interesting to examine how far insolation may be employed in the preservation of meat from putrefaction. I would not be understood to say that it is impossible to sterilize an infusion by insolation, but merely to indicate that I have thus far noticed no case of the kind.

* British Association, 1881.

PLANTÉ'S RHEOSTATIC MACHINE.*

Translated from the French by the Marchioness Clara Lanza.

Ruhmkorff's electric induction machine has proved in the most satisfactory manner that by the intermediary of inductive action, we can transform voltaic electricity into electricity of high tension. M. Bichat has likewise shown that by the same means, currents of high tension can be changed to currents of quantity, analogous to voltaic currents. M. Planté, with his secondary piles, has rendered this demonstration still more emphatic, and as his experiments demanded a greater tension than he was able to produce with his batteries, he undertook the manufacture of an apparatus by which he could obtain veritable discharges of static electricity, capable of forming at will, long thread-like sparks, or short, thick ones. In this way he was induced to make the battery of which we are about to speak, and which he calls the *rheostatic machine.*

Although this apparatus (fig. 1) was presented to the Academy of Sciences and exhibited to most of the physicians who witnessed M. Planté's fine experiments, it is as yet, but little known. Why this should be the case we are at a loss to understand, for it is one of the most perfect machines that can be employed in experiments of static electricity. Had the apparatus borne a foreign name, we are confident it would have attracted considerable attention long ago. It is much to be regretted that we are so constituted in France, that whatever is invented by an unknown man, a *savant* who does not rejoice in an established position or who is not a member of some scientific coterie originating from a celebrated school, is looked upon entirely as a matter of subordinate interest. "It is only an amateur's work," we hear on all sides for awhile and then the subject is dropped forever. In England it is quite different. Amateurs such as Grove, Gassiot, Warren, Delarue, Spottiswoode, Lords, Ross, Lindsay, Raleigh, Elphinstone and many others, find their efforts are appreciated as they deserve to be, and no one ever thinks of inquiring whether they are *savants* patented by the government or not.

M. Planté therefore, not being among the last-mentioned, was forced to meet with indifference which he forcibly overcame later by the fine work he performed with his accumulators. He was not so successful, unfortunately, with his rheostatic machine, and for this reason we shall dwell a little upon the important results it has afforded us.

M. Planté's machine consists of a series of condensers with mica plates, parallel one with the other and capable of being charged and discharged in a manner similar to his secondary batteries without any other alimentary electric source than these latter.

The various pieces composing the apparatus must be

* *La Lumière Electrique*, August 6th, 1881.

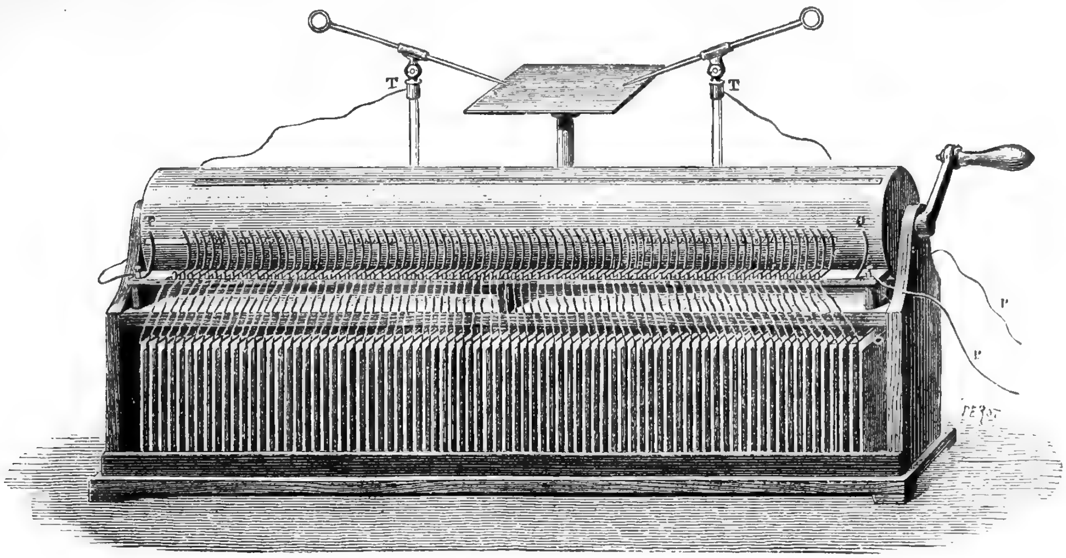

FIG. 1.

taken apart with great care. The commutator is formed of a long cylinder made of hard rubber. It is furnished with longitudinal metallic bands destined to connect the surface condensers, and crossed by pieces of copper wire bent at the ends, the object of the latter being to unite the condensers of tension. To this end, metallic wires, fashioned like springs, rest upon the cylinder and are associated with the two armatures of each condenser by very fine copper wires covered with gutta-percha. They are attached to an ebonite plaque on each side of the cylinder, and the latter can be made to rotate rapidly and continuously by means of a set of wheels. The final springs are separated considerably from those preceding them, in order to prevent the electric sparks from discharging between the tension poles of the rheostatic machine and those of the secondary battery.

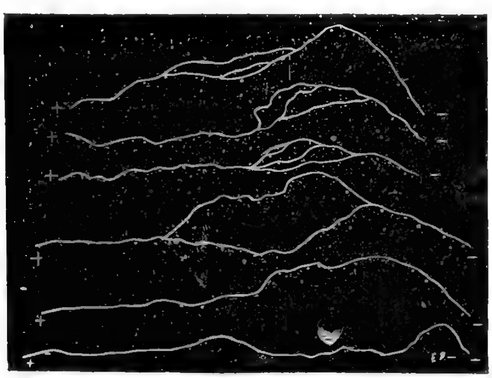

FIG. 2.

The mica plates in the condensers are 0^m 18 in length and 0^m 14 in breadth. The armatures are made of tinfoil. The edges of the condensers are rendered adherent by frames or simple ebonite plaques. These give them more rigidity and cause them more readily to maintain a vertical position, one beside the other, without coming in contact.

When the cylinder is so turned that the longitudinal metallic bands come in juxtaposition with the springs, the armatures in an even range with all the condensers unite on one side, while those in an uneven range are joined on the other, forming a single condenser of large surface. The armatures discharge by causing the right hand boundaries, which can be easily distinguished in the figure, to communicate with the poles of the battery.

When, on the contrary, the cylinder is so turned that its transversal pins are presented to the springs, all the charged condensers are connected in a series or in tension. The armature of the furthest condenser on the left, communicates with the last spring on the other side of the cylinder and ends at branch T of the excitant. The armature of the final condenser on the right communicates with the spring next to the last, and this spring unites with the last metallic pin traversing the cylinder. The last spring placed in the opposite side of the cylinder communicates with the other branch T′ of the excitant. While the condensers are thus connected, the pole, or battery, which charges the apparatus, is entirely beyond the circuit.

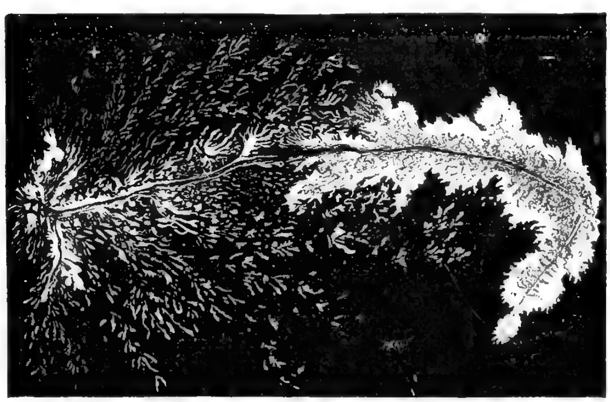

FIG. 3.

M. Planté has constructed rheostatic machines of different sizes. The one here represented is supplied with eighty condensers. The commutative cylinder is is one meter long and $0^m.15$ in diameter.

When this cylinder is put in motion and the machine connected with the battery of 800 secondary couples, we perceive, as the charge begins to act upon the commutator, long lines of sparks at those points where the metallic contact is effected. It becomes a tube of sparkling light and the effect is equally apparent when the discharge in tension occurs. At the same time we obtain a long spark at the excitant T T′.

Sparks produced by this machine attain, when fully exposed to the air, a length of 12 centimetres when influenced by the secondary battery of 800 couples. With less powerful machines, however, the length is reduced, and, according to M. Planté, it will be *in proportion to the number of condensers.* When a spark discharges across metallic filings it sometimes reaches the length of 70 centimetres.

We must remark here that discharges produced in this way have no alternate positive and negative sense, but are always the same. The loss of force resulting from the transformation should, therefore, be less than in induction machines; for as the Voltaic circuit is never closed upon itself for a single instant, no portion of the current is converted into a calorific effect. The machine, moreover, can be kept rotating for a long time and it produces a considerable number of discharges without any apparent weakness being visible on the part of the secondary battery.

The most interesting effects studied by M. Planté, by means of the sparks of this machine, were obtained by causing them to pass over pulverized sulphur in a compound of sulphur and minium. If these powders are spread upon a surface composed of resin and paraffine (1/10) the sparks, while passing over the sulphur, leave a bluish line distinctly visible, as though traced in black lead. This gives us an exact autograph, we may say, of the spark's course. It is easily effaced, however, by being rubbed. But if carefully followed and indented with some sharp pointed instrument it can be rendered intact. Afterwards we can study it thoroughly by tracing a drawing of it. The sparks represented by Fig. 2 were produced as above described.

When we come to investigate these sparks, we find, according to M. Planté, that when they have not the maximum of length which they are capable of attaining, they often display enclosed branches resembling *anastomoses*, and which are likely to escape while our attention is fixed upon the luminous track. Their sinuosities are always rounded, and that angular zig-zag, which is apparent in most electric sparks, is never seen. It is true that this form is sometimes indicated by effects of perspective when the flash is at the horizon. The sinuous shape, however, predominates, and frequently the spark resolves itself into two demi-undulations, forming a sort of S, which is also often seen in flashes of lightning that strike the ground. We find there particularly a very characteristic hook-shaped form, upon which M. Planté has long endeavored to attract attention, and which is produced at the negative pole in a constantly varying manner. M. Planté thinks the formation of this hook arises from the collision of two motions opposed to the ponderable matter drawn from the points of the excitant, an effect which always happens under an angle more or less pronounced, and with a more rapid movement on the side of the positive pole than the negative, doubtless because there is greater electric tension at the former. Our readers will probably recollect that I demonstrated this tension in several ways with the induced currents of Ruhmkorff's bobbin.[1]

If a portion of the sulphur spread upon the plate of the excitant is removed by giving the latter a few slight taps, the sparks change to luminous branch-like aigrettes which are truly magnificent. Fig. 3 represents one of these at its natural size, having a luminous track 15 centimetres in length. M. Planté calls these *arborescent sparks*, and he thinks they serve to explain those impressions of a vegetable appearance sometimes observed upon the bodies of persons struck by lightning, and which merely result from the ramifications of the fiery track made by the flash itself. He attributes these impressions to certain pulverulent particles which are in the course of the discharge, and which, after being projected into the air, heated or blazing, in various directions, fall upon the body that has been struck and produce a kind of cauterization if the particles are merely heated, and luminous impressions if they are blazing.

These experiments are extremely interesting, for they clearly show that the pretended reproduction of neighboring objects upon persons struck by lightning is purely imaginary.

If we neglect to give the taps, before mentioned, to the sulphur-powdered plate, the spark is displayed as represented by Fig. 4. We observe, in this case, that the size

FIG. 4.

of the track is increased on the side of the positive pole, and grows contracted as it advances towards the negative pole. Around the positive pole we see traces corresponding to branches or rays in proportion to the quantity of sulphur removed. On the side of the negative pole we find circular tracks of an entirely different kind, representing, probably, the luminous spots, generally blue, which appear at the negative pole simultaneously with the spark of Ruhmkorff's bobbin.

If the plaque of the excitant belonging to M. Planté's apparatus is arranged so as to produce Lichtenberg figures—that is to say, covered with a compound of pure powdered resin, pulverized sulphur and minium—magnificent arborescent sparks of yet another kind can be obtained, the most curious examples of which are shown by Figs. 5 and 6. Tracings of these sparks are made by placing a sheet of varnished black paper upon the plate.

The different effects produced by the aigrettes and the sparks are particularly marked. When the distance between the points of the excitant is too great to admit of the spark discharging, and merely an aigrette appears, the electric movement of ponderable matter which leaves the negative pole and is manifested by the powdered minium adhering to the resin, does not extend to the positive pole. The latter presents no traces of red powder in the sulphur wreath and divergent rays surrounding it, as may be seen in Fig. 5. If the spark has

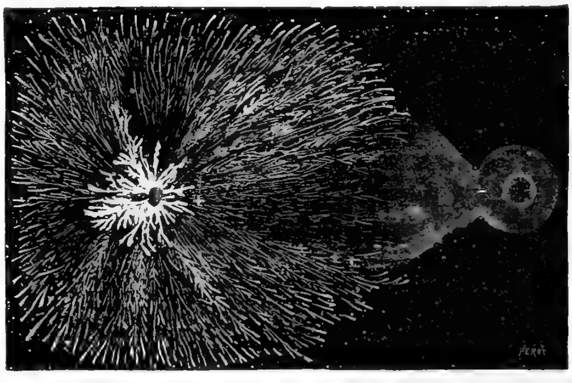

FIG. 5.

discharged, however, the wreath is open and the interior filled with red dust, showing that the electric movement proceeding from the negative pole, extends to the departing point of the positive electricity, as represented by Fig. 6.

[1] See Vol. II. of La Lumierè Electrique p. 439, and also a paper on *La nom homogénéité de l'étincelle d'induction*, p. 89.

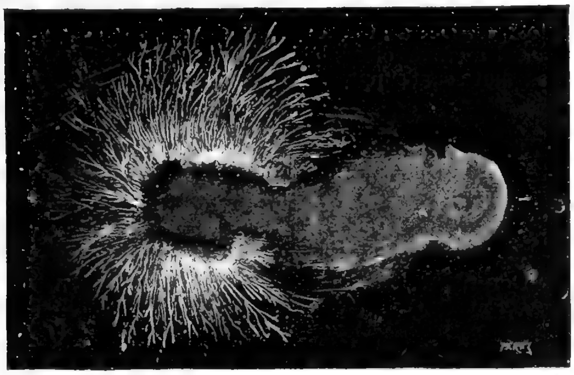

Fig. 6.

"With the spark," says M. Planté, "the distribution of negative electricity presents a curious crab-shaped appearance (Fig. 6.) With the aigrette, the electric movement around this same negative pole gives us the no less bizarre form of a polypus whose tentacles extend towards the positive pole, but do not reach it." (Fig. 5.)

From these results and other experiments quoted by M. Planté, he concludes that a blending of the two electricities may exist at each pole. This would infer that with electric currents of sufficient tension to obtain a continued series of discharges of static electricity, we could have a complete decomposition of the water at each pole and consequently a mixture of hydrogen and oxygen.

Pushing the study of these sparks still further, we find that the movement proceeding from the positive pole, externally, envelopes the negative electric movement like a bundle of curved sky-rockets. However, we often see at the same time an inward flux of positive electricity around the line of the spark between the positive current enveloping the exterior, and between both, the negative electric current which appears as though inhaled by the positive pole. This led M. Planté to suppose that the negative electricity, or else the ponderable matter which it carries with it, moves in an annular space furnished by the electrified matter proceeding from the positive pole. According to him, it would follow that the aspiratory or ascendant effects of the water obtained by electric currents of high tension might explain the ascension of water in a cloudy form as seen in water-spouts.

In a forthcoming article we will study other phenomena no less remarkable, which have been revealed by M. Planté's rheostatic machine. Among these are colored sparks and vibrations determined in a platinum wire traversed by a current of interrupted quantity, a phenomenon which can account for the effects produced in telephones by a simple wire crossed by a current.

Th. Du Marcel.

To be continued.

ON A PROCESS FOR UTILIZING WASTE PRODUCTS AND ECONOMIZING FUEL IN THE EXTRACTION OF COPPER.*

By J. Dixon (Adelaide, South Australia.)

This paper contains an account of a process for extracting copper from sulphurous ores, in which the heat generated by the combination of the oxygen of the air with the sulphur of the ore is utilized for the smelting of the ore. This process is based upon experiments, which, although the author regards as incomplete, show (1) that the charge grows visibly hotter by simply blowing air through it; (2) that the melting of the raw ore or regulus and its reduction can be carried on in the same furnace; (3) that if the ore is in lumps, and fed at the top whilst the air is admitted by the side, a practically clean slagg can be obtained; but if added in a coarse powder, as it is generally found in the market, it either blows out again or chokes the furnace; (4) that a rough copper of about 96 per cent pure metal can be obtained by the successful working of this process.

*British Association, 1881.

ON THE CHEMICAL ACTION BETWEEN SOLIDS.*

By Prof. Thorpe, Ph. D., F.R.S.

The author drew attention to the extremely rare instances of such action hitherto observed, showing how many of these might be explained on the supposition that combination actually occurred between the bodies either in solution or in a state of gas. For example, the formation of cement steel, by the combination of carbon with iron, which had long been adduced as an example of such combination between solids, was now explained by the fact that iron at a high temperature was permeable to gases, and that in the actual process of cementation oxides of carbon were formed, which were in reality conveyors of carbon to the metal. He then illustrated by experiments the formation of several compounds by bringing together the components in solid form, choosing as examples such as would manifest their formation by characteristic coloring. Thus, as instances, potassium iodide and mercuric chloride, potassium iodide and lead nitrate, and silver nitrate and potassium chromate, were powdered together in a mortar, and in each case evidence of an action was exhibited by the production of characteristic colors of the product of the reaction of these compounds. The author referred to the memoir of the Belgian phycisist, Prof. Spring, on the same subject, some of whose experiments he had repeated and in the main confirmed. One of the most remarkable results obtained by the Belgian professor was the formation of coal from peat by subjecting the latter material to a high pressure. Peat from Holland and Belgium, when exposed to a pressure of about 6,000 atmospheres, was, according to Spring, changed into a mass which in all physical characters resembled ordinary coal. Experiments of the same nature made by Dr. Thorpe with various samples of British peat yielded, however, a very dissimilar result. These experiments were made with pressures which were considerably less and more than those employed by Spring. Although solid, compact masses, hard and very much changed in structure, were attained, in no case was any product obtained which could be confounded with bituminous coal. He said it was highly improbable, on purely chemical grounds, that mere pressure had been little more than an important factor in the transformation of woody matter into coal.

A NEW DEMONSTRATION OF THE CARBONIC ACID OF THE BREATH.

By C. F. Cross.

Some time since I made the observation that the carbonic acid of the breath determines the liberation of iodine from a mixture of potassium iodide and iodate, and that the presence of starch renders the decomposition a very effective lecture-experiment, in demonstration of the presence of an active acid body in respired air. A friend to whom I lately communicated this result, threw doubt upon my interpretation, and while admitting the occurrence of the decomposition under the condition of respiring vigorously into the solution, preferred to attribute it to the action of the air or of acid vapors accidentally present. I therefore repeated the experiments

*British Association, 1881.

with special precautions, viz., washing the respired gases, and performing parallel experiments, in which, for the breath I substituted a rapid current of air, and lastly raising the latter to a temperature of 40° C. The result was to prove conclusively my original statement that the decomposition is brought about by a constituent of the respired air, and therefore by its carbonic acid. In performing this experiment it is only necessary to secure the neutrality of the solution; this being done, the development of a full purple color occupies from two to three minutes.

It is evident that this demonstration of the presence of *some* acid body precedes the lime-water test in the logical development of the complete proof of the presence of *carbonic* acid.—*Chemical News.*

THE BEST METHOD OF MOUNTING WHOLE CHICK EMBRYO.*

By Dr. Charles S. Minot.

The blastoderm is removed and cleaned in the usual manner, and then floated out on a glass slide, where it remains permanently. It is carefully spread out and allowed to dry until the edges become glued to the slide. It is then treated with a 0.5 per cent osmic acid solution, until a slight browning occurs. Stain with picro-carmine. The next step is particularly important, because it prevents the further darkening by the osmium, which otherwise injures or ruins the specimen. Pour Müller's fluid, or 0.5 per cent chromic acid solution, on the slide, and leave it over night. The next morning the blastoderm is ready for dehydration by alcohol, and mounting in the usual manner in balsam or dammar lac. Embryos prepared in this manner make particulary beautiful specimens.

ON THE ALLEGED DECOMPOSITION OF THE ELEMENTS.†

By Prof. Dewar, M.A., F.R.S.

In his remarks Prof. Dewar dealt chiefly with the spectroscopic work from which Mr. Norman Lockyer had drawn conclusions very different from those of Professors Liveing and Dewar, especially concerning the value of evidence on the subject. Prof. Dewar argued that Mr. Lockyer's views regarding the existence of carbon vapor in the corona of the sun would not bear scientific investigation, and that his views regarding the modification of the spectrum of magnesium were equally illusory, and gave no proof of the decomposition of elementary substances. Finally he discussed Mr. Lockyer's theory of "basic lines," and addressed himself to a refutation of the same. The results recorded, he said, strongly confirmed Young's observations, and left little doubt that the few as yet unresolved coincidences either would yield to a higher dispersion, or were merely accidental. It would indeed be strange if amongst all the variety of chemical elements and the still greater variety of vibrations which some of them were capable of taking up, there were no two which could take up vibrations of the same period. They certainly should have supposed that substances like iron and titanium, with such a large number of lines, must each consist of more than one kind of molecule, and that not single lines, but several lines of each, would be found repeated with the spectra of some other chemical elements. The fact that hardly a single coincidence could be established was a strong argument that the materials of iron and titanium, even if they be not homogeneous, were still different from those of other chemical elements. The supposition that the different elements might be resolved into simple constituents and even into a single substance had long been a favorite speculation with chemists; but however probable that hypothesis might appear *à priori*, it must be acknowledged, according to Prof. Dewar, that the facts derived from the most powerful method of analytical investigation yet devised, gave it but scant support.

* Read before the A. A. A. S., Cincinnati, 1881.

† British Association, 1881.

ASTRONOMY.

Elements and Ephemeris of Comet (*d*), 1881.—Barnard.

Mr. S. C. Chandler, Jr., has computed the following elements and ephemeris of Comet (*d*), 1881—Barnard—which are published, by permission of Prof. E. C. Pickering, of Harvard College Observatory. The observations upon which the computation is based are the following: Washington Mean Time being given with the Nashville observation, which was obtained at Vanderbilt University, by Prof. O. H. Landreth, and Cambridge Mean Time with the two others:

							R. A.			Decl.		
		d.	*h.*	*m.*	*s.*		*h.*	*m.*	*s.*	°	′	″
1881.	Sept.	20	7	46		Nashville	13	28	2	+3	47	
		21	7	34	43	Harvard Obs.	13	30	20	4	54	
		25	7	17	52	Harvard Obs.	13	36	29.63	9	6	43.7

The observation of the 20th was received by telegraph, and that of the 21st depends on only two comparisons, taken when the comet was but one degree and a half above the horizon.

ELEMENTS.

T = 1881, September, 14.785. Washington Mean Time.

$$\left.\begin{array}{rll} & ° & ′ \\ \pi = & 271 & 22 \\ \Omega = & 260 & 43 \\ i = & 107 & 27 \end{array}\right\} \text{Mean Eq., 1881. 0.}$$

$$\log. q = 9.7053$$

EPHEMERIS.

Wash. midnight, 1881.	R. A. *h.*	*m.*	*s.*	Decl. °	′	Log. *r*.	Log. Δ.	Light
Sept. 29	13	41	36	+13	4	9.7894	0.1350	1.00
Oct. 3	13	45	28	16	26	9.8270	0.1467	.80
7	13	48	40	19	29	9.8648	0.1569	.65
11	13	51	32	22	18	9.9014	0.1628	.52

The light of the comet on September 29 is taken as unity, and in this scale its light at discovery, on September 17, was 1.85. The orbit does not resemble that of any known comet.

The comet is circular, not over one minute of arc in diameter, with a very decided central condensation. Its collective brightness is not more than equivalent to that of an 8½ mag. star. The comet is rapidly decreasing in light, and the moon is advancing, so that observations of it at once are very desirable. So far as is known, positions have been obtained only at Nashville and Cambridge, the early setting of the comet, and clouds, having greatly interfered. Under the circumstances, the orbit cannot be other than a rough one, and considerable latitude for error had better be allowed in searching for it.

MICROSCOPY.

The following method of hardening the spinal cord for microscopic sections has been highly recommended by Dr. M. Debove:

Place the cord in a 4 per cent solution of bichromate of ammonia for three weeks, then in a solution of phenic gum for three days, and for three days more in alcohol. Sections may then be cut with great facility. They should be placed in water to prevent curling. They are then immersed in a saturated solution of picric acid for twenty-four hours, and colored with carmine for about twenty minutes, the picric acid acting as a mordant.—*Archives de Neurologie.*

An era of microtomes appears to be approaching, and numerous are such devices which are advertised by the opticians. Mr. Thomas Taylor of the Agricultural de-

partment, Washington, has arranged a new microtome in which all the parts are reduced to their most simple form. Mr. Taylor described his invention as consisting essentially of a thin brass tube about one inch and a half strength by one inch in diameter. A ¼ inch brass tube secured within the large cylinder. This tube enters the bottom where it is secured, and proceeds to within a quarter of an inch of the inside surface of the top. To the outside open end of this tube a rubber tube is attached; the other end of the rubber tube is made to communicate with a freezing mixture composed of finely cut ice and salt in about equal proportions. The pail containing this mixture is placed over and about fifteen inches higher than the section cutter. The object of this arrangement is to fill the brass cylinder with a freezing liquid drained from the pail, and caused by the liquefying salt and ice, the temperature of which is about zero. On filling the cylinder with the liquid any object on the top of the cylinder becomes frozen in a short period and may then be cut to any degree of thickness. In order to preserve the low degree of temperature in the cylinder a second tube is secured in the cylinder to remove air and keep up a constant current of the freezing liquid. This tube enters the bottom of the cylinder, where it is fastened. It projects upwards to within an eighth of an inch of the top and has a diameter of about one-half of the supply tube. This microtome or freezing cylinder in other respects is arranged like other microtomes, such as are used for ether or rhigoline; and the same mathematical accuracy attained in cutting sections,

THE editor of the *American Monthly Microscopical Journal* devotes an article to the selection of microscopes, and expresses his belief that the microscope of the future will be an instrument of quite moderate size, and about the same dimensions as that of the forms used by the German student. We believe this to be a correct view of the microscopist's requirements, if the instrument is employed as often as it should be. The colossal instruments which have been recently constructed show no advance in the manufacture of microscopes, but rather a return to the monstrosities of 100 years ago, when their size was "prodigious," and the display of ornamentation profuse. We once saw the microscope "built" for George III., which was a marvel of the brass finisher's art, as elaborate as a Louis XIV. clock, and probably as useful, as an optical instrument.

We believe the form of microscope which will be accepted as a standard by future microscopists will be the "Stevenson" model. Five years ago we submitted drawings for an inexpensive instrument on this plan, but was met by a variety of objections from opticians.

We now find that two London makers are offering microscopes on this model, the Stevenson form having been modified, so as to considerably reduce the expense. The advantages of this model is very great. *First,* a horizontal stage. *Second,* the comfort of sloping tubes. *Third,* an erected image.

We notice in the new edition of "Carpenter" (page 86) that such an instrument (binoculen) can be sold complete, with objective, for $100, or simplified as a student's microscope (binoculen, with 2 objectives) for $64.

For those who merely practice the refinements of the microscope, such an instrument would present many objections, but for biological studies and ordinary microscopical work, we strongly advocate its use, and desire to find it manufactured in its new and cheaper form by American manufacturers of microscopes.

PROF. ALEXIS A. JULIEN has published a reprint from the Journal of the Amer. Chemical Society of his paper "*On the Examination of Carbon Dioxide in the Fluid Cavities of Topaz.*" He describes two simple and inexpensive apparatus for the microscopical determination of carbonic acid in the cavities of minerals; and a recent study of large numbers of cleavage slices from fifty pebbles of topaz from Minas Geraes, Brazil, has presented facts of some interest hitherto not recorded. In some of the slices many extremely angular, elongated, branching, and even reticulated forms of considerable size and novelty abound. Their outlines are at many points decidedly crystalline, with arms projecting at an angle of about 135 degrees, which seems to indicate that Brewster's generalization, that the cavities were generally "capriciously distributed when the substance of the crystal was in a soft or plastic state," may have been pressed too far. In general, the larger expansions of the cavities are mostly occupied by brine, while their attenuated extremities and fine tubular connections are filled by liquid carbonic acid, occasionally including a bubble due to contraction.

MR. C. M. VORCE has forwarded to us a drawing of the many forms of microscopical life found by him in water from Lake Erie, and used as a water supply for Cleveland City. This appears to be but the first instalment of the subject. He draws and names nearly two hundred specimens.

PRELIMINARY REMARKS ON THE MICROSCOPIC STRUCTURE OF COAL FROM EAST SCOTLAND AND SOUTH WALES, by Prof. Williamson, F. R. S., Owens College.—This subject will not be worked out until ten years, but he described layers of vascular tissue which can be separated layer by layer, while in other cases the charcoal layer on the surface of the coal and the organic structure is not capable of separation, and he stated that charcoal contains a tubular structure, like tissues of ordinary bark. The association of tissues resembles that of Cycadian plants; and referred to the genus *Cordaites* having been proved to belong to this group by M. Renault; the author has made nearly a thousand distinct observations on the structure of coal. Separates ordinary coal with large quantities of mineral charcoal, with macrospores of Lepidendroid plants filled up with myriads of microspores which were certainly not floated to the spots, from the *paraffine coals* which do not contain these large macrospores. He divides coal into "Iso-sporous" coals and "Heterosporous" coals; both abound in *Cordaites,* which form the mineral charcoal.

NOTE ON THE OCCURRENCE OF SELENIUM AND TELLURIUM IN JAPAN, by E. DIVERS, M. D.—The author draws attention to the fact that the presence of these two elements has been observed in Japanese sulphuric acid, and considers it probable that these substances occur in material quantities in Japan.

BREWING IN JAPAN, by R. W. ATKINSON, B.SC. (LOND.)—The Japanese brewing process is divided into two parts comparable with the malting and brewing processes of beer-making. The mode of preparation and the properties of the diastatic materials are different in the two cases. The Japanese equivalent of malt or "kōji" hydrates maltose in addition to cane-sugar, dextrin, and starch, and the ultimate products of its action on starch-paste are dextrose and dextrin, or perhaps dextrose alone. Kōji differs from malt in being rendered inactive by heat at a much lower temperature than malt. Kōji is prepared as follows: A mixture of steamed rice and water is allowed to remain in shallow tubs at a low temperature (0°–5° C.) until quite liquid; it is then heated, fermentation commences, and continues until nearly all the dextrine first formed is exhausted. This product is now used like yeast, and is added to fresh quantities of steamed rice and water, fermentation proceeding until the percentage of alcohol amounts to about 13 or 14 per cent by weight. After the greater part of the rice added has been used up, the mash is filtered and clarified by standing. The "saké" so produced requires very careful watching, and when summer approaches, or it exhibits signs of putrefactive fermentation, it is then heated in iron vessels; this operation has frequently to be repeated. Analyses of various specimens, fresh and diseased, are given in the paper.

BOOKS RECEIVED.

A TREATISE ON BRIGHT'S DISEASE AND DIABETES, WITH SPECIAL REFERENCE TO PATHOLOGY AND THERAPEUTICS. By JAMES TYSON, A. M., M. D., with Illustrations. Including a section on Retinitis in Bright's Disease. By WILLIAM F. NORRIS., A. M , M. D. Lindsay and Blackiston, Philadelphia. 1881.

Dr. Tyson needs no apology for publishing this work, and we express the hope that it will be extensively read by the medical profession.

It is true that many excellent treatises exist on this subject, but a mere glance at Dr. Tyson's work shows that in it the subject has been treated in a manner that is original, presenting all the facts in a concise form, and yet omitting no detail which is essential for the full comprehension of this intricate and difficult subject.

Those who have watched the course of recent literature relating to Diseases of the Kidneys and Glycosuria, are aware that we are very far from possessing precise knowledge in regard to such complications, and that while we are still ignorant of the precise pathology of some of these diseases, the very facts bearing on the subject are in a chaotic condition, and inaccessible to the majority of those who should be well informed.

The writer of this book is an accomplished writer, and one who has during the past fifteen years devoted his thoughts and studies to these subjects, and also engaged in practical work bearing on them. Surely the result of such an experience must be useful to both experts and students, if properly used and appreciated.

The number of calls made upon physicians by patients suffering from various forms of Bright's disease and Diabetes is daily on the increase, and no one knows better than the intelligent practitioner, that a large number of their confrères are miserably ignorant on the subject, unacquainted with the Pathology of these diseases, and disgracefully incompetent to treat them. We are not now speaking of quacks, but holders of medical diplomas.

Cases have come to our knowledge, where patients have succumbed on account of their physicians being unable to make a proper diagnosis of the diseases we refer to, or even to analyze or report on a sample of urine.

To such Dr. Tyson's work will probably still be a sealed book, but the advanced and intelligent physicians will, under our advice, procure a copy, for, although other works on this subject may have been studied with profit, we believe a perusal of the work before us will still contribute to his knowledge of this important and interesting subject.

SOLAR PHYSICS.—In concluding a series of lectures on Solar Physics, Professor J. Norman Lockyer said: "I am in honor bound to say, as the result of the work on our solar physics, in that small branch of the inquiry into solar matters with which I am more personally connected, that my belief is that the late work has changed the views which were held, say twenty years ago, to this extent: whereas twenty years ago, we imagined ourselves to be in full presence in the sun of chemical forms with which we are familiar here, I think in this present year, we are bound to consider that that view may be modified to a certain extent, and that we are justified in holding the view, that not these chemical forms with which we are acquainted here, but their germs really, are revealed to us in the hottest regions of the sun."

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 1, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

SEPTEMBER AND OCTOBER.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 25	29.936	30.000	7 a. m.	29.892	5 p. m.	79.3	70.3	89	4 p. m.	73	4 p. m.	71	6 a. m.	67	7 a. m.	143.
Monday, 26	29.943	29.986	9 a. m.	29.900	5 p. m.	81.6	72.0	91	3 p. m.	75	3 p. m.	73	7 a. m.	69	7 a. m.	145.
Tuesday, 27	29.937	29.994	9 a. m.	29.900	9 p. m.	79.7	72.7	86	3 p. m.	76	3 p. m.	75	5 a. m.	70	5 a. m.	139.
Wednesday, 28	29.933	30.018	12 p. m.	29.890	3 p. m.	78.0	70.0	88	4 p. m.	73	5 p. m.	71	12 p. m.	68	12 p. m.	143.
Thursday, 29	30.196	30.238	9 p. m.	30.018	0 a. m.	69.6	64.3	75	3 p. m.	66	3 p. m.	64	12 p. m.	62	12 p. m.	134.
Friday, 30	30.189	30.238	9 a. m.	30.108	5 p. m.	74.0	68.6	82	3 p. m.	74	3 p. m.	64	0 a. m.	62	0 a. m.	138.
Saturday, 1	30.156	30.198	12 p. m.	30.100	3 p. m.	79.0	69.6	87	3 p. m.	72	6 p. m.	72	4 a. m.	67	0 a. m.	141.

Mean for the week .... 30.042 inches.
Maximum for the week at 9 a. m., Sept. 30th .... 30.238 "
Minimum " at 3 p. m., Sept. 28th .... 29.890 "
Range .... .348 "

	Dry.		Wet.
Mean for the week	77.3 degrees		69.6 degrees.
Maximum for the week	at 3 pm. 26th 91. "	at 3 pm 27th,	76. "
Minimum "	" 12 pm. 29th 64. "	at 12 pm 29th,	62. "
Range "	" 27. "		14. "

SEPTEMBER AND OCTOBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 25	w. s. w	s. w.	s. w.	233	5½	3.00 pm	.595	.596	.677	76	48	66	0	2 cir. cu.	0	-----	-----	-----	--	0
Monday, 26	w.	s. s. w.	s. w.	177	1¾	3.00 pm	.655	.665	.650	80	47	59	0	2 cu.	0	-----	-----	-----	--	0
Tuesday, 27	s. s. w	s.	s. w.	145	3¼	2.30 pm	.652	.733	.744	72	61	77	2 cir.	3 cir. cu.	2 cu. s.	4.30pm	5.30pm	1.00	.04	0
Wednesday, 28	w. s. w.	w.	n.	221	7¼	5.40 pm	.666	.596	.618	77	48	76	4 cir. cu.	4 cu.	2 cu.	-----	-----	-----	--	0
Thursday, 29	e. n. e.	e.	e. s. e.	174	3½	10.20 am	.516	.545	.536	70	67	84	2 cir. cu.	7 cir. cu.	10	-----	-----	-----	--	0
Friday, 30	n. e.	s.	s. w.	146	6¾	7.00 pm	.569	.703	.628	89	66	72	10	2 cu.	0	-----	-----	-----	--	0
Saturday, 1	w. s. w.	w. s. w.	s. w.	199	3	7.20 am	.631	.518	.651	80	41	66	5 cir. cu.	2 cir. cu.	0	-----	-----	-----	--	0

Distance traveled during the week .... 1,295 miles.
Maximum force .... 7¼ lbs.

Total amount of water for the week .... .04 inch
Duration of rain .... 1 hours, 00 minutes

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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JOHN MICHELS, Editor.

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ILLUSIONS.*

In reality this work might have been styled an essay on error, for the author deals, in his clear and masterly way, with other errors of the human judgment than those which are termed illusions in the narrower sense of that term. His essay loses nothing, and gains much by thus occupying a much broader field than the one, furnished by the sensory illusion, would constitute *per se.* Perhaps the most unfortunate part of the work, is the opening passage: "Common sense, knowing nothing of fine distinctions, is wont to draw a sharp line between the region of illusion and that of sane intelligence. To be the victim of an illusion is, in the popular judgment, to be excluded from the category of rational men. The term at once calls up images of stunted figures with ill-developed brains, half-witted creatures, hardly distinguishable from the admittedly insane. . . . . . The nineteenth century intelligence plumes itself on having got at the bottom of mediæval visions and church miracles, and it is wont to commiserate the feeble minds that are still subject to these self-deceptions."

We say this passage is an unfortunate one, and this particularly because of its position in the opening chapter of a book which, as we must particularly emphasize, is throughout one of the clearest and most readable psychological treatises that we have found in the English language; this passage on the other hand, is as full of wrong assumptions, misconstructions, and errors as a single paragraph can well be. The popular mind fails to contemn the bearer of an illusion, as it does the bearer of a delusion; the mediæval visions were not, even in popular parlance illusions, but hallucinations, and indeed the popular sense in which the term illusion is used, that is, the one employed by poets and classical writers, is anything but a reflection on the bearer of the illusion. The day-dream, the poetic illusion, and the constructions of a sanguine temperament, are the objects associated in the lay-mind with that term.

On the fourth page is further evidence that the author has failed to discriminate practically between delusions, hallucinations, and illusions. After stating that alienists have good reason to limit the word illusion to illusory perceptions, he adds "such illusions of the senses are the most palpable and striking evidences of mental disease." Inasmuch as illusions are common with the sane, it is incorrect to lay greater stress on the not very frequent illusions of the insane, than on the marked and characteristic hallucinations and the still more universal delusions of that class.

The author defines an illusion as a species of error which counterfeits the form of immediate, self-evident, or intuitive knowledge whether as a sense perception or otherwise. Further on he discriminates between the illusion and the fallacy, by characterising the former as a falsification of primary or intuitive knowledge, and the latter as a falsification of secondary or inferential knowledge. It must be admitted that the author is happier in his discrimination than in his definition, and an illustration of the difficulty under which definers labor recurs in the peroration of the same chapter, where he says that the illusion is seen to arise through "some exceptional feature in the situation or condition of the individual, which, for the time, breaks the chain of intellectual solidarity which under ordinary circumstances binds the single member to the collective body." The greater portion of this passage would constitute an excellent nucleus for a definition of insanity, but at the same time it seems to us that it fails to cover those common illusions, which involve the visual apparatus, and of which familiar illustrations are furnished in most physiological text books. The dividing line between the delusion, the hallucination, and the illusion, should have been strictly drawn at the outset, by our author. We have offered the following as showing the difference between the hallucination and the illusion: While a hallucination is a subjective perception of an object as a real presence, without a real presence to justify the perception, and a memory is the subjective per-

* *Illusions, a psychological study.* By JAMES SULLY. New York, D. Appleton and Co.—Volume XXXIII. of the International Scientific Series.

ception of an object not actually present, involving the recognition of its absence, an illusion is the subjective perception of an object actually present, but in characters which the object does not really possess. With appropriate alterations these definitions will cover the abstract hallucination and phantastic illusion of Wundt as well.

In his second chapter, the author ably, but we believe unsuccessfully, endeavors to defend his refusal to recognize the distinction between illusion and hallucination as the leading principle of classification, though he admits the necessity of making this distinction in accordance with the leading alienists. Wundt, an authority whose teachings in psychological physiology the author of the present volume has most successfully assimilated, has drawn attention to the numerous connecting links existing between illusions and hallucinations, and yet strongly insists on utilizing their general differences as a basis of classification. We find the chief drawback to the otherwise great value of the work, in its failure to give adequate space to the anatomical mechanism concerned in false registrations of the perceptional and conceptional sphere. If it be borne in mind that while even hallucinations may be based on actual impressions, the latter are not the determining factor of the hallucination, the difficulty in discriminating between these perversions is overcome; this is illustrated by the occasional persistence of dream-images in the waking state, and the moving of certain hallucinated images consonant with the movements of the eye-ball. If an actual or subjective impression, say in the shape of *chromatopsia* and *tinnitus*, be granted to exist in a subject hallucinating the vision and voice of the Virgin Mary, it will be instantly recognized by every observant alienist, that the real determining factor is here centrifugal, while in the illusion, which constructs, out of a ball rolling in an ill-lighted apartment, a mouse, the determining factor takes a centripetal course. In the former instance, the misinterpretation lies ready made in the Cortex, and seizes on the slight external pretext, whose existence we only admit for the sake of the argument, to incorporate it, in its substance; in the latter, it is based upon an imperfect registration and a gradual constructive interpreting process. Nothing could more forcibly illustrate the correctness of these propositions than the very case cited from Wundt by Mr. Sully of a forester who saw the real objects of the outer world, (furniture and tapestry, for example,) through the wood piles which formed the subjects of his hallucinations.

With these remarks on the propositions of the opening chapters, our criticism ceases to be adverse. In the last twelve chapters of the book, the author gives a concise review of the chief theories held by alienists and metaphysicians on the perceptional illusion, the introspective illusion, dreams illusions of memory, and those of belief. We refrain from again pointing out places where the author encroaches on the fields of delusion and hallucination, as he has given a wider scope to his definition of the illusion, than we are inclined to consider proper. It is but just to say that he gives a just interpretation to the views of alienists, an interpretation which only occasionally manifests that tincture of uncertainty which is unavoidable on the part of one devoid of a practical knowledge of the insane.

The perusal of this work cannot fail to be profitable to the student of mental pathology as well as of metaphysics. More reliable in the latter field, than in the former, it is yet a successful attempt to present the modern German ideas on the subject, and to combine the teachings of the practical and the abstract psychologists. To the general reader we can only repeat, what we said at the outset, it is the clearest rendition of a difficult yet fascinating theme, to be found in our language.

E. C. SPITZKA, M. D.

ON THE DISCOVERIES OF THE PAST HALF-CENTURY RELATING TO ANIMAL MOTION.

By J. BURDON-SANDERSON, M.D., LL.D., F.R.S.

The two great branches of Biology with which we concern ourselves in this section, Animal Morphology and Physiology, are most intimately related to each other. This arises from their having one subject of study—the living animal organism. The difference between them lies in this, that whereas the studies of the anatomist lead him to fix his attention on the organism itself, to us physiologists it, and the organs of which it is made up, serve only as *vestigia*, by means of which we investigate the vital processes of which they are alike the causes and consequences.

To illustrate this I will first ask you to imagine for a moment that you have before you one of those melancholy remainders of what was once an animal—to wit, a rabbit—which one sees exposed in the shop of poulterers. We have no hesitation in recognising that remainder as being in a certain sense a rabbit; but it is a very miserable vestige of what was a few days ago enjoying life in some wood or warren, or more likely on the sand-hills near Ostend. We may call it a rabbit if we like, but it is only a remainder—not the thing itself.

The anatomical preparation which I have in imagination placed before you, although it has lost its inside and its outside, its integument and its viscera, still retains the parts for which the rest existed. The final cause of an animal, whether human or other, is muscular action, because it is by means of its muscles that it maintains its external relations. It is by our muscles exclusively that we act on each other. The articulate sounds by which I am addressing you are but the results of complicated combinations of muscular contractions—and so are the scarcely appreciable changes in your countenances by which I am able to judge how much, or how little, what I am saying interests you.

Consequently the main problems of physiology relate to muscular action, or as I have called it, animal motion. They may be divided into two—namely (1) in what does muscular action consist—that is, what is the process of

which it is the effect or outcome? And (2) how are the motions of our bodies co-ordinated or regulated? It is unnecessary to occupy time in showing that, excluding those higher intellectual processes which, as they leave no traceable marks behind them, are beyond the reach of our methods of investigation, these two questions comprise all others concerning animal motion. I will therefore proceed at once to the first of them—that of the process of muscular contraction.

The years which immediately followed the origin of the British Association exceeded any earlier period of equal length in the number and importance of the new facts in morphology and physiology which were brought to light; for it was during that period that Johannes Müller, Schwann, Henle, and, in this country, Sharpey, Bowman, and Marshall Hall, accomplished their productive labors. But it was introductory to a much greater epoch. It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologist—*e.g.* J. Müller—recognized that the knowledge they possessed, both of vital and physical phenomena, was insufficient to refer both to a common measure. The method, therefore, was to study the process of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all, unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. Let us inquire for a moment what causes have conduced to the change.

The most efficient cause was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the Conservation of Energy. In the application of this great principle to physiology, the men to whom we are indebted are, first and foremost, J. R. Mayer, of whom I shall say more immediately; and secondly to the great physiologists still living and working among us, who were the pupils of J. Müller—viz.; Helmholtz, Ludwig, Du Bois-Reymond, and Brücke.

As regards the subject which is first to occupy our attention, that of the *process* of muscular contraction, J. R. Mayer occupies so leading a position that a large proportion of the researches which have been done since the new era, which he had so important a share in establishing, may be rightly considered as the working out of principles enunciated in his treatise [1] on the relation between organic motion and exchange of material. The most important of these were, as expressed in his own words: (1) "That the chemical force contained in the ingested food and in the inhaled oxygen is the source of the motion and heat which are the two products of animal life; and (2) that these products vary in amount with the chemical process which produces them." Whatever may be the claims of Mayer to be regarded as a great discovery in physics, there can be no doubt, that as a physiologist, he deserves the highest place that we can give him, for at a time when the notion of the correlation of different modes of motion was as yet very unfamiliar to the physicist, he boldly applied it to the phenomena of animal life, and thus re-united physiology with natural philosophy, from which it had been rightly, because unavoidably, severed by the vitalists of an earlier period.

Let me first endeavor shortly to explain how Mayer himself applied the principle just enunciated, and then how it has been developed experimentally since his time.

The fundamental notion is this: the animal body resembles, as regards the work it does and the heat it produces, a steam-engine in which fuel is continually being used on the one hand, and work is being done and heat produced on the other. The using of fuel is the chemical process, which in the animal body, as in the steam engine, is a process of oxidation. Heat and work are the useful products, for as, in the higher animals, the body can only work at a constant temperature of about 100° F., heat may be so regarded.

Having previously determined the heat and work severally producible by the combustion of a given weight of carbon, from his own experiments and from those of earlier physicists, Mayer calculated that if the oxidation of carbon is assumed to represent approximately the oxidation process of the body, the quantity of carbon actually burnt in a day is far more than sufficient to account for the day's work, and that of the material expended in the body not more than one-fifth was used in the doing of work, the remaining four-fifths being partly used, partly wasted in heat production.

Having thus shown that the principles of the correlation of process and product hold good, so far as its truth could then be tested, as regards the whole organism, Mayer proceeded to inquire into its applicability to the particular organ whose function it is "to transform chemical difference into mechanical effect"—namely muscle. Although, he said, a muscles acts under the direction of the will, it does not derive its power of acting from the will, any more than a steamboat derives its power of motion from the helmsman. Again (and this was of more importance, as being more directly opposed to the prevalent vitalism), a muscle, like the steamboats use in the doing of work, not the material of its own structure, or mechanism, but the fuel—*i. e.* the nutriment—which it derives directly from the blood which flows through its capillaries. "The muscle is the instrument by which the transformation of force is accomplished, not the material which is itself transformed." This principle he exemplified in several ways, showing that if the muscles of our bodies worked, as was formerly supposed, at the expense of their own substance, their whole material would be used up in a few weeks, and that in the case of the heart, a muscle which works at a much greater rate than any other, it would be expended in as many days—a result which necessarily involved the absurd hypothesis that the muscular fibres of our hearts are so frequently disintegrated and reintegrated that we get new hearts once a week.

On such considerations Mayer founded the prevision, that, as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the limits of the chemical process, either in the whole animal body or in a single muscle, during a given period, and to measure the production of heat and the work done during the same period, the result would show a quantitative correlation between them.

If the time at our disposal permitted, I should like to give a short account of the succession of laborious investigations by which these previsions have been verified. Begun by Bidder and Schmidt in 1851,[1] continued by Pettenkofer and Voit,[2] and by the agricultural physiologists[3] with reference to herbivora, they are not by any means completed. I must content myself with saying that by these experiments the first and second parts of this great subject—namely, the limits of the chemical process of animal life and its relation to animal motion under different conditions—have been satisfactorily worked out, but that the quantitative relations of heat production are as yet only insufficiently determined.

Let me sum up in as few words as possible how far what we have now learnt by experiment justifies Mayer's anticipations, and how it falls short of or exceeds them. First of all, we are

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel; ein Beitrag zur Naturkunde,' Heilbronn, 1845.

[1] Bidder and Schmidt, "Die Verdauungssäfte und der Stoffwechsel," Leipzig, 1852.

[2] Pettenkofer and Voit, *Zeitschr. f. Biologie*, passim, 1866–80.

[3] Henneberg and Stohmann, "Beiträge zur Begründung einer rationellen Futterung der Wiederkäuer," Brunswick and Göttingen, 1860–70.

as certain as of any physical fact that the animal body in doing work does not use its own material— that, as Mayer says, the oil to his lamp of life is food; but in addition to this we know what he is unaware of, that what is used is not only not the living protoplasm itself, but is a kind of material which widely differs from it in chemical properties. In what may be called commercial physiology —*i.e.*, in the literature of trade puffs—one still meets with the assumption that the material basis of muscular motion is nitrogenous; but by many methods of proof it has been shown that the true "Oel in der Flamme des Lebens" is not proteid substance, but sugar, or sugar-producing material. The discovery of this fundamental truth we owe first to Bernard (1850-56), who brought to light the fact that such material plays an important part in the nutrition of every living tissue; secondly, to Voit (1866), who in elaborate experiments on carnivorous animals, during periods of rest and exertion, showed that, in comparing those conditions, no relation whatever shows itself between the quantity of proteid material (flesh) consumed, and the amount of work done; and finally to Frankland, Fick, and his associate Wislicenus, as to the work-yielding value of different constituents of food, and as to the actual expenditure of material in man during severe exertion. The subjects of experiment used by the two last-mentioned physiologists were themselves; the work done was the mountain ascent from Interlaken to the summit of the Faulhorn; the result was to prove that the quantity of material used was proportional to the work done, and that that material was such as to yield water and carbonic acid exclusively.

The investigators to whom I have just referred aimed at proving the correlation of process and product for the whole animal organism. The other mode of inquiry proposed by Mayer, the verification of his principle in respect of the work-doing mechanism—that is to say, in respect of muscle taken separately—has been pursued with equal perseverance during the last twenty years, and with greater success; for in experimenting on a separate organ, which has no other functions excepting those which are in question, it is possible to eliminate uncertainties which are unavoidable when the conditions of the problem are more complicated. Before I attempt to sketch the results of these experiments, I must ask your attention for a moment to the discoveries made since Mayer's epoch, concerning a closely related subject, that of the Process of Respiration.

I wish that I had time to go back to the great discovery of Priestley (1776), that the essential facts in the process of respiration are the giving off of fixed air, as he called it, and the taking in of dephlogisticated air, and to relate to you the beautiful experiments by which he proved it; and then to pass on to Lavoisier (1777), who, on the other side of the Channel, made independently what was substantially the same discovery a little after Priestley, and added others of even greater moment. According to Lavoisier, the chemical process of respiration is a slow combustion which has its seat in the lungs. At the time that Mayer wrote, this doctrine still maintained its ascendency, although the investigations of Magnus (1838) had already proved its fallacy. Mayer himself knew that the blood possessed the property of conveying oxygen from the lungs to the capillaries, and of conveying carbonic acid gas from the capillaries to the lungs, which was sufficient to exclude the doctrine of Lavoisier. Our present knowledge of the subject was attained by two methods—viz., first, the investigation of the properties of the coloring matter of the blood, since called "hæmoglobin," the initial step in which was made by Prof. Stokes in 1862; and secondly, the application of the mercurial air-pump as a means of determining the relations of oxygen and carbonic acid gas to the living blood and tissues. The last is a matter of such importance in relation to our subject that I shall ask your special attention to it. Suppose that I have a barometer of which the tube, instead of being of the ordinary form, is expanded at the top into a large bulb of one or two litres capacity, and that, by means of some suitable contrivance, I am able to introduce, in such a way as to lose no time and to preclude the possibility of contact with air, a fluid ounce of blood from the artery of a living animal into the vacuous space—what would happen? Instantly the quantity of blood would be converted into froth, which would occupy the whole of the large bulb. The color of the froth would at first be scarlet, but would speedily change to crimson. It would soon subside, and we should then have the cavity which was before vacuous occupied by the blood and its gases—namely, the oxygen, carbonic acid gas, and nitrogen previously contained in it. And if we had the means (which actually exist in the gas-pump) of separating the gaseous mixture from the liquid, and of renewing the vacuum, we should be able to determine (1) the total quantity of gases which the blood yields, and (2), by analysis, the proportion of each gas.

Now, with reference to the blood, by the application of the "blood-pump," as it is called, we have learned a great many facts relating to the nature of respiration, particularly that the difference of venous arterial blood depends not on the presence of "effete matter," as used to be thought, but on the less amount of oxygen held by its coloring matter, and that the blood which flows back to the heart from different organs, and at different times, differs in the amount of oxygen and of carbonic acid gas it yields, according to the activity of the chemical processes which have their seat in the living tissues from which it flows.[1] But this is not all that the blood-pump has done for us. By applying it not merely to the blood, but to the tissues, we have learned that the doctrine of Lavoisier was wrong, not merely as regards the place, but as regards the nature of the essential process in respiration. The fundamental fact which is thus brought to light is this, that although living tissues are constantly and freely supplied with oxygen, and are in fact constantly tearing it from the hæmoglobin which holds it, yet they themselves yield no oxygen to the vacuum. In other words, the oxygen which living protoplasm seizes upon with such energy that the blood which flows by it is compelled to yield it up, becomes so entirely part of the living material itself that it cannot be separated even by the vacuum. It is in this way only that we can understand the seeming paradox that the oxygen, which is conveyed in abundance to every recess of our bodies by the blood-stream, is nowhere to be found. Notwithstanding that no oxidation-product is formed, it becomes latent in every bit of living protoplasm; stored up in quantity proportional to its potential activity—*i. e.*, to the work, internal or external, it has to do.

Thus you see that the process of tissue respiration—in other words, the relation of living protoplasm to oxygen—is very different from what Mayer, who localized oxidation in the capillaries, believed it to be. And this difference has a good deal to do with the relation of Process to Product in muscle. Let us now revert to the experiments on this subject which we are to take as exemplification of the truth of Mayer's forecasts.

If I only desired to convince you that during the last half-century there has been a greater accession of knowledge about the function of the living organism than during the previous one, I might arrange here a small heap at one end of the table the physiological works of the Hunters, Spallanzani, Fontana, Thomas Young, Benjamin Brodie, Charles Bell, and others, and then proceed to cover the rest of it with the records of original research on physiological subjects since 1831, I should find that, even if I included only genuine work, I should have to heap my table up to the ceiling. But I apprehend this would not give us a true answer to our question. Although, etymologically, Science and Knowledge mean the same thing, their real meaning is different. By science we mean, first of all,

that knowledge which enables us to sort the things known according to their true relations. On this ground we call Haller the father of physiology, because, regardless of existing theories, he brought together into a system all that was then known by observation or experiment as to the processes of the living body. But in the "Elementa Physiologiæ" we have rather that out of which science springs than science itself. Science can hardly be said to begin until we have by experiment acquired such a knowledge of the relation between events and their antecedents, between processes and their products, that in our own sphere we are able to forecast the operations of nature, even when they lie beyond the reach of direct observation. I would accordingly claim for physiology a place in the sisterhood of the sciences, not because so large a number of new facts have been brought to light, but because she has in her measure acquired that gift of prevision which has been long enjoyed by the higher branches of natural philosophy. In illustration of this I have endeavored to show you that every step of the laborious investigations undertaken during the last thirty years as to the process of nutrition, has been inspired by the provisions of J. R. Mayer, and that what we have learnt with so much labor by experiments on animals is but the realization of conceptions which existed two hundred years ago in the mind of Descartes as to the mechanism of the nervous system. If I wanted another example I might find it in the provisions of Dr. Thomas Young as to the mechanism of the circulation, which for thirty years were utterly disregarded, until, at the epoch to which I have so often adverted, they received their full justification from the experimental investigations of Ludwig.

But perhaps it will occur to some one that if physiology founds her claim to be regarded as a science on her power of anticipating the results of her own experiments, it is unnecessary to make experiments at all. Although this objection has been frequently heard lately from certain persons who call themselves philosophers, it is not very likely to be made seriously here. The answer is, that it is contrary to experience. Although we work in the certainty that every experimental result will come out in accordance with great principles (such as the principle that every plant or animal is both, as regards form and function, the outcome of its past and present conditions, and that in every vital process the same relations obtain between expenditure and product as hold outside of the organism), these principles do little more for us than indicate the direction in which we are to proceed. The history of science teaches us that a general principle is like a ripe seed, which may remain useless and inactive for an indefinite period, until the conditions favorable to its germination come into existence. Thus the conditions for which the theory of animal automatism of Descartes had to wait two centuries, were (1) the acquirement of an adequate knowledge of the structure of the animal organism, and (2) the development of the sciences of physics and chemistry; for at no earlier moment were these sciences competent to furnish either the knowledge or the methods necessary for its experimental realization; and for a reason precisely similar Young's theory of the circulation was disregarded for thirty years.

I trust that the examples I have placed before you today may have been sufficient to show that the investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life, and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

The Science of Physiology has already afforded her aid to the Art of Medicine in furnishing her with a vast store of knowledge obtained by the experimental investigation of the action of remedies and of the causes of diseases. These investigations are now being carried on in all parts of the world with great diligence, so that we may confidently anticipate that during the next generation the progress of pathology will be as rapid as that of physiology has been in the past, and that as time goes on the practice of medicine will gradually come more and more under the influence of scientific knowledge. That this change is already in progress we have abundant evidence. We need make no effort to hasten the process, for we may be quite sure that, as soon as science is competent to dictate, art will be ready to obey.

METEORIC DUST.

By Prof. Schuster.

A committee of the British Association was appointed for the double purpose of examining the observations hitherto recorded on the subject of meteoric dust and of discussing the possibility of future more systematic investigations. With regard to the first point we note that in a paper presented to the Royal Astronomical Society in 1879, Mr. Ranyard has given what appears to be a pretty complete account of the known observations as to the presence of meteoric dust in the atmosphere. It appears that in the year 1852 Prof. Andrews found native iron in the basalt of the Giant's Causeway. Nordenskjöld found particles of iron which in all probability had a cosmic origin in the snows of Finland and in the ice-fields of the Arctic regions. Dr. T. L. Phipson, and more recently Tissandier, found similar particles deposited by the winds on plates exposed in different localities. Finally, Mr. John Murray discovered magnetic particles raised from deposits at the bottom of the sea by H. M. S. *Challenger*. These articles were examined by Prof. Alexander Herschel, who agreed with Mr. Murray in ascribing a cosmic origin to them. For fuller details and all references we must refer to Mr. Ranyard's paper. There cannot be any doubt that magnetic dust, which in all probability derives it origin from meteors, has often been observed, and the question arises, in what way we can increase our knowledge on these points to an appreciable extent. A further series of occasional observations would in all probability lead to no result of great value, unless they were carried on for a great length of time in suitable places. Meteoric dust, we know, does fall, and observations ought if possible to be directed rather towards an approximate estimate of the quantity which falls within a given time. Difficulties very likely will be found in the determination of the locality in which the observations should be conducted. The place ought to be sheltered as much as possible against any ordinary dust not of meteoric origin. The lonely spots best fitted for these observations are generally accessible to occasional experiments only, and do not lend themselves easily to a regular series of observations. Nevertheless experiments continued for a few months at some elevated spot in the Alps might lead to valuable results. The Committee would like to draw attention to an instrument which is well fitted for such observations. It was devised by Dr. Pierre Miquel for the purpose of examining, not the meteoric particles, but organic and organized matters floating about in the air. A description, with illustrations, will be found in the *Annuaire de Montsouris* for 1879. Two forms of the instrument are given. In the first form, which is only adapted to permanent places of observations, an aspirator draws a quantity of air through a fine hole. The air impinges on a plate coated with glycerine, which retains all solid matter. By means of this instrument we may determine the quantity of solid particles within a given volume of air.

The second, more portable form, does not allow such an accurate quantitative air analysis. The instrument is attached to a weathercock, and thus is always directed against the wind, which traverses it, and deposits, as in the other permanent form, its solid matter on a glycerine plate. An anemometer placed in the vicinity serves to give an approximate idea of the quantity of air which has passed through the apparatus. These instruments have been called aëroscopes by their inventor. It is likely that the second form given to the apparatus will be best fitted for the purpose which the Committee has in view.

THE NEW ASTRONOMER ROYAL.

Mr. William Henry Mahony Christie, who has succeeded Sir George Airy in the office of Astronomer Royal at the Royal Observatory, Greenwich Park, was born on October 1, 1845, at Woolwich. He is a younger son of the late Professor S. H. Christie, of the Royal Military Academy, Woolwich, and formerly Secretary to the Royal Society. Mr. W. H. M. Christie was educated at Kings College School, London; and at Trinity College, Cambridge, which he entered in 1864, having won a Minor's Scholarship of that College; he subsequently gained a Foundation Scholarship and was afterwards elected a Fellow of Trinity College. He took his degree of B.A. in 1868, as fourth wrangler in the Mathematical Tripos, and in 1871 proceeded to the M. A. degree. In 1870, Mr. Christie was appointed Chief Assistant at the Royal Observatory; and he has, during the past ten years, done special good service by contriving and introducing several valuable improvements in the scientific apparatus there in use; a new form of spectroscope, an instrument for determining the colors and brightness of the stars, a recording micrometer, and a polarising solar eye-piece, are to be mentioned as his inventions. In the recent address of the President of the British Association, at York, a passing reference was made to Mr. Christie's work in verifying the results obtained by Dr. Huggins, with regard to the motions of stars, as inferred from spectroscopic observations. The new Astronomer Royal has directed particular attention, at the Royal Observatory, both to spectroscopy and to photography, as a means of recording the observations. He is a fellow of the Royal Society, and was elected Secretary of the Royal Astronomical Society last year. He contributed to the proceedings of the Royal Society, in March, 1877, a paper "on the magnifying power of the half-prism, as a means of obtaining great dispersion, and on the general theory of the half-prism spectroscope." To the monthly notices of the Royal Astronomical Society, he has furnished these: in June, 1873, a paper on the recording micrometer; in January, 1874, on the color and brightness of stars, as measured with a new photometer; in May, 1875, on the determination of the scale in photographs of the Transit of Venus; in 1876, (January) on a new form of solar eye-piece; (May) on the displacement of lines in the spectra of stars; (November) on the effect of wear in the micrometer screws of the Greenwich Transit Circle; same year (December) on the gradation of light on the disk of Venus; in 1878 (January) on specular reflection from Venus; (June) on the existence of bright lines in the solar spectrum; in 1879 (January) on a phenomeno seen in the occultation of a star by the moon's bright limb; in 1880, November, on the spectrum of Hartwig's comet of that year; in 1881 (January) on Mr. Stone's alterations of Bessel's refractions; (May) on the flexure of the Greenwich transit circle, and some further remarks on Mr. Stone's alterations of Bessel's refractions; besides various papers on the Greenwich spectroscopic and photographic observations, communicated by the late Astronomer Royal; and a paper which will be found in the Memoirs of the Royal Astronomical Society, published in January, 1880, on the systematic errors of the Greenwich North Polar distances. Mr. Christie is also the founder and editor of a journal entitled "*The Observatory*, a Monthly Review of Astronomy," which has been published during the past four years; and he is author of the "Manual of Elementary Astronomy," published in 1875 by the Society for Promoting Christian Knowledge.

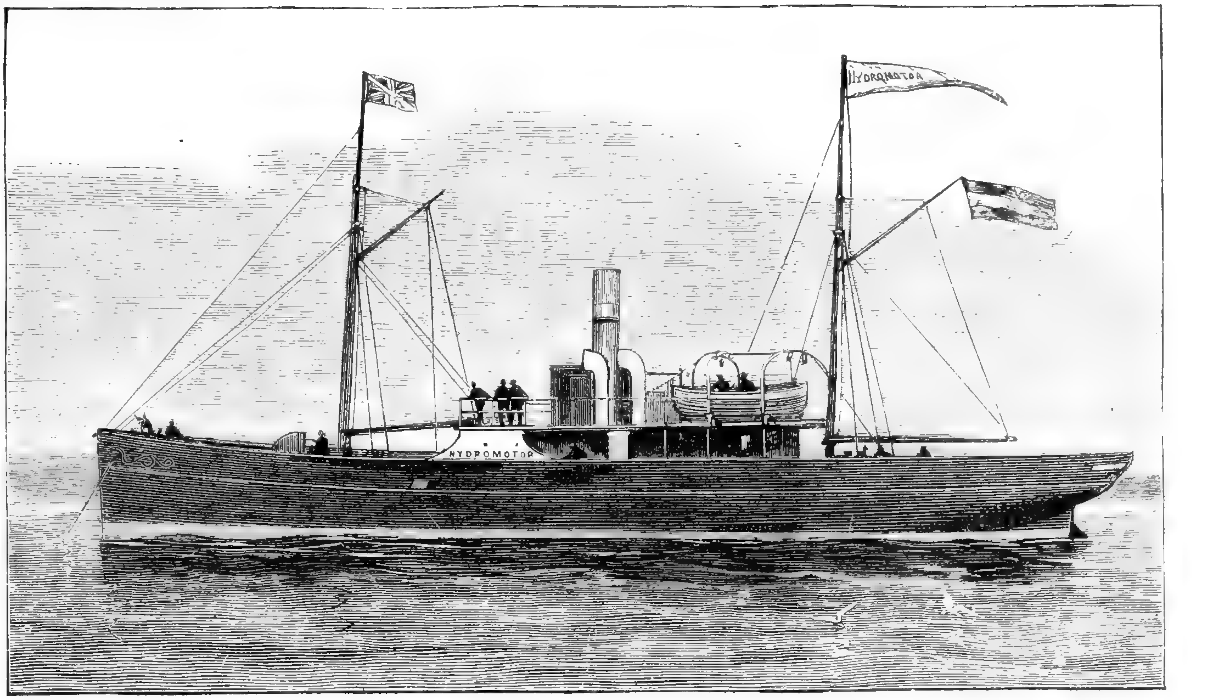
WILLIAM H. M. CHRISTIE.

ON THE ELECTRIC CONDUCTIVITY AND DICHROIC ABSORPTION OF TOURMALINE.*

By Prof. SILVANUS P. THOMPSON.

The electric conductivity of tourmaline differs in different directions; being, according to the author's experiments, a minimum along the optic axis. Tourmaline also possesses the optical property of dichroism, its absorption being a maximum for rays parallel to the axis, and greater for blue rays than for red, equal thicknesses of crystal being considered. According to the electro-magnetic theory of light, bodies which are good conductors of electricity should be opaque to light. The author has in the August number of the *Philosophical Magazine* rewritten the equations of Maxwell's electro-magnetic theory for the case of crystalline media possessing different conductivities in different directions. From these equations it appears that in tourmaline and negative uniaxial crystals electric displacements at right angles to the axis will be more absorbed than electric displacements parallel to the axis. This accounts for the well-known greater absorption of the ordinary ray, provided the views of Stokes and Fresnel are correct, that these displacements are at right angles to the so-called plane of polarization. The difference of velocity between the rays of different color accounts for the difference of absorption being greater in that direction in which the conductivity is a minimum. It was also pointed out that in positive uniaxial crystals, in which the electric conductivity is a maximum along the axis, there will be maximum absorption of the extraordinary ray, and there will be least opacity along the axis. Smoky quartz and magnesic platinocyanide fulfil the latter condition. Specimens of tourmaline cut into cubes to show the colors in different directions were shown, and also specimens of magnesic platinocyanide and of herapathite. Mechanico-optical models were also shown illustrating the theory; a tourmaline being represented by a cube built up of layers of glass and wire-gauze. In conclusion it was shown that crystals in which the electric conductivity differs in three different directions will exhibit *trichroism;* and that di- or trichroic absorption is a general property of all colored crystals other than those of the cubical system.

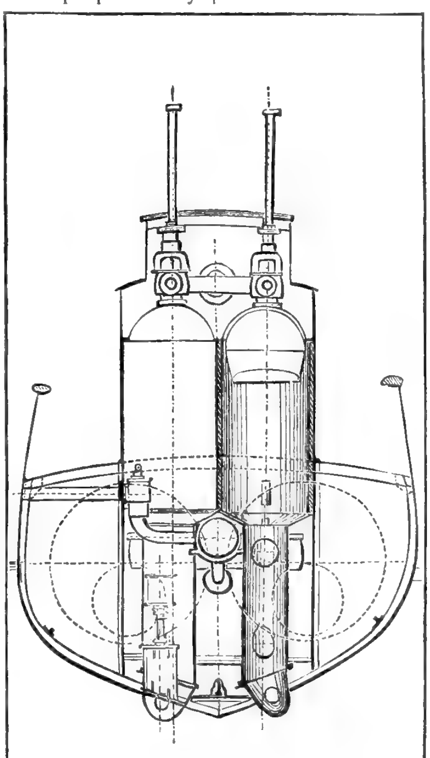
* British Association, 1881.



THE VESSEL AS IT APPEARS AFLOAT

THE HYDROMOTOR SHIP.

We present a drawing of a vessel propelled by hydraulic reaction, and recently constructed at Kiel by Dr. Emil Fleischer, of Dresden.



Machines propelled on the reactive principle are by no means a novelty, but hitherto have been attended with indifferent success. Nearly 40 years ago a model boat, which used to travel up and down a tank propelled by such means, was exhibited in London. But Dr. Fleischer's hydromotor allows of as much as 90 per cent. of the indicated steam power being applied to the production of the outflowing water stream, while not more than 30 per cent. has been secured with the reaction machines hitherto constructed. In his vessel the usual ship's engines, worked by means of wheel or screw, are replaced by hydraulic reaction, by the drawing in and shooting out of a stream of water. The steam power acts immediately on the water, without any loss of such power in conveyance from steam engines and pumps. The manœuvring capabilities of the vessel are greatly increased, and the usual complicated machinery is replaced by a remarkably simple contrivance.

The professional men who took part in a short trip with the hydromotor, expressed the most unqualified appreciation of the invention, and of every detail of its execution. The easy manœuvring of the vessel, its small consumption of coal, and the practicability of adapting the system to all rates of speed were clearly shown, and the simplicity of its construction was regarded as particularly valuable for war ships. The hydromotor is undergoing further tests in English waters.

HYDRODYNAMIC ANALOGIES TO ELECTRICITY AND MAGNETISM.

From a scientific and purely theoretical point of view there is no object in the whole of the Electrical Exhibition at Paris of greater interest than the remarkable collection of apparatus exhibited by Dr. C. A. Bjerknes, of Christiania, and intended to show the fundamental phenomena of electricity and magnetism by the analogous ones of hydrodynamics. I will try to give a clear account of these experiments and the apparatus employed; but no description can convey any idea of the wonderful beauty of the actual experiments, whilst the mechanism itself is also of most exquisite construction. Every result which is thus shown by experiment had been previously predicted by Prof. Bjerknes as the result of his mathematical investigations.

It has long been known that if a tuning-fork be struck and held near to a light object like a balloon it attracts it. This is an old experiment, and the theory of it has been worked out more than once. Among others, Sir William Thomson gave the theory in the *Philosophical Magazine* in 1867. In general words the explanation is that the air in the neighborhood of the tuning-fork is rarefied by the agitation which it experiences. Consequently the pressure of the air is greater as the distance from the tuning-fork increases. Thus the pressure on the far side of the balloon is greater than that on the near side, and the balloon is attracted.

Dr. Bjerknes has followed out the theory of this action until he has succeeded in illustrating most of the

fundamental phenomena of electricity and magnetism. He causes vibrations to take place in a trough of water about six inches deep. He uses a pair of cylinders fitted with pistons which are moved in and out by a gearing which regulates the length of stroke and also gives great rapidity. These cylinders simply act alternately as air-compressers and expanders, and they can be arranged so that both compress and both expand the air simultaneously, or in such a way that the one expands while the other compresses the air, and *vice versa*. These cylinders are connected by thin india-rubber tubing and fine metal pipes to the various instruments. A very simple experiment consists in communicating pulsations to a pair of tambours, and observing their mutual actions. They consist each of a ring of metal faced at both sides with india-rubber and connected by a tube with the air-cylinders. One of them is held in the hand; the other is mounted in the water in a manner which leaves it free to move. It is then found that if the pulsations are of the same kind, *i.e.* if both expand and both contract simultaneously, there is attraction. But if one expands while the other contracts, and *vice versa*, there is repulsion. In fact the phenomenon is the opposite of magnetical and electrical phenomena, for here like poles attract, and unlike poles repel.

Instead of having the pulsation of a drum we may use the oscillation of a sphere; and Dr. Bjerknes has mounted a beautiful piece of apparatus by which the compressions and expansions of air are used to cause a sphere to oscillate in the water. But in this case it must be noticed that opposite sides of ths sphere are in opposite phases. In fact the sphere might be expected to act like a magnet; and so it does. If two oscillating spheres be brought near each other, then, if they are both moving to and from each other at the same time, there is attraction; but if one of them be turned round, so that both spheres move in the same direction in their oscillations, then there is repulsion. If one of these spheres be mounted so as to be free to move about a vertical axis, it is found that when a second oscillating sphere is brought near to it, the one which is free turns round its axis and sets itself so that both spheres in their oscillations are approaching each other or receding simultaneously. Two oscillating spheres, mounted at the extremities of an arm, with freedom to move, behave with respect to another oscillating sphere exactly like a magnet in the neighborhood of another magnetic pole. I believe that these directive effects are perfectly new, both theoretically and experimentally. The professor mounts his rod with a sphere at each end in two ways: (1) so that the oscillations are along the arm, and (2) so that they are perpendicular. In all cases they behave as if each sphere was a little magnet with its axis lying along the direction of oscillation.

Dr. Bjerknes looks upon the water in his trough as being the analogue of Faraday's medium; and he looks upon these attractions and repulsions as being due, not to the action of one body on the other, but to the mutual action of one body and the water in contact with it. Viewed in this light, his first experiment is equivalent to saying that if a vibrating or oscillating body have its motions in the same direction as the water, the body moves away from the centre of disturbance, but if in the opposite direction, towards it. This idea gives us the analogy of dia- and paramagnetism. If, in the neighborhood of a vibrating drum, we have a cork ball, retained under the water by a thread, the oscillations of the cork are greater than those of the water in contact with it, owing to its small mass, and are consequently *relatively* in the same direction. Accordingly we have repulsion, corresponding to diamagnetism. If, on the other hand, we hang in the water a ball which is heavier than water, its oscillations are not so great as that of the water in its vicinity, owing to its mass, and consequently the oscillations of the ball relatively to the water are in the opposite direction to those of the water itself, and there is attraction, corresponding to paramagnetism. A rod of cork and another of metal are suspended horizontally by threads in the trough. A vibrating drum is brought near to them; the cork rod sets itself equatorially, and the metal rod axially.

If a pellet of iron be floated by a cork on water and two similar poles (*e. g.* both north) be brought to its vicinity, one above and the other below the pellet, the latter cannot remain exactly in the centre, but will be repelled to a certain distance, beyond which, however, there is the usual attraction. The reason is that when the pellet is nearly in the line joining the two poles the north pole of the pellet (according to our supposition) is further from this line than the south one. The angle of action is less; so that although the north pole is further away, the horizontal component of the north pole repulsion may be greater than that of the south pole attraction. Dr. Bjerknes reproduces this experiment by causing two drums to pulsate in concord, the one above the other. A pellet fixed to a wire, which is attached by threads to two pieces of cork, is brought between the drums, and it is found impossible to cause it to remain in the centre.

Dr. Bjerkes conceived further the beautiful idea of tracing out the conditions of the vibrations of the water when acted upon by pulsating drums. For this purpose he mounted a sphere or cylinder on a thin spring and fixed a fine paint-brush to the top of it. This is put into the water. The vibrations are in most cases so small that they could not be detected, but by regulating the pulsations so as to be isochronous with the vibrations of the spring, a powerful vibration can be set up. When this is done a glass plate mounted on four springs is lowered so as to touch the paint-brush, and the direction of a hydrodynamic line of force is depicted. Thus the whole field is explored and different diagrams are obtained according to the nature of the pulsations. Using two drums pulsating concordantly, we get a figure exactly like that produced by iron filings in a field of two similar magnetic poles. If the pulsations are discordant it is like the figure with two dissimilar poles. Three pulsating drums give a figure identical with that produced by three magnetic poles. The professor had previously calculated that the effects ought to be identical, and I think the same might have been gathered from the formulæ in Sir William Thomson's "Mathematical Theory of Magnetism," but this only enhances the beauty of the experimental confirmation.

Physicists have been in the habit of looking upon magnetism as some kind of molecular rotation. According to the present view it is a rectilinear motion. Physicists have been accustomed to look upon the conception of an isolated magnetic pole as an impossibility, but here, while the oscillating sphere represents a magnetic molecule with north and south poles, the pulsating drum represents an isolated pole. These are new conceptions to the physicists, let us see whither they lead us. The professor shows that if a rectilinear oscillation constitutes magnetism, a circular oscillation must signify an electric current, the axis of oscillation being the direction of the current. According to this view what would be the action of a ring through which a current is passing? If the ring were horizontal the inner parts of the ring would all rise together and all fall together, they would vibrate and produce the same effect as the rectilinear vibrations of a magnet. This is the analogue of the Amperian currents.

To illustrate the condition of the magnetic field in the neighborhood of electric currents, Dr. Bjerknes mounted two wooden cylinders on vertical axes, connecting them by link-work, which enabled him to vibrate them in the same or opposite ways. To produce enough friction he was forced to employ syrup in place of water. The figures which are produced on the glass plate are in every case the same as those which are produced by iron filings in the neighborhood of electric currents, including the

case of currents going in parallel and in opposite directions.

The theory is carried out a step further to explain the attraction and subsequent repulsion after contact of an electrified and a neutral substance and the passage of a spark. But it is extremely speculative, and is not as yet experimentally illustrated, and I think that at present it is better to pass it by,

I believe that the professor will exhibit his experiments and give some account of his mathematical investigations, which have occupied his time for five years, to the Académie des Sciences this afternoon. His results have not been published before. GEORGE FORBES.

THE ELECTRIC DISCHARGE THROUGH COLZA OIL.*

By A. MACFARLANE, D. SC., F. R. S. E.

The electrical properties of colza oil which I have examined are its dielectric strength and some phenomena which accompany the passage of the spark. By the dielectric strength of a substance I mean the ratio of the difference of potential required to pass a spark through air under the same conditions. The electrodes used were two parallel brass plates each four inches in diameter. When comparing the gases the standard distance of the plate chosen was 5 mm. In the case of liquids it is convenient to observe for a shorter distance, and reduce the result by the law which previous experiments of mine have established, namely, that in the case of the discharge between parallel plates through a liquid dielectric, the difference of potential required is proportional to the distance between the plates. (*Trans.* R. S. E., vol. xxix. p. 563). One set of observations gave the ratio for colza oil to be 2.7, another gave 2.5. Hence 2.6 may be taken. I have now obtained the following table of dielectric strengths for liquids (1 being unity).

Substance.	Dielectric Strength.
Paraffine oil	3.7
Oil of turpentine...........	4.0
Paraffine liquefied..........	2.4
Olive oil..................	3.5
Colza oil..................	2.6

The specific gravity of the colza oil is .91. The passage of the spark was accompanied by the formation of gas bubbles, but there was no deposition of solid particles. As the 4-inch plates were placed horizontally in the oil a bubble produced by the discharge was prevented from escaping by the upper plate. When the upper plate is again electrified such a bubble behaves in the following manner. If it is large enough it will extend itself somewhat like an hour-glass between the plates, but if it is smaller it takes the form of an acorn with a flat base, the base resting on one or other of the plates. When the upper plate is charged positively the bubble is repelled so as to place its base on the lower plate; when the electricity is changed to negative the bubble remains with its base on the upper plate. A reversal of the order of charging did not change the effect. After a few electrifications a sufficient number of solid particles collect to form a chain, and thus interfere with the phenomenon, the bubbles then being lengthened out in a remarkable manner, but never repelled to the lower plate. When the upper plate was charged negatively, gas bubbles appeared to me to rise from the lower plate, as if they had been formed there. To test this point further I took some sparks between two smaller disks placed vertically in the oil. The gas-bubbles were observed to rise up at the negative surface as if they had been formed at the positive surface, and had been repelled or carried straight across, and then rose up at the negative surface. When the spark was taken between two points bent at right angles to two rods dipping into the oil, the bubbles were observed to shoot out in the direction from the positively charged point, and to circulate round the earth-rod some time before rising to the surface. These phenomena indicate that the bubble is positively electrified.

* British Association, 1881.

ASTRONOMY.

ON THE POSSIBILITY OF THE EXISTENCE OF INTRA-MERCURIAL PLANETS.*

By BALFOUR STEWART, LL.D., F.R.S.

It is a somewhat frequent speculation amongst those who are engaged in sun-spot research to regard the state of the solar surface as influenced in some way by the positions of the planets.

In order to verify this hypothesis observers have tried whether there appear to be solar periods exactly coinciding with certain well-known planetary periods. This method has been adopted by the Kew observers (Messrs. De La Rue, Stewart, and Loewy), who had an unusually large mass of material at their disposal, and they have obtained from it the following results :—

1. An apparent maximum and minimum of spotted area approximately corresponding in time to the perihelion and aphelion of Mercury.

2. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Mercury and Jupiter.

3. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Venus and Jupiter.

4. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Venus and Mercury.

The Kew observers make the following remarks upon these results :—

"There appears to be a certain amount of likeness between the march of the numbers in the four periods which we have investigated, but we desire to record this rather as a result brought out by a certain specified method of treating the material at our disposal than as a fact from which we are at present prepared to draw conclusions. As the investigation of these and similar phenomena proceeds, it may be hoped that much light will be thrown upon the causes of sun-spot periodicity.

The Kew observers have likewise produced evidence of a different kind in favor of the planetary hypothesis, for they have detected a periodicity in the behavior of sun-spots with regard to increase and diminution apparently depending upon the positions of the two nearer planets, Mercury and Venus. The law seems to be that as a portion of the sun's surface is carried by rotation nearer to one of these two influential planets, there is a tendency for spots to become less and disappear, while on the other hand, when it is carried away from the neighborhood of one of these planets, there is a tendency for spots to break out and increase.

But whatever truth may be in these conclusions, it appears to be quite certain that periodical relations between the various *known* planets will not account for *all* the sun-spot inequalities with which we are acquainted. They may account for some, but certainly not for all. For there are solar inequalities of short duration which presuming them to be real, can only be accounted for on the planetary hypothesis, by supposing the existence of several unknown intra-Mercurial planets.

Indeed these short-period inequalities in sun-spots and the allied phenomena of terrestrial magnetism and meteorology have so augmented in number of late years as to make some observers inclined to question their reality; while others again resort to the above-mentioned hypothesis, and attribute them to intra-Mercurial planetary agency.

The method to be pursued in detecting the existence

* British Association, 1881.

of inequalities will be easily understood by an illustration. Suppose we had in our possession extensive records of the temperature of the earth's atmosphere at some one place in middle latitudes, and that, independently of astronomical knowledge, we were to make use of these for the purpose of investigating the natural inequalities of terrestrial temperature. We should begin by grouping the observations according to various periods taken, say, at small but definite time-intervals from each other. Now if our series of observations were sufficiently extensive, and if some of our various groupings together of this series should correspond to a real inequality, we should expect it to exhibit a well-defined and prominent fluctuation, whose departures above and below the mean should be of considerable amount.

Suppose, for instance, that we have twenty-four points in our series, and that we group a long series of temperature observations in rows of twenty-four each, the time distance between two contiguous members of one row being one hour. The series would thus represent the mean solar day, and we should, without doubt, obtain from a final summation of our rows a result exhibiting a prominent temperature fluctuation of a well-defined character, which we might measure (as long as we keep to twenty-four points) by simply adding together all the departures of its various points from the mean, whether these points lie above or below; in fine, by obtaining the area of the curve, which is the graphical representation of the inequality above and below the line of abscissæ taken to represent the mean of all the poiuts. Suppose next that, still keeping to rows of twenty-four, we should make the time interval between two contiguous members of a row somewhat different from one hour, whether greater or less, we should now in either case obtain a result exhibiting, when measured as above, a much smaller inequality than that given when the interval was exactly one hour; and it is even possible that, if our series of observations were sufficiently extensive, we should obtain hardly any traces of an inequality whatever.

In fine, when each row accurately represented a solar day, the result would be an inequality of large amount; but when each row represented a period either slightly less or greater than a day, the result would be an inequality of small amount. This process, as far as I have described it, is not new, inasmuch as something of this kind must be pursued in all attempts to detect inequalities. In the present instance we should by its means, after bestowing enormous labor in variously grouping, in accordance with a great number of periods taken at small intervals from each other, obtain definite results. These might be graphically represented in the following manner:—

The line of abscissæ might be taken to denote the exact values of the various periods, forming a time-scale, in fact, while the ordinates might represent the areas or summations obtained as above by employing these various periods. There would thus be in the case now used for illustration a very prominent peak, corresponding to twenty-four hours, which would fall off very rapidly on either side.

By means of the process now described we should at length, after enormous labor, obtain a graphical result, showing the exact position in the time-scale of the observed maximum inequality. In conjunction with Mr. William Dodgson, I have devised a method by which this labor is very greatly reduced, and the process so modified has been applied by us in order to determine whether there be inequalities of short period in the observed areas of the sun-spots occurring on the visible hemisphere of the sun. We have detected an inequality of this nature corresponding in period to 24.011 days, which, when subjected to a certain purifying treatment, appears to us to exhibit the marks of a true periodicity. But it has been suggested by Prof. Stokes that a method of this nature for detecting inequalities might with greater propriety be employed as a crucible for testing the value of some hypothesis introduced into it from without.

Acting upon this suggestion I have ventured to introduce the planetary hypothesis, and to ask whether the above sun-spot inequality of short period may not in reality be caused by an intra-Mercurial planet. It is quite easy to put this hypothesis to a test, taking for our guidance the results obtained by the Kew observers. For what do these results exhibit? In the first place they exhibit the probability of a sun-spot inequality corresponding to the period of Mercury round the sun; and in the next they exhibit the probability of similar inequalities corresponding to the synodic period of Mercury and Venus, and to the synodic period of Mercury and Jupiter.

Now if there be an intra-Mercurial planet of period 24.011 days, it will have the following synodic periods:—

With Mercury................33.025 days.
With Venus.................26 884 days.
With Jupiter................24.145 days.

In conjunction with Mr. Dodgson I have applied the above method of analysis with the view of ascertaining whether there be well-marked sun-spot inequalities nearly corresponding to these periods, and we have obtained the following results:—

A very prominent inequality of period......32.955 days.
A very prominent inequality of period......26.871 days.
A less prominent inequality of period......24.142 days.

It will thus be noticed that there are prominent sun-spot inequalities, the periods of which agree very well with the synodic periods of the supposed planet with Mercury, Venus, and Jupiter, more especially if we bear in mind that this is only a first approximation.

The test, however, is not yet complete. Referring once more to the results of the Kew observers, it will be noticed that we have approximately maxima of sun-spot areas when Mercury and Venus, or when Mercury and Jupiter are in conjunction. Now if we assume that there is an intra-Mercurial planet of period 24.011 days, we are as yet unable to assign its exact position in ecliptical longitude at any moment. We know its period, and we may presume that it has considerable eccentricity, but we know nothing else. We may, however, assume as most probable that the maximum point of the inequality of period 32.955 days corresponds to the conjunction of the planet with Mercury, the maximum point of the inequality of period 26.871 days to its conjunction with Venus, and the maximum point of the inequality of period 24.142 days to its conjunction with Jupiter. On this assumption, and knowing the average rate of motion of the planet in its orbit, we may deduce approximately its position at a given epoch independently from each of the three synodic periods above mentioned, and these positions ought to agree together, if our hypothesis be correct.

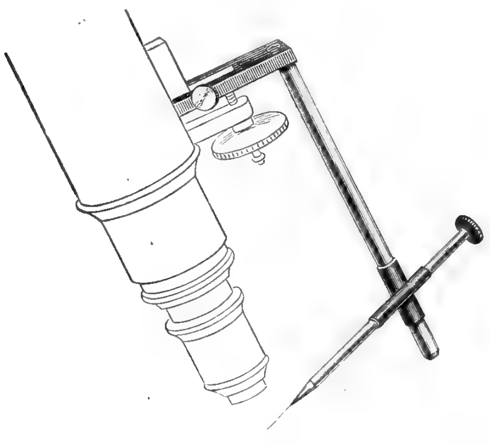
I have done this approximately, but am not able to bring exact figures before this meeting. The agreement is as great as can be expected, bearing in mind that we know only the average rate of motion of the planet, and not the variations of its rate, inasmuch as we are ignorant of its eccentricity. I think I may state that three independent values of its position corresponding to January 1, 1832, will be obtained, and that the mean difference of a single value from the mean of the whole will probably not be more than twenty degrees. It would thus appear from this investigation that the evidence is in favor of the sun-spot inequality of 24.011 days being due to an intra-Mercurial planet. Of course a single research of this nature is insufficient to establish a theory of this importance, but as there are several short-period solar inequalities, the same method may be pursued for each, an operation which demands nothing but time and labor. It appears to me of great importance that these short-period solar inequalities should be systematically examined after this method.

MICROSCOPY.

Mr. C. Henry Kain thus describes in the August number of *The American Journal of Microscopy*, his new mechanical finger for the microscope, which will be found a useful addition to the instrument.

A glance at the engraving will render the working of it intelligible to all. It consists essentially of a slotted bar which may be firmly clamped to the upper (immovable) bar of the fine adjustment by means of a milled-headed screw. Through the end of this is fastened a round rod, at such a distance from the objective that, when lowered, the end will not strike the stage. Over this rod slips a split tube, to which is soldered at an angle, a smaller tube. Through the small tube passes a rod carrying a glass hair at its extremity. This rod is easily rotated by means of a milled head. The capillary glass thread is attached to the extremity by means of beeswax. The arrangement of split tubes was suggested by Mr. Edward Pennock, to take the place of a binding screw which I had intended; it is a very neat and convenient affair, and much less clumsy than the arrangement I originally proposed. It will be noticed that the finger has no revolving collar, as it is quite unnecessary, especially when the microscope is provided with a revolving stage. By dispensing with the revolving collar and making all movements depend entirely upon the adjustments of the microscope, greater stability and accuracy in working are secured.

To use the finger, the point of the glass thread is first brought into the focus of the objective, or nearly so, by sliding the tube on the vertical rod and pushing or pulling the rod carrying the glass thread until the desired position is attained. It is not difficult to do this, and, having once been done by hand, it does not have to be repeated, as all further movements are made by the adjustments of the microscope. Supposing now the point of the glass thread



A NEW MECHANICAL FINGER.

to be in focus; by means of the fine adjustment throw the focus *ahead* of the point, then, by means of the coarse adjustment, rack down and search for the object you wish to pick up. Having found the object desired, again bring the point of the thread into focus by means of the fine adjustment; then rack down with the coarse adjustment and pick it up. Now rack back with the coarse adjustment, remove the slip on which the material is spread, and place your prepared slip or cover upon the stage. Again, by means of the fine adjustment, throw the focus ahead of the object, rack down with the coarse adjustment and search for the spot where you wish to deposit the object, and, having found it, again focus the object, then rack down with the coarse adjustment, and, when the object touches the slide and has been placed in proper position, fix it by means of a very gentle breath. There are many other devices by which this useful little instrument may be used for a variety of purposes, for a description of which we refer the reader to Professor Phin's journal.

PENNOCK'S OBLIQUE DIAPHRAGM.—The accompanying engravings show a new form of oblique diaphragm devised by Mr. E. Pennock, and described by him in *The American Journal of Microscopy* (August, 1881). It is designed to be attached to the under side of the stage for shutting off all light except a small pencil from the mirror. Its function is the same as Smith's >-shaped diaphragm. It is an adaptation of Mr. Mayall's spiral diaphragm, which was originally designed for use with condensers of wide aperture, and was described in a recent number of the *Journal of the Royal Microscopical Society*.

It may be mounted in either of two forms: the one to fit into the usual tube, which, in the cheaper microscopes, is attached to the under side of the stage; the other to screw directly into the stage aperture.

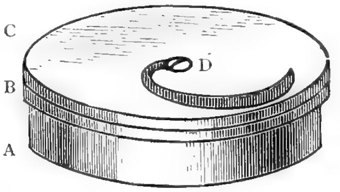


FIG. 1.

A. Tube 1½ inch in diameter, fitting into accessory tube beneath stage.

B. Upper plate (shown as under) having radial slot.

C. Under plate, having spinal slot.

D. Screw joining the plates.

The manner of using it to obtain pencils of varying degrees of obliquity will be sufficiently manifest from the construction.

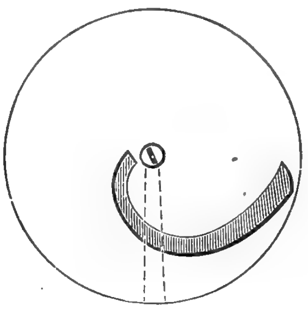


FIG. 2—PLAN OF UPPER AND LOWER PLATES MOVING FULLY ON EACH OTHER.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

I do not like to see so great an authority as Faraday misunderstood, as he evidently is by your correspondent on page 459 of your journal, and that too, in a way which he took particular care to caution against—as to the law of gravitating action. That it acts inversely as the square of the distance he fully believed and admitted; or, to use his own words, "I know it is so."

If your correspondent finds difficulty to account by this law for the return of the earth from aphelion to perihelion, let him try to account for the return of a stone to the earth when thrown up into the air; for precisely the same explanation applies to both, the highest point of the stone's path being "aphelion." The resistance of the air need not be regarded, for, though it modifies the stone's path, it does not affect the theory of the action of gravity.

GEO. B. MERRIMAN.

RUTGERS COLLEGE.

To the Editor of "SCIENCE:"

I have no desire to make any rejoinder to Dr. Rogers' reply (see SCIENCE, p. 459), but am willing to leave his answer with your readers just as he has given them.

I desire, however, to make the following corrections in my published letter:—

On p. 458, next to last paragraph, for "author of above question" read "author of above quotation." Same page, last paragraph, for "As to the law of inertia" read "As by the law of inertia." And on p. 459, last line of first paragraph, for "centrifugal" read "centripetal."

DES MOINES, *Sept.* 26, 1881. J. E. HENDRICKS.

BOOKS RECEIVED.

CELESTIAL OBJECTS FOR COMMON TELESCOPES, by the REV. T. W. WEBB, M. A., F. R. A. S.—Fourth Edition — Revised and greatly enlarged — The Industrial Publication Company, No. 14 Dey street, New York. Price $3.00.

From the number of inquiries we have received respecting the expected issue of a fourth edition, we believe it will be welcome intelligence to our readers, to learn that the work can now be obtained.

As the third edition was an enlargement of its predecessors, so the present and latest edition has been re-written and again enlarged. Mr. Webb thus states his reasons for remodeling his work, and at the same time indicates many of the improvements that he has introduced.

"The unprecedented diffusion of a taste for astronomical observation during the last seven years has brought with it such a corresponding increase in the optical capacity of telescopes in private hands that the very title of this treatise would convey an inaccurate impression unless its contents were modified in accordance with the requirements of the time.

Without abandoning that elementary character which may still make it serviceable to beginners, its compass must now be greatly extended, if it may hope for acceptance as a manual by the more advanced student; and with this object, as the increase of telescopic range chiefly affects the sidereal portion, recourse has been had for additional Double Stars to the great catalogue of Struve I., as well as in a lesser degree to those of his son and Burnham, and as regards Nebulæ to that of Herschel II., with a total increase of about 1500 objects, some of which are chosen as tests worthy of the finest instruments, but occasionally, as is well known, within reach of those of more moderate dimensions."

The present edition of Mr. Webb's will soon find purchasers, and we advise all those who desire to possess a copy, to be prompt in securing it. The work is an indispensable manual to all who possess a telescope, or have a taste for astronomical studies.

A CORRECTION.—Professor Edward S. Morse desires to withdraw the first part of the last paragraph of the abstract of his paper on "Changes in Mya and Lunatia since the Deposition of the New England Shell Heaps," and substitute the following:—

"A comparison of the common beach cockle (Lunatia) from the shell heaps of Marblehead, Mass., showed that the present form living on the shore to-day had a more depressed spire than the ancient form; and this variation," etc., etc.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 8, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 2	30.293	30.348	9 a. m.	30.196	12 p. m.	66.3	62.0	75	0 a. m.	68	0 a. m.	63	11 p. m.	61	11 p. m.	92.
Monday, 3	29.961	30.196	0 a. m.	29.898	6 p. m.	74.3	68.3	82	4 p. m.	71	4 p. m.	63	0 a. m.	61	0 a. m.	137.
Tuesday, 4	29.739	29.902	0 a. m.	29.632	3 p. m.	67.3	59.3	77	3 p. m.	66	3 p. m.	50	12 p. m.	45	12 p. m.	131.
Wednesday, 5	30.135	30.268	12 p. m.	29.788	0 a. m.	40.0	35.6	46	4 p. m.	40	5 p. m.	35	8 a. m.	31	8 a. m.	110.
Thursday, 6	30.246	30.350	9 a. m.	30.196	4 p. m.	49.0	43.3	60	4 p. m.	50	4 p. m.	36	6 a. m.	36	7 a. m.	118.
Friday, 7	30.229	30.298	9 a. m.	30.188	4 p. m.	60.7	53.7	70	4 p. m.	59	3 p. m.	48	6 a. m.	46	6 a. m.	130.
Saturday, 8	30.022	30.196	0 a. m.	29.894	12 p. m.	69.6	61.3	80	4 p. m.	67	5 p. m.	59	7 a. m.	55	4 a. m.	134.

Mean for the week .... 30.089 inches.
Maximum for the week at 9 a. m., Oct. 6th .... 30.350 "
Minimum " at 3 p. m., Oct. 4th .... 29.632 "
Range .... .718 "

	Dry.		Wet.
Mean for the week	61.0 degrees		54.5 degrees.
Maximum for the week, at 4 pm. 3d	82. "	at 4 pm 3d,	71. "
Minimum " " 8 am. 5th	35. "	at 8 pm 5th,	31. "
Range " "	47. "		40. "

OCTOBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in Lbs. per Sqr. Feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of Rain and Snow in Inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0 10
Sunday, 2	n. e.	e. n. e.	e.	184	5	8.30 pm	.495	.502	.497	70	78	83	9 cu.	9 cu.	10	-----	-----	-----	--	0
Monday, 3	s. w.	w.	n. w.	124	5	3.50 pm	.577	.598	.666	84	58	77	10	4 cir. cu.	4 cu.	4.30 am 4.00 pm	9.30 am 4.20 pm	5.00 0.20	.26 .07	0 7
Tuesday, 4	w.	w. n. w.	n. n. w.	201	9	8.00 pm	.529	.490	.230	75	53	51	8 cu.	7 cu.	9 cu.	-----	-----	-----	--	3
Wednesday, 5	n. n. w.	n. n. w.	n. n. w.	377	12¾	4.40 am	.142	.129	.190	70	44	74	0	0	0	-----	-----	-----	--	0
Thursday, 6	n. w.	w. n. w.	w.	176	2¼	1.00 pm	.199	.179	.256	90	40	61	0	0	0	-----	-----	-----	--	0
Friday, 7	w. s. w.	s. w.	s. s. w	153	3½	1.00 pm	.258	.290	.433	71	42	73	8 cu.	3 cir. cu.	0	-----	-----	-----	--	1
Saturday, 8	w. s. w	w. s. w.	w. s. w.	212	3½	2.30 pm	.409	.422	.476	82	45	59	3 cir. cu.	4 cir. cu.	4 cu.	-----	-----	-----		

Distance traveled during the week .... 1,427 miles.
Maximum force .... 12¾ lbs.

Total amount of water for the week .... .33 inch.
Duration of rain .... 5 hours, 20 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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JOHN MICHELS, Editor.

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SATURDAY, OCTOBER 22, 1881.

TO OUR ENGLISH READERS.

We have received from Messrs. Deacon & Co., of 150 Leadenhall street, London, England, a standing order for a large supply of "Science," which will be forwarded weekly. We shall be obliged if our English readers will make this fact known to their friends.

THE REVELATIONS OF THE AUTOPSY HELD ON THE BODY OF THE LATE PRESIDENT.

The *Medical Record* of October 8th contains an account by Dr. Bliss, the late President's attending surgeon, of the life history of his illustrious patient, as well as the *post mortem* observations recorded at the time of the autopsy, and also at a later period, by those who examined the specimens preserved in the Army Medical Museum.

Inasmuch as the various diagnoses, made during Mr. Garfield's life, as to the location of the bullet, and the injury sustained by various organs, were all of them erroneous, and as the secular and medical journals have already discussed those topics *ad nauseam*, we shall limit ourselves to a relation of the leading features in the light of the anatomical findings.

For similar reasons, we shall give no space to a discussion of the views expressed by a physician, who, after incurring considerable ridicule at the hands of the medical profession, and much obloquy at the hands of the public, on account of his sensational experiments on dead bodies, and whose claimed results no doubt misled the eminent surgeons at the President's bed-side, publishes a *post mortem* diagnosis of the case in the same issue of the *Record.* In it he seeks to prove that if the bullet *was* found in an entirely different locality from the one his experiments induced him to surmise, it *would*, had it have gone a little further, infallibly have dropped into a similar situation on the *opposite* side! One of the leading medical journals has no other comment to make on this performance, and, as far as we can learn, that comment expresses the general opinion of the medical profession, than to announce that a leading circus company has engaged the services of Dr. Fanueil D. Weisse to repeat his celebrated experiments in the course of the programme.

We subjoin the essential portions of Dr. Bliss' report:—

The depressed cicatrix of the wound made by the pistol-bullet was recognized over the tenth intercostal space, three and one-half inches to the right of the vertebral spines. A deep linear incision (made in part by the operation of July 24, and extended by that of August 8) occupied a position closely corresponding to the upper border of the right twelfth rib. It commenced posteriorly about two inches from the vertebral spines, and extended forward a little more than three inches. At the anterior extremity of this incision there was a deep, nearly square abraded surface about an inch across.

On *inspection of the abdominal viscera in situ*, the transverse colon was observed to lie a little above the line of the umbilicus. It was firmly adherent to the anterior edge of the liver. The greater omentum covered the intestines pretty thoroughly from the transverse colon almost to the pubes. It was still quite fat, and was very much blackened by venous congestion. On both sides its lateral margins were adherent to the abdominal parietes opposite the eleventh and twelfth ribs. On the left side the adhesions were numerous, firm, well organized and probably old.

These adhesions, and the firm ones on the right side, as well as those of the spleen, possibly date back to an attack of chronic dysentery, from which the patient is said to have suffered during the civil war. On the right side there were a few similar adhesions, and a number of more delicate and probably recent ones.

A mass of black, coagulated blood covered and concealed the spleen and the left margin of the greater omentum. On raising the omentum it was found that this blood-mass extended through the left lumbar and iliac regions and dipped down into the pelvis, in which there was some clotted blood and rather more than a pint of bloody fluid. The blood-coagula having been turned out and collected, measured very nearly a pint. It was now evident that secondary hemorrhage had been the immediate cause of death, but the point from which the blood had escaped was not at once apparent.

The adhesions between the liver and the transverse colon proved to bound an *abcess-cavity* between the under-surface of the liver, the transverse colon, and the transverse mesocolon, which involved the gall-bladder, and extended to about the same distance on each side of it, measuring six inches transversely and four inches from before backward. This cavity was lined by a thick pyogenic membrane, which completely replaced the capsule of that part of the undersurface of the liver occupied by the abscess. It contained

about two ounces of greenish yellow fluid—a mixture of pus and biliary matter. This abscess did not involve any portion of the substance of the liver except the surface with which it was in contact, and no communication could be detected between it and any part of the wound.

Some recent peritoneal adhesions existed between the upper surface of the right lobe of the liver and the diaphragm. The *liver* was larger than normal, weighing eighty-four ounces; its substance was firm, but of a pale yellowish color on its surface and throughout the interior of the organ, from fatty degeneration. No evidence that it had been penetrated by the bullet could be found, nor were there any abscesses or infarctions in any part of its tissue.

The *spleen* was connected to the diaphragm by firm, probably old, peritoneal adhesions. There were several rather deep congenital fissures in its margins, giving it a lobulated appearance. It was abnormally large, weighing eighteen ounces; of a very dark lake-red color both on the surface and on section. Its parenchyma was soft and flabby, but contained no abscesses or infarctions.

There were some recent peritoneal adhesions between the posterior wall of the *stomach* and the posterior abdominal parietes. With this exception no abnormities were discovered in the stomach or *intestines*, nor were any other evidences of general or local peritonitis found besides those already specified.

The *right kidney* weighed six ounces, the *left kidney* seven. Just beneath the capsule of the left kidney, at about the middle of its convex border, there was a little abscess one-third of an inch in diameter, and there were three small serous cysts on the convex border of the right kidney, just beneath the capsule; in other respects the tissue of both kidneys was normal in appearance and texture.

The *urinary bladder* was empty.

Behind the right kidney, after the removal of that organ from the body, the dilated *track of the bullet* was dissected into. It was found that from the point at which it had fractured the right eleventh rib (three and one-half inches to the right of the vertebral spines) that missile had gone to the left, obliquely forward, passing through the body of the first lumbar vertebra and lodging in the adipose connective tissue immediately below the lower border of the pancreas, about two and one-half inches to the left of the spinal column, and behind the peritoneum. It had become completely encysted.

The track of the bullet between the point at which it had fractured the eleventh rib and that at which it entered the first lumbar vertebra was considerably dilated, and the pus had burrowed downward through the adipose tissue behind the right kidney, and thence had found its way between the peritoneum and the right iliac fascia, making a descending channel which extended almost to the groin. The adipose tissue behind the right kidney, and thence had found its way between the peritoneum and the right iliac fascia, making a descending channel which extended almost to the groin. The adipose tissue behind the kidney in the vicinity of this descending channel was much thickened and condensed by inflammation. In the channel, which was almost free from pus, lay the flexible catheter introduced into the wound at the commencement of the autopsy; its extremity was found doubled upon itself, immediately beneath the peritoneum, reposing upon the iliac fascia, where the channel was dilated into a pouch of considerable size. This long descending channel, now clearly seen to be caused by the burrowing of pus from the wound, was supposed, during life, to have been the track of the bullet.

The last dorsal, together with the first and second lumbar vertebra and the twelfth rib, were then removed from the body for more thorough examination.

When this examination was made, it was found that the bullet had penetrated the first lumbar vertebra in the upper part of the right side of its body. The aperture by which it entered involved the intervertebral cartilage next above, and was situated just below and anterior to the intervertebral foramen, from which its upper margin was about one-fourth of an inch distant. Passing obliquely to the left, and forward through the upper part of the body of the first lumbar vertebra, the bullet emerged by an aperture, the centre of which was about one-half inch to the left of the median line, and which also involved the intervertebral cartilage next above. The cancellated tissue of the body of the first lumbar vertebra was very much comminuted and the fragments somewhat displaced. Several deep fissures extended from the track of the bullet into the lower part of the body of the twelfth dorsal vertebra. Others extended through the first lumbar vertebra into the intervertebral cartilage between it and the second lumbar vertebra. Both this cartilage and that next above were partly destroyed by ulceration. A number of minute fragments from the fractured lumbar vertebra had been driven into the adjacent soft parts.

It was further found that the right twelfth rib also was fractured at a point one and one-fourth inch to the right of the traverse process of the twelfth dorsal vertebra; this injury had not been recognized during life.

On sawing through the vertebra, a little to the right of the median line, it was found that the spinal canal was not involved by the track of the ball. The spinal cord, and other contents of this portion of the spinal canal, presented no abnormal appearances. The rest of the spinal cord was not examined.

Beyond the first lumbar vertebra, the bullet continued to go to the left, passing behind the pancreas to the point where it was found. Here it was enveloped in a firm cyst of connective tissue, which contained, besides the ball, a minute quantity of inspissated, somewhat cheesy pus, which formed a thin layer over a portion of the surface of the lead. There was also a black shred adherent to a part of the cyst-wall, which proved, on microscopical examination, to be the remains of a blood-clot. For about an inch from this cyst the track of the ball behind the pancreas was completely obliterated by the healing process. Thence, as far backward as the body of the first lumber vertebra, the track was filled with coagulated blood, which extended on the left into an irregular space rent in the adjoining adipose tissue behind the peritoneum and above the pancreas. The blood had worked its way to the left, bursting finally through the peritoneum behind the spleen into the abdominal cavity. The rending of the tissues by the

extravasation of this blood was undoubtedly the cause of the paroxysms of pain which occurred a short time before death.

This mass of coagulated blood was of irregular form, and nearly as large as a man's fist. It could be distinctly seen from in front through the peritoneum, after its site behind the greater curvature of the stomach had been exposed by the dissection of the greater omentum from the stomach, and especially after some delicate adhesions between the stomach and the part of the peritoneum covering the blood-mass had been broken down by the fingers. From the relations of the mass as thus seen, it was believed that the hemorrhage had proceeded from one of the mesenteric arteries, but as it was clear that a minute dissection would be required to determine the particular branch involved, it was agreed that the infiltrated tissues and the adjoining soft parts should be preserved for subsequent study.

On the examination and dissection made in accordance with this agreement, it was found that the fatal hemorrhage proceeded from a rent, nearly four tenths of an inch long, in the main trunk of the splenic artery, two and one-half inches to the left of the cœliac axis. This rent must have occurred at least several days before death, since the everted edges in the slit in the vessel were united by firm adhesions to the surrounding connective tissue, thus forming an almost continuous wall bounding the adjoining portion of the blood-clot. Moreover, the peripheral portion of the clot in this vicinity was disposed in pretty firm concentric layers. It was further found that the cyst below the lower margin of the pancreas, in which the bullet was found, was situated three and one-half inches to the left of the cœliac axis.

Besides the mass of coagulated blood just described, another, about the size of a walnut, was found in the greater omentum, near the splenic extremity of the stomach. The communication, if any, between this and the larger hemorrhage mass could not be made out.

The examination of the *thoracic viscera* resulted as follows:

The *heart* weighed eleven ounces. All the cavities were entirely empty except the right ventricle, in which a few shreds of soft, reddish, coagulated blood adhered to the internal surface. On the surface of the mitral valve there were several spots of fatty degeneration; with this exception the cardiac valves were normal. The muscular tissue of the heart was soft, and tore easily. A few spots of fatty degeneration existed in the lining membrane of the aorta just above the semilunar valves, and a slender clot of fibrin was found in the aorta, where it was divided, about two inches from these valves, for the removal of the heart.

On the right side slight pleuritic adhesions existed between the convex surface of the lower lobe of the lung and the costal pleura, and firm adhesions between the anterior edge of the lower lobe, the pericardium, and the diaphragm. The *right lung* weighed thirty-two ounces. The posterior part of the fissure, between its upper and lower lobes, was congenitally incomplete. The lower lobe of the right lung was hypostatically congested, and considerable portions, especially toward its base, were the seat of broncho pneumonia. The bronchial tubes contained a considerable quantity of stringy muco-pus; their mucous surface was reddened by catarrhal bronchitis. The lung-tissue was œdematous, but contained no abcesses or infarctions.

On the left side the lower lobe of the lung was bound behind to the costal pleura, above to the upper lobe, and below to the diaphragm, by pretty firm pleuritic adhesions. The *left lung* weighed twenty-seven ounces. The condition of the bronchial tubes and of the lung-tissue was very nearly the same as on the right side, the chief difference being that the area of the broncho-pneumonia in the lower lobe was much less extensive in the left lung than in the right. In the lateral part of the lower lobe of the left lung, and about an inch from its pleural surface, there was a group of four minute areas of gray hepatization, each about one-eighth of an inch in diameter. There were no infractions and no abcesses in any part of the lung tissue.

The surgeons assisting at the autopsy were unanimously of the opinion that, on reviewing the history of the case in connection with the autopsy, it is quite

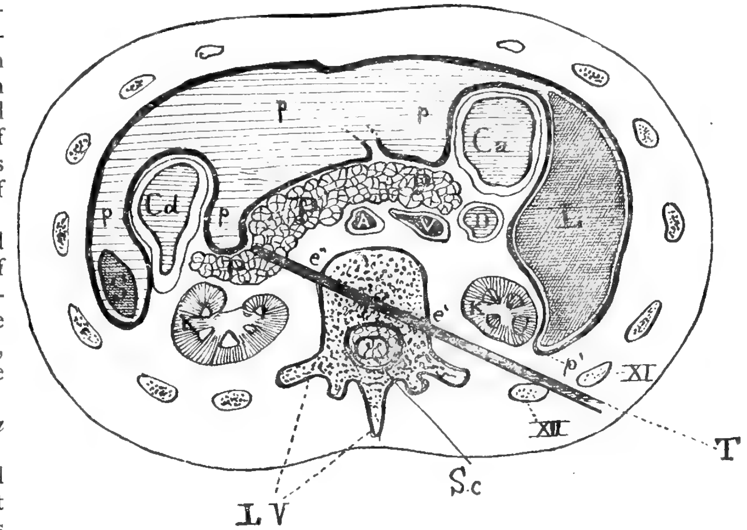


Diagram of a Horizontal Section through a Frozen Human Cadaver at the Level of the Pancreas.

evident that the different suppurating surfaces, and especially the fractured spongy tissue of the vertebra furnish a sufficient explanation of the septic conditions which existed during life.

The accompanying diagrams (page 503) from the *Medical Record* illustrate the course of the bullet very aptly. Fig. 6 is, in our opinion, valueless as a diagram, and it is difficult to harmonize the statements about the track of the ball, and the figure reference, "injury to artery" in the latter illustration. The figure reference, "Point of impact of ball *and deflection*," may be also taken with some allowance.

We have added a schematic drawing taken from a plate representing a frozen section through the human

body, in the horizontal plane (supposing the body standing), and figured of the life size in Braune's magnificent "Atlas." Such a diagram more accurately represents the course of the bullet topographically, than the ones selected by Dr. Bliss seem to us to do.

It is a noteworthy fact, that on marking the topographical projection of the fracture of the 11th rib, that is the point of impact (XI), the fracture of the 12th rib (XII), the aperture of entry into the lumbar vertebra (c'), the aperture of exit (c"), and the terminus of the bullet track, where the bullet was found encysted, on this frozen section, and *connecting all these points by a line, that line is found to be perfectly straight!* It will be seen on examining the figures 4 and 5 furnished by the President's physician, that the bullet track was as straight as an arrow in its frontal projection, and as far as it involved the bony structures.

Now, a line which appears as a straight line in two projections of space, is a straight line in fact, and from this point of view, the case merits renewed discussion.

It must be recollected that our diagram represents a horizontal plane of the body, and the one where the bullet was found encysted. The point of impact lies much higher, the liver in that higher level occupies a greater area of the section, and the bullet in passing the peritoneum seems to have grazed the latter. It could hardly have done so without "nicking" the lower part of the pleural cavity, which descends to this level as a fine slit. The pleuritic adhesions found at the lower part of the right lung, sustain this view.

In the light of modern surgery, an examination of this wound could have presented no difficulties. It is illustrative of the unfortunate position in which the President's physicians find themselves placed, when attempting to defend their course, that the only examination of the wound, which they are able to refer to, is that made by the Surgeon General of the Navy, who was checked in his examination, and excluded from the President's bed-side by those later in charge of the patient. That the fracture of the ribs was not recognized by the attendants, until suppuration near the point of impact necessitated the removal of dead and decaying fragments of bone, is also a remarkable feature of the case. The course of the bullet from the point of impact, being a direct one to the lumbar vertebra, it is difficult to conceive how a thorough digital and instrumental examination could have failed to detect the irregularity produced on the vertebral body by the entrance of the bullet.

None of the procedures necessary to have determined the bullet track from the point of impact to the point of entry into the spinal column, would have been unwarrantable to the most conservative surgeon. *None of these procedures would have been more risky than the one later resorted to in the case, when the false bullet track was repeatedly probed to a distance of twelve inches.*

At this point criticism must necessarily pause, with reference to the question of the examination of the wound. The perforation of the lumbar vertebra having been detected, a bold surgeon might shrink from passing his probe further and following up the bullet-track into the regions beyond, through a perforated segment of the spinal column. What now, in the light of the *post mortem* seems perfectly feasable, during the President's life would have been considered as "heroic" a surgery, as can well be conceived. That a surgeon endowed with the necessary amount of daring, and that great essential the *tactus eruditus*, could have passed a probe through the vertebra and touched the bullet, *without injuring any important organ*, must be admitted by every impartial expert who bears in mind the directness of the bullet track.

This question, however, represents rather a side-issue, when it is recollected that it was not the location of the ball, or its presence in the body, that killed the President. It is only as an illustration of the extent to which the public was misled, and we fear intentionally misled, by some of the President's physicians, that it merits being referred to.

One of the consulting surgeons stated in an ironical way that the bullet could have been removed if its situation had been known, provided the surgeon had, as a preliminary, removed several lumbar vertebræ, and groped his way among the great nerves, the thoracic duct, the Aorta and the Vena Cava. This assertion is stated by no less a one than Dr. Hammond, to be an intentional deception of the people, as it was made through the columns of the daily papers. The reader will see in our diagram that the Aorta and Vena Cava have absolutely no relations to the bullet-track or to the seat of the bullet. Had the situation of the latter been appreciated during life, an incision on the left side of the spinal column would have been in order. This incision and the entire operation *would have been the strict counterparts of certain of the legitimate operations of modern surgery*, namely of *nephrotomy* and *lumbo-colotomy*. It is bad surgery which insists that in every bullet wound, the bullet has to be extracted through its own track. A bullet perforating the thigh to within an inch of the surface on the other side, is to be extracted from the point where it is nearest the surface.

The reader will find the left kidney indicated at K, this is operated on in the operation of nephrotomy, he will find the descending colon at (C. d.), this is opened into in the operation of lumbo-colotomy;

The same risks would have to be taken in the extraction of the bullet that are familiar to the surgeon who performs the latter operation.

Leaving aside all speculative issues, let us trace out the symptoms of the heroic and patient sufferer and their basis, as inferable from the autopsy.

When first struck by the bullet, the President suffered from Surgical Shock. The wound was so severe that his death might have taken place in a few hours; death so occurs to soldiers on the field of battle, who receive similar wounds. His treatment, which consisted in the administration of stimulants and opium, was eminently proper, and doubtless aided by the powerful constitution, overcame the tendency to a fatal collapse.

A tendency to such a collapse recurred at a later period, when Dr. Wales thought the President dying. The primary hemorrhage from the wound was insignificant, no important blood vessels were injured, and the hemorrhage seems to have been mainly external. At a later period an escape of dark venous blood from the wound seems to us to have indicated necrotic usure of the large veins running in the substance of the injured vertebral body.

The painful sensations experienced in the distribution of all the nerves, originating from the crural and particularly the sacral plexus, are attributable to the sudden shock acting on the vertebral column. There is no evidence that any hemorrhage occurred around the nerve roots, or any inflammatory disturbance of the cord or its membranes. The spontaneous disappearance of these nervous symptoms proves conclusively that they were due to the kind of shock, frequently occurring in the practice of military surgeons, and well described by Mitchell, Morehouse and Keene in their work on nerve-injuries. That the nerves on the right side suffered more than those of the left, is attributable to the greater nearness of the right vertebral bullet aperture to the inter-vertebral foramen, where the nerves concerned in the tegumentary supply of the most painful region in this case emerge.

The main injury done by the bullet in its further course, consisted in the irritation of important nerve filaments connected with the Solar Plexus of the Sympathetic System. Irritation of this Plexus, or its derivative branches, accounts for the obstinate and frequently recurring spells of nausea and vomiting, as well as for the great acceleration of the pulse rate, so marked a feature of the President's history. These same symptoms, through the same mechanism, occur with peritonitis, and some of our best physicians suspected the existence of this trouble, from these two symptoms alone. It cannot be said that the imperfect and ambiguous post-mortem record quoted, proves them to have been wrong in their surmise.

In view of the great irritability of the alimentary canal, it is to be considered highly unfortunate that the President's dietary was not properly attended to. At least two of his relapses were due to the undue massing of food in his stomach, at what were considered convalescent periods. Though it is denied that he was allowed improper articles, yet in view of the fact that much of what occurred in the sick-chambers, was rigidly ignored or suppressed by the medical staff, and that on their own confession he was fed on so injurious a combination as milk and lemon-juice for the first few days after the injury, we can not consider that assertion as an invention, until a more authoritative denial be made, than the one vouchsafed by Dr. Bliss.

Several days after the injury, when that examination of the wound was made, which should, in the opinion of ninety-nine out of a hundred surgeons, whom we have consulted or heard opinions from, been made in the first place, a canal was found extending downward to the pelvis. This is now known as the fistulous tract represented on the body-diagram. It was due to burrowing of the pus originating, be it borne in mind, not from the bullet, not from the perforated vertebra, but from the neglected fragments of the eleventh rib. The surgeons did all in their power for a long period to oppose Nature's attempt to close this passage. Thus the pus-absorbing surfaces were largely increased, and with it the dangers of pyæmia.

Without entering into the details of the management of the case, let us conclude with an examination as to the cause of death. The immediate cause of death is stated by the attending surgeon to have been hemorrhage from a dissecting aneurism of the splenic artery. The evidence offered as to the existence of such an aneurism is exceedingly feeble. It is founded on an examination of the specimen after it had lain in alcohol. Under such circumstances laminated layers, as well as membranous precipitates are very apt to form, and on the existence of these the diagnosis appears to rest. A far more natural explanation suggests itself. Every medical student knows that when the dead subject is injected for anatomical purposes masses of blood or of the injecting material are very apt to be found in the abdominal cavity, having escaped through rents in the arteries artificially produced. The pressure used by the embalmer when injecting the President's body was so great that in several places Dr. Bliss is constrained to speak of cavities, a large part of the fluid contained in which had probably transuded from the injecting material of the embalmer. The existence of other clots in the omentum, and elsewhere in the peritoneal cavity, shows that vessels altogether unconnected with the

wound were ruptured by the injecting pressure. The autopsy should have been made before the injection, and then we should have known whether a heart-clot as an accompanying factor of death from post-pyæmic exhaustion was present or not. A physician might well blush for a profession, a member of which could in the face of the criticism waiting to hang on every expression that fell from his lips, deliberately state that the President died of "Neuralgia of the Heart."

The primary cause of death was unquestionably pyæmia. The attending surgeons persist in speaking of septicæmia, and their apologist in the *Medical Record*, Dr. Shrady, ably shields their diagnosis by saying that strictly speaking there is no such thing as pyæmia. It should be known, he claims, as metastatic septicæmia. The attending surgeons knew and know what is meant by pyæmia, and deliberately denied, and still deny, that the condition passing under that name existed. The abscess of the parotid gland, the abscess in the kidneys, and the foci in the lungs are stubborn facts; but they do not appear to exist for those who seem interested in placing their critics in the wrong.

In the conclusion of the report it is stated:—

"The surgeons assisting at the autopsy were unanimously of the opinion that, on reviewing the history of the case in connection with the autopsy, it is quite evident that the different suppurating surfaces, and especially the fractured spongy tissue of the vertebra, furnish a sufficient explanation of the septic conditions which existed during life."

This is admitted to be a correct inference by all those competent to form an opinion. Probably many will cavil at the term "especially" as destined to make light of the responsibility involved in keeping up that largest suppurating area in the President's body, the fistulous tract.

The lessons to be drawn from this surgical case, and it must be borne in mind that just such a case is reported as recovered in Dr. Hamilton's text book, are the following:—

1st. Experiments with projectiles on the dead body do not constitute any guide applicable to given living cases of gun-shot wound."

2d. Surgeons will sin less by being bold in probing and examining wounds, even when near the great cavities of the body, than by being over-conservative and taking chances.

3d. With a constitution like that of Mr. Garfield, almost any operative procedure would be preferable to a conservatism which, through its efforts to keep up a false tract, increases the fatal chances of pyæmia.

Those interested in the mechanism of the impingement of projectiles, will scarcely credit the claim that the bullet was deflected at its impinging point on the eleventh rib. A bullet which crushes through two ribs, cuts clean through a vertebra, and penetrates altogether over eight inches of bony, muscular and fatty tissue in a straight line, and fired at so short a range, can scarcely have been deflected by the very rib it crushed to pieces. The simplest explanation of its course is, that the assassin fired at the President in a line directly continuous with the bullet track in the latter's body. That is, he fired while the plane of the President's back was oblique to the plane of the mouth of the revolver. With this the account given by the assassin himself, the coolest and most unmoved witness of the deed, is in accord throughout.

It should be recollected, what seems to have been overlooked by most or all of those who have criticized this case, that the relations of the parts into which the bullet was fired, were altogether different at the time of the assassination than when the autopsy was performed. At that time large, fatty and muscular masses had to be traversed by the ball, which, in the course of the wasting process ensuing, had nearly disappeared.

It is unfortunate that the brain was not examined. The continual delirious state of the sufferer suggests some metastatic affection of that organ. Probably the reason this organ was not examined was the desire to avoid disfigurement, but the brain can be removed in even a bald person without the latter.

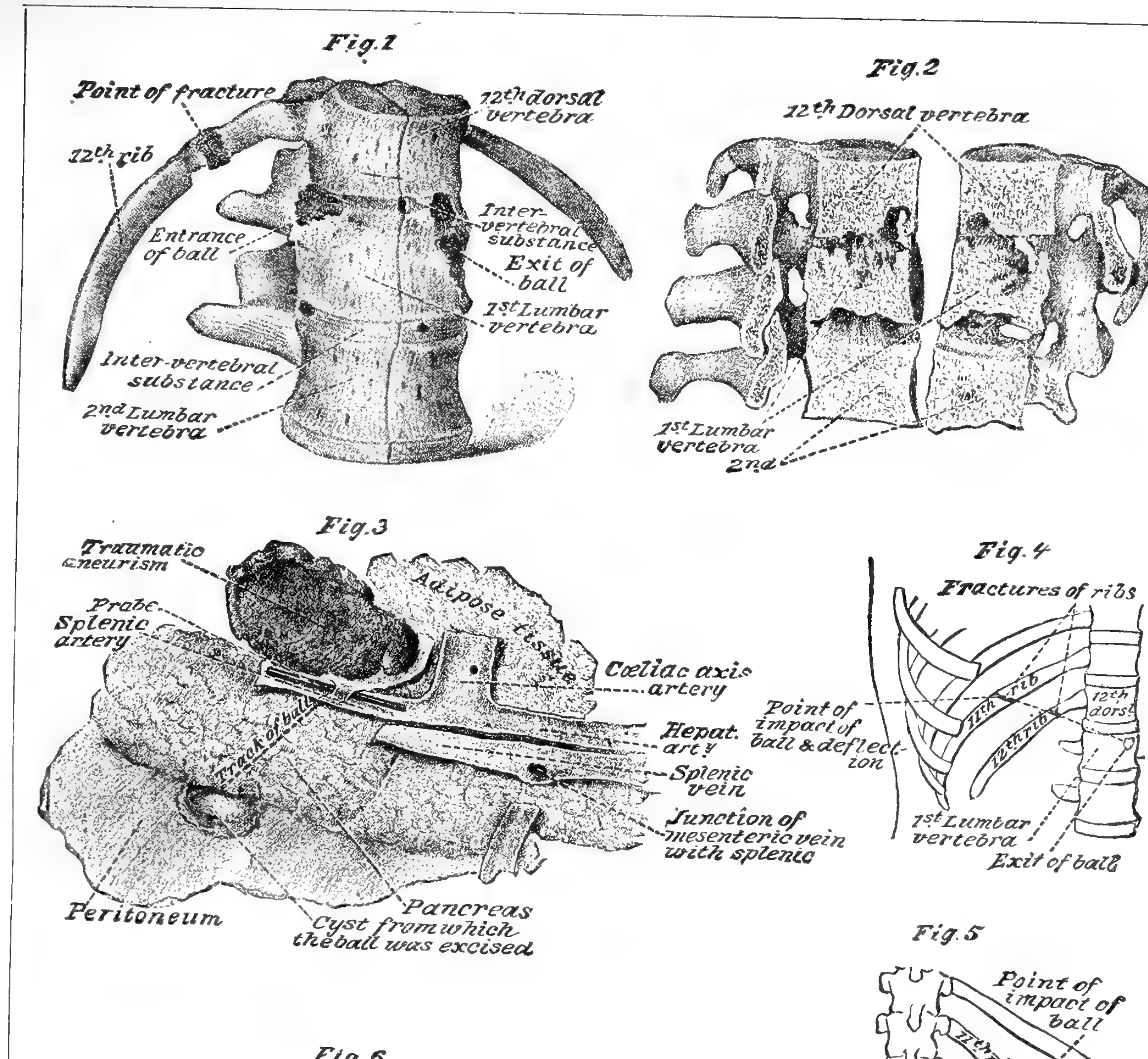
It may be here urged that the early performance of the autopsy should not have interfered with the subsequent embalming. The embalming procedure resorted to in the President's case was of the most routine and imperfect character, and not remotely comparable to the perfected processes employed by the German and Italian anatomists and embalmers.

REFERENCES TO OUR DIAGRAM. PAGE 499.

P.	Pancreas.
p. p. p.	Peritoneal cavity, the contained intestines and omental masses omitted.
e'	Vertebral entry of bullet.
e''	" exit of "
Ca.	Ascending colon.
Cd.	Descending colon.
D.	Extraperitoneal part of Duodenum.
H.	Aorta.
V.	Vena Cava.
K. K.	Kidneys.
Sc.	Spinal Cord.
S.	Spleen.

The thick straight line represents the bullet track.

PROTOPLASM STAINED WHILST LIVING.—Mr. L. F. Henneguy publishes the result of some experiments made on living infusoria, in which he confirms the observations of Brandt, made in 1879, that an aqueous solution of aniline brown, known in commerce as Bismarck brown, will give an intense brownish-yellow color to the protoplasm of the infusoria without in any way interfering with their enjoyment of life. The coloration first appears in the vacuoles of the protoplasm, then this latter is itself stained, the nucleus being most generally not at first colored, and so being made more conspicuous. Experiments made on vegetable protoplasm seemed to exhibit the same result.



DRAWINGS ILLUSTRATING AUTOPSY ON THE BODY OF THE LATE PRESIDENT GARFIELD.

THE AMERICAN CHEMICAL SOCIETY.

The October meeting of the American Chemical Society was held Friday evening, the 7th inst., with Dr. A. R. Leeds in the chair. The following new members were declared elected: H. C. Heipe, Wm. L. Leman, Dr. H. Von Bauer, Lewis Habel, Dr. Lauber, Dr. P. Radenhauser, and Mr. A. L. Colby. The first paper announced was by Prof. Leeds "On the Comparative Purity of City Water." In consideration of the recent litigation in regard to the pollution of the water of the Passaic river, Prof. Leeds was appointed to investigate the purity of the water from a chemical standpoint. The water supply of the cities of Newark, Jersey City and Hoboken is taken from the above mentioned river. Before it reaches Newark, the sewerage of Paterson, a city of 50,000 inhabitants, is emptied into it: besides this, along the river, the stream receives the refuse from a number of factories.

A short distance beyond Paterson, at a paper mill where carbolized paper was manufactured, the entire refuse was dumped into the river. In consequence of the dissolving of the carbolic acid, its presence was soon detected at Jersey City and Hoboken, and it became so objectionable that the water could not be used for drinking purposes. Legal measures were at once adopted, and the nuisance stopped. Simultaneous collection of specimens of the drinking water of the leading cities of the United States were collected, and a comparative examination of the organic matter (estimated according to the permanganate method) undertaken. Without any other special reference to the data given by Prof. Leeds, his results were as follows:—

. The purity of drinking water:—

1, Brooklyn; 2, Rochester; 3, Philadelphia; 4, Baltimore; 5, New York; 6, Washington; 7, Newark, Jersey City and Hoboken; 8, Cincinnati; 9, Boston; 10, Oswego; 11, Wilmington, Del.

In answer to questions which arose during the discussion of the paper, it was stated that during the past summer an excessive amount of chlorine was found in the analysis of the Passaic river, a fact contrary to all previous experience, and one which was considered as due to the extreme drought of the past summer so diminishing the amount of fresh water that the sea water had extended quite a ways back up the river. A similar circumstance was stated in regard to the Hudson river this year, the salt water being detected higher than usual. In regard to the statements recently made by Prof. Huxley in reference to the spread of disease by germs in the water, a very significant fact was mentioned by Prof. Leeds, in commenting on the rags used in the paper mills, who stated that they were imported from the plague stricken regions of Smyrna, and yet not one case of analogous disease had been observed from those who used the water, in which these rags were cleansed, for drinking purposes. The desirable property of precipitating out organic material from water by the use of the basic chloride of iron was remarked by Dr. E. R. Squibb. This fact has been used to advantage by one of the large hotels at Coney Island.

"Upon some new Salts of Thymole Sulpho Acid, and some new facts concerning the same," was the title of the second paper. It was by Mr. James H. Stebbins, Jr., and was essentially a resumé of some recent salts prepared by him and description of their important characteristics.

The third paper was by Dr. W. Hempel, who gave in the German language a descriptive "Exhibition of some new Gas Apparatus." Not only were they exhibited, but Dr. Hempel, in the presence of the Society, made analysis of the illuminating gas (which he considers superior to that used in Europe) and of the air. To those who are especially interested in this branch his recently published book will give the requisite information, and for the average reader a general description is almost impossible without cuts.

M. B.

MOUNDBUILDER SKELETONS.*

I.

By W. C. Holbrook, Coleta, Ill.

The skeletons found in the mounds of Rock River Valley, although always partially decomposed, present the following anatomical peculiarities:—

The *cranium* is small, low and broad. The superciliary ridges are very large and cause the forehead to appear even lower than it really is.

The malar process and the zygoma small and low.

Traces of a *frontal suture* are sometimes found in adult skulls. In the skull of a child about six years old, the suture was well developed. It appears that the two lateral portions of the frontal bone did not then unite as early in life as they now do, and that the traces of this suture remained through life in some persons. In one adult skull I found ten bones, viz.: two occipital, two parietal, two frontal, two temporal, sphenoid and ethnoid. The occipital was divided into two lateral portions by an *occipital suture.*

The *frontal suture* was also well developed. The *sagittal suture*, therefore, extended from the glabella over the vertex to the *foramen magnum.*

The *sagittal suture* is usually quite short. In one skull it measured only 4 67-100 inches, and the frontal and the occipital bones in this specimen were normal.

The supraorbital foramen is usually large and about one-eighth of an inch above the orbit. I never saw a supraorbital notch in a moundbuilder skull. *Ossa triquetra* are very uncommon and are confined to the lambdoid suture. This suture, together with traces of an occipital suture sometimes form one or two large triangular *ossa triquetra* in the superior angle of the occipital bone.

The posterior half of the synamus-suture is often completely grown up and the adjacent part of the temporal and parietal bones completely united.

The grooves for the *arterie menigea media* are very deep, while the foresæ that correspond to the brain are shallow and indistinct. The frontal sinus large and triangular in shape. The lower joint was large, massive and broad. The teeth are usual remarkably sound. I have never found but two or three "decayed teeth" in all of my explorations. Toothache was not, therefore, one of the troubles that beset the moundbuilders.

The humerus presents one marked peculiarity. About midway between the external and the internal condyloid ridges, and in the center of the fossa for the coronoid process of the ulwa, there is sometimes a well developed foramen.

In some mounds that contained fifteen or twenty persons this foramen was found in more than fifty per cent. of the humeri. I sometimes found it in both the right and the left arms. When only present in one arm, traces of an obliterate or grown up foramen were sometimes found in the opposite arm. Traces of this foramen are quite frequent, and in all moundbuilder humeri, the flat portion of bone between the condyloid ridges are very thin. This foramen is usually small and circular. Sometimes, however, it is large and triangular in shape, the base of the triangle parallel with the trochlea and the sides parallel with the condyloid ridges. A nutritious foramen sometimes enters the lower end of the shaft of the humerus at the superior angle of this triangular foramen. I believe that the moundbuilders were slowly outgrowing this Simian characteristic, for the humeri containing the triangular foramens are found in the oldest mounds, and are associated with the lowest foreheads and the smallest crania. In both the right and the left humerus of the skeleton whose skull contained ten bones, I found this foramen well developed. In the more modern mounds this foramen is less frequently found, and when present, is small and circular. The

* American Association for the Advancement of Science, 1881.

partition of bone between the two fossæ is also thicker.

The elimination of this "degraded affinity" is but one instance of the general evolution that has shaped or moulded all of the innumerable forms of animal and vegetable life. The vertebral column is always so badly decayed that anatomical comparison is impossible. The parts that "resist decay" indicate great physical perfection and strength. The Sacrum presents different forms in respect to curvature. Sometimes it is very slight, while in other specimens it is considerable. This curvature of the Sacrum is a more constant sexual characteristic in mound-builder skeletons than in the Caucasian or the African races, but I have not examined specimens enough to tabulate the difference. The only constant sexual characteristic of the Sacrum among all races of men is its greater breadth in the female, and this characteristic is well developed in the mound-builder skeletons. Comparing portions of almost every one of the larger bones of the mound-builder skeletons with several Caucasian and one Negro, and two Indian skeletons, it is certain that the primitive people of the Rock River Valley were strong, broad-shouldered, muscular men, with broad, round faces, and low receding foreheads. Exostosis, or foreign growth of bone, has been found. One, I remember, was found in one of the mounds near Sterling, Illinois. The foreign growth of bone in this specimen was stratified—deposited on the surface of the bone, in thin layers, like the layers in stalagmite. Bones exhumed from a mound on the west bank of Rock River, near Como, were very brittle, of a light and beautiful purplish color, when recently broken, and contained no animal matter. They resemble, in every respect, the bones exhumed from the church-yard of Ste. Genevieve, Paris, after a burial of over seven hundred years.—(Orfila Exhumations Juridiques, Vol. I., p. 350.)

WHITE CORPUSCLES OF THE BLOOD.

The London *Lancet* draws attention to an interesting memoir on the White Corpuscles of the Blood, which appears in the part just issued of the *Archives de Physiologie*, in which M. Renaut describes the different forms presented by the white corpuscles in different animals. In the river crayfish, for example, besides the ordinary lymph-corpuscles, there are many larger bodies with well defined nuclei, the protoplasm of which contains large highly refracting granules, resembling in many respects the vitelline granules of the frog and other batrachia. These corpuscles have a sharply limited but thin exoplastic pellicle; and if a drop of such lymph be allowed to fall into a drop of a one per cent. solution of osmic acid, the white corpuscles are instantly fixed, with their pseudopodia or protoplasmic processes extended; and these processes can then be seen to perforate the thin membrane, now blackened with the acid. There are thus two kinds of white corpuscles in the decapod crustacea—the lymphoid corpuscles and the amœboid corpuscles. Do similar differences exist in the blood of vertebrata? In reply to this, M. Renaut states that in the blood of all the vertebrata, from the cyclostome to the saurians, the white corpuscles are of two kinds; one, the ordinary white corpuscle, composed of hyaline protoplasm, presenting many short projecting points, with a nucleus undergoing gemmation and sending forth branched pseudopodia when placed under favorable conditions; the other containing numerous brilliant granules embedded in the protoplasm and surrounding the nucleus. These resemble the second form of corpucle described above as existing in the lymph of the river crayfish, but differ from them in having no outer limiting layer of condensed protoplasm, or exoplasm, as Haeckel has named it. The application of osmic acid shows that they may be subdivided into two other forms: one closely analogous to cells undergoing transformation into fat-cells, which present numerous granules, and stain black with osmic acid, and another set which contains granules that are not fatty, but which stain red with eosine. The best mode of demonstrating the existence of these three forms is to fix the blood in the rete mirabile of the capillary of the choroid in the posterior segment of the eye of a frog, by removing the anterior segment and exposing it to the vapor of osmic acid. At the expiration of twelve hours the eye is removed from the vapor, washed, the chorio-capillaris detached from the retina, and spread on glass; it is afterwards colored with, and mounted in, hæmatoxylate of eosine. The corpuscles may then be studied, and the three forms of ordinary, granular, and fatty corpuscles can be easily distinguished. M. Renaut finds that the white corpuscles of mammals generally, and of man in a state of health, all closely resemble each other, and are of the ordinary kind; but in disease, as in leucocythæmia, the white corpuscles are not only greatly increased in number, but vary considerably in size. Moreover, they are round, and present no pseudopodia. They are hyaline, and have a smooth, well defined limiting membrane, and some of them have nuclei which have undergone fission, just as in a cell that is about to segment. Hence, he is of the opinion that the white corpuscles multiply and increase in number whilst floating in the blood; other corpuscles may be observed, which are charged with granules of some proteid substance, resembling vitelline granules, or small masses of hæmoglobin; and, lastly, there are still other cells, which are charged with fat. M. Renaut has made some observations on the development of the red corpuscles of the lamprey, and gives the following succession of forms:—White corpuscle with nucleus proliferating, and protoplasm, not limited by an exoplasmic layer; corpuscle with nucleus proliferating, the protoplasm forming an uncolored disc, limited by an exoplasm; corpuscle with proliferating nucleus, protoplasm limited by an exoplasm, and forming a disc, more or less charged with hæmoglobin; red corpuscle with proliferating nucleus; and, finally, circular red corpuscle, with rounded nucleus.

MICROSCOPY.

It has been decided by the Executive Committee of the American Society of Microscopists to convene the next annual meeting of the Society at Elmira, N. Y., August 17, 1882, at 10 A. M. It is thought that there will be papers and discussions enough at Elmira to occupy us four days; thus, by adjourning Friday evening, August 20, or Saturday noon, August 21, there will be ample time—for those who wish to do so—to reach Montreal in time for the meeting of the A. A. A. of Science on Tuesday, August 24.

At the Columbus meeting Mr. E. H. Griffith, of Fairport, N. Y., a member of Executive Committee of this Society, renewed his generous offer of a prize of a Bausch & Lomb half-inch objection of 98° air angle (about 0.76 numerical aperture), to be awarded as follows:

"The prize shall be assigned to the author of the best paper on the adulteration of some important article of food or medicine. The paper shall be accompanied by permanently mounted slides, illustrating the various points under discussion; all papers and slides to become the property of the Society. The papers and accompanying studies to be in the possession of the President on the first day of the next annual meeting. He shall appoint a committee of three to examine the same, and report the name of the successful candidate before the close of the meeting. The names of the competitors shall not be made known to any member of the committee until after the award is made. The award shall not be made unless there shall be more than one competitor."

In order to carry out Mr. Griffith's instructions the following rules are established:

1. The papers and slides for the competition should be forwarded to Elmira in time to reach there before the day of the beginning of the next annual meeting, and should be addressed:

To the President of
THE AMERICAN SOCIETY OF MICROSCOPISTS,
Care of the Elmira Microscopical Society, Elmira, N. Y.

2. The envelopes should bear in their upper left-hand corner one of the enclosed labels, with some appropriate name or device other than the name of the author. The slides should bear the same label similarly marked (ten labels are sent with each circular).

3. Each paper should be accompanied by a sealed envelope bearing the same label and device, containing a slip with the name of the author.

4. The committee will be appointed on the first day of the Elmira meeting, and the papers and slides put into their hands. When they reach a decision, they will make a public report, stating the name or device of the successful paper. The sealed envelope bearing the same will then be opened by the President, who will announce the name of the successful author.

Persons not now members, but who shall become so at the Elmira meeting, are eligible as competitors for this prize, and can obtain the necessary labels by making application to the Secretary.

It is hoped that several papers will be received in competition for this prize which shall be found worthy of publication in the Transactions, though only one, of course, can receive the prize. The envelopes containing the names of unsuccessful competitors will not be opened except by permission of the authors, but will be destroyed by a committee appointed for that purpose.

Dr. George E. Blackham, the present President of the Society, has issued a stirring address to the members, and makes some excellent suggestions to those who would by their personal acting promote the success of the society.

Professor D. S. Kellicott of 119 Fourteenth Street, Buffalo, N. Y., is the Secretary of the Society.

FOSSIL POLYZOA—NOMENCLATURE.

In the second report of the committee consisting of Prof. P. M. Duncan and Mr. G. R. Vine, appointed for the purpose of reporting on Fossil Polyzoa, for the British Association, the order is divided into three subdivisions.—

1. *Cheilostoma*, Bark. = *Celleporina*, Ehrenberg.
2. *Cyclostomata*, „ = *Tabuliporina*, Milne-Ed., Hagenow, Johnston.
3. *Ctenostomata*, „

The following terms are used in this Report in describing the genera:—

ZOARIUM.—"The composite structure formed by repeated gemmation" = Polyzoarium and Polypidom of authors.

ZOŒCIUM or cell.—"The chamber in which the Polypide is lodged.

CŒNŒCIUM.—"The common dermal system of a colony." Applicable alike to the "Frond," or "Polyzoary," of Fenestella, Polypora, Phyllopora, or Synocladia: or to the associated Zoœcia and their connecting "interstitial tubuli," of Ceriopora, Hyphasmapora, and Archæopora, or species allied to these.

FENESTRULES.—The square, oblong, or partially rounded openings in the zoarium—*connected by non-cellular dissepiments*—of Fenestella, Polypora, and species allied to these.

FENESTRÆ applied to similar openings, whenever connected by the general substance of the zoarium—as in Phyllopora, Clathropora, and the Permian Synocladia.

BRANCHES.—The CELL-bearing portions of the zoarium of Glauconome, Fenestella, Polypora, or Synocladia; or the offshoots from the main stem of any species.

GONÆCIUM,—"A modified zoœcium or cell, set apart for the purposes of reproduction."

GONOCYST.—"An inflation of the surface of the zoarium in which the embryos are developed." Modern terms from the Rev. Thos. Hincks.

"PRESERVED VEGETABLES."

By OTTO HEHNER, F. I. C., F. C. S.

When some time ago public attention was forcibly drawn to the occasional injurious effects of preserved "canned" goods, I undertook a lengthy series of chemical and physiological experiments to ascertain the cause of such poisonous action. The results having so far only been communicated to professional chemists (*The Analyst*., vol. v., No. 57), I hope you will allow me, by way of affirmation of the paragraph in *The Lancet* of September 24, to give a short summary of them, as I think they may be of interest, and of some degree of importance, to medical readers.

Very frequently the gastric disturbances traceable to the consumption of preserved articles of food have been assigned to traces of lead dissolved from the solder with which the tins are closed, or present as impurity in the metal with which the can is lined. Now, although the occasional though very rare presence of lead in such articles cannot be denied, the effects should be attributed to the *tin* itself. Tin, even perfectly pure, is far more readily attacked by food matters than is commonly supposed; it is to be found in comparatively large amounts in an overwhelming majority of canned goods, irrespective of the nature of the same. Acid fruits, such as peaches or cherries, corrode the tins to an appalling extent; but even meats, nay, condensed milk, dissolve and become contaminated with serious quantities of the metal.

I base my observations upon the examination of the following foods:—Vegetable: French asparagus, American asparagus, peas, tomatoes, peaches (three different brands), pine apple (two kinds), white and red cherries, and marmalade. Animal: Corned beef (five brands), ox cheek, ox tongue (three kinds), collared head, tripe, oysters, sardines in oil, salmon, salmon cutlets, lobster, shrimps, curried fowl (two kinds), boiled rabbit, boiled mutton, roast chicken, roast turkey, ox cheek soup, gravy soup, sausages, condensed milk (three brands).

With the exception of the sausages, the whole of the samples contained more or less tin, many to such an extent that abundant reactions could be obtained from two or three grammes of the vegetable substances; whilst of the animal foods one of the soups contained thirty-five milligrammes, one of the condensed milks eight milligrammes, and oysters forty-five milligrammes of tin to the pound.

Pure tin is readily attacked even by carbonic acid in solution, all samples of soda-water or of other aerated beverages which I have tested giving distinct *tin* reactions. Aerated beverages are generally stated to be liable to lead contamination, but seeing that lead does not enter into the composition of any of the pipes or vessels of the machines made by modern manufacturers, I do not doubt but that the black coloration produced by sulphuretted hydrogen in the beverage in question has usually been erroneously attributed to lead, and is in reality due to tin. Tin, in fact, prevents the lead passing into solution; it completely precipitates the metal from lead solutions, an equivalent quantity of tin being taken up.

The question arises, is tin, when taken into the system, injurious to health or not? Forensic literature does not furnish a positive or satisfactory reply, but the following experiments appear to me completely to settle the point.

A half-grown guinea-pig took with its ordinary food seventy-five milligrammes of pure *stannous* hydrate in

two doses of twenty-five and fifty milligrammes each. Death resulted under symptoms of irritant poisoning. Tin was detected in large amount in the fæces and in the viscera, notably the liver.

Another similar animal took within three days, in six doses 450 milligrammes of *stannic* hydrate, *without* serious effect, tin appearing abundantly in the excrements. Accustomed in a manner to stannic salts, it quickly succumbed to fifty milligrammes of stannous hydrate.

It plainly follows that while stannic compounds are not injurious in the doses given, tin in the stannous condition is a virulent irritant poison.

These experiments lead me most strongly to support your demand for a better method of packing preserved food matters than in tin canisters. Tin invariably dissolves in the stannous condition in such solvents as occur in vegetable or animal substances, and the amount of oxygen in the sealed canisters being very minute, oxidation cannot render the metal comparatively unobjectionable.

I trust that the medical profession will object, unmistakably and strongly, to the administration of tin by grocers and oilmen to young and old alike, and, whilst acknowledging the enormous benefits conferred upon the masses by the introduction of preserved foods, will insist that the present system of packing be speedily abandoned.—*Lancet.*

CORRESPONDENCE.

To the Editor of " SCIENCE."

DEAR SIR: I have carefully read your article on " The Warner Astronomical Prizes," published in SCIENCE, of Sept. 24, wherein myself and Mr. Warner, are severely and unjustly criticised. In a former number you had criticised one of the conditions of the prize: viz, that " the comet must be telescopic and unexpected,' saying that a person might discover a comet by the aid of an opera glass. But what, I ask, is an opera glass but a telescope. In order to defend myself from even the semblance of crookedness, allow me to state a few facts, familiarity with which would, doubtless, have kept you from error. When the great comet (known as comet B) made its appearance so suddenly, all familiar with the conditions of the award, conceded that no just demand on Mr. Warner could be made, as it was neither telescopic nor unexpected, but very many people, not conversant with the conditions, and supposing that it applied to all comets, began to send in claims for discovery. Then Mr. Warner said, inasmuch as the comet was such a large and brilliant one, and that so many seemed not to have understood the conditions imposed, he would offer a *special* prize of $200 to the one whom I, after an examination of claims, should decide had first seen it. It is a point of no little significance, to remember that this in no sense was to be considered as the Warner-prize proper to be adjudicated upon by Profs. Hall and Young, *in the event of a controversy*, but was distinctly stated to be a *special* prize. The conditions of the original prize were neither in this, nor any other instance, to be deviated from. From a misconception of this vital point, which, under the circumstances, was, perhaps, natural, you have endeavored to make your readers believe that Mr. Warner took—wrongfully and unjustly—the matter out of the hands of Profs. Hall and Young, and placed it in my own, but you are grievously in error. I do not purpose to burden your columns with the reasons for not awarding the prize for comet B. Not an astronomer in the world, with all those letters before him, would have awarded it.

You make the task of deciding the question a very easy one, and so might I have found it by placing myself in the position of a judge, who must decide according to the evidence, true or false. Instead of condensing the letters to a half dozen, as you suggest, I could have reduced them to a single one, for one of the claimants solemnly declared that he saw it a year ago last August, and that he had watched it ever since, while another averred that he discovered it last January, and several claimed it before its discovery in South Africa, and some of these statements were sworn to at that.

Every astronomer knows that the comet (which was discovered in South Africa on May 21), in its northward journey, passed the sun, 8° west of it, at noon on the 19th of June, and, therefore, after its disappearance in the southern hemisphere, could not have been seen by any person, in any part part of the world, before the morning of the 22d of June, and yet not less than 1000 persons claimed (the statements of many being substantiated by affidavits), that they saw the comet at dates ranging all the way from May 1 to June 20. Was I to accept such statements as those, and accord to them the dignity of evidence, and award the prize for an invisible comet? The comet first became visible to us near the time of the summer solstice, when twilight commenced at about half past two A. M., which rendered even a bright and expected comet very difficult to see until its declination north became at least 15° greater than the sun's. Your assertion that I have awarded myself the prize for the discovery of one comet, is erroneous to the last degree. Where there is but one claimant, as was the case with Swift's, with Schaeberle's, and with Barnard's comets, Mr. Warner, without consultation with any one, pays the prize. Should any dispute arise as to priority of discovery, &c., then, according to the conditions, the matter was to be left to Profs. Hall and Young for a decision.

Again, you do me great injustice in saying that the essays ought not to be filed with me, because I am both a competitor and a judge. I am not a competitor for that prize, nor am I to be a judge. The essays are placed in my hands for safe keeping, and when the first of November arrives, will three astronomers (if as many can be found who are not competitors) be appointed as judges, to whom I shall send the essays for a decision agreeably to condition. 3d. As to who will appoint the judges, I am as ignorant as are you.

Trusting you will give this letter in its entirety, to the public, through the columns of your journal, I remain,

Yours truly, LEWIS SWIFT.

ROCHESTER, *October* 10, 1881.

RELATIONS BETWEEN THE CRANIUM AND THE REST OF THE SKELETON.—These relations form the subject of a paper by M. Manouvier, read at the last meeting of the French Association. The following are the author's conclusions:— 1. The weight of the cranium varies, in a general way, with the weight of the skeleton, but not proportionally, like the weight of the brain. 2. The weight of the skeleton, less the cranium, in a given race, varies nearly in proportion to the weight of the femur. 3. The weight of the cranium is greater relatively to that of the femur, the lighter the latter is. 4. The weight of the cranium is much more considerable relatively to that of the femur in woman than in man. 5. This sexual difference is so pronounced that it constitutes one of the best secondary sexual characters. About 82 women in 100 have the cranium heavier than the two femurs, while 82 men in 100 have it lighter. 6. The lower jaw is heavier relatively to the cranium in the anthropoids than in man, is inferior than in civilized races, in man than in woman, and in the adult than in the child. 7. The weight of the cranium is smaller relatively to that of the lower jaw, the heavier the latter is, etc.

NOTES AND QUERIES.

ELECTRICITY.—I wish to inquire if it has been determined whether upon the union of two currents of electricity of different electromotive forces, they form one current of an intermediate intensity, as two streams of water of different temperatures would form one of an intermediate temperature. Or, whether they each retain its own E. F. M., and follow the terms of its own intensity. And, if the problem has been determined, where I can find the particulars.

And, further, if the two retain their separate identities, whether any instrument has been made to measure the different quantities and intensities which pass in a single conductor.

This is a very important question in view of the great practical problems which we have now to solve in regard to the production and use of electricity.

SAML. J. WALLACE.

THE COLOR CHANGES OF AXOLOTL.—Prof. Semper has lately examined axolotl with regard to the influence of light on its color (*Würzburg Phys. med. Ges*). When young axoloti are reared in darkness they become quite dark; nearly as dark in red light; in yellow, on the other hand, pretty bright; and brightest in bright daylight. The difference is connected not only with the chromatic function found in various degrees in all amphibia, but on pronounced formation of a peculiar diffuse yellowish green coloring matter, increase of white, and diminution of dark chromatophores. Further, when axolotl are exposed to daylight in white dishes covered with white paper, much less dark pigment forms in them than when they are kept in white dishes without a paper cover (other things equal); though in the latter case they are apparently exposed to the most intense light; these darker axolotl are, however, still much brighter than those reared in red light or in darkness. Since (as experiment showed) the white covering paper let through much light, but very little of the chemical rays, it appears that chemical rays play no part in the formation of pigment. But the causes of the whitening in bright daylight and the darkening in absence of light remain unknown as before.

THE BLOOD OF INSECTS.—Operating with the larva of *Oryctes nasicornis*, M. Fredericq has observed (*Bull. Belj. Acad.*) that the blood of the animal, drawn off in a small glass cannula, is a colorless liquid, but on exposure to the air presently takes a decided brown color, and coagulates. The coloration he regards as a purely cadaveric phenomenon. The substance which becomes brown is probably formed in the moment of coagulation, and does not serve in the body as a vehicle between the external air and the tissues, like *hæmoglobin* in Vertebrates and many Annelids, *hæmocyanin* in Crustaceans, &c. When the larva is kept a quarter of an hour in hot water (50° to 55°), the blood extracted does not coagulate or become brown. Once the substance which browns is produced, even a boiling temperature does not prevent its browning. The brown substance once formed is very stable, not being decomposed either by acids or alkalies, and not made colorless by being submitted to vacuum or kept in a closed vessel. The existence of an intermediary in insects corresponding to hæmoglobin M. Fredericq thinks very problematical in view of the anatomical system, letting air penetrate into the heart of the tissues.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 15, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 9	29.899	29.900	9 a. m.	29.862	3 a. m.	63.3	59.6	71	0 a. m.	64	10 a. m.	55	12 p. m.	55	12 p. m.	100.
Monday, 10	30.069	30.324	12 p. m.	29.900	0 a. m.	54.6	49.3	64	2 p. m.	55	12 m.	41	12 p. m.	40	12 p. m.	128.
Tuesday, 11	30.426	30.478	9 a. m.	30.324	0 a. m.	44.7	41.7	51	5 p. m.	47	5 p. m.	37	5 a. m.	37	7 a. m.	119.
Wednesday, 12	30.179	30.396	0 a. m.	30.096	12 p. m.	54.0	52.0	57	2 p. m.	55	3 p. m.	41	2 a. m.	41	2 a. m.	92.
Thursday, 13	29.968	30.096	0 a. m.	29.908	4 p. m.	65.7	62.3	73	4 p. m.	67	4 p. m.	55	6 a. m.	55	6 a. m.	130.
Friday, 14	30.239	30.298	9 a. m.	30.000	0 a. m.	50.0	46.6	60	0 a. m.	57	0 a. m.	45	8 a. m.	43	8 a. m.	120.
Saturday, 15	30.085	30.244	0 a. m.	30.006	12 p. m.	62.0	58.3	69	3 p. m.	63	4 p. m.	50	0 a. m.	48	0 a. m.	121.

Mean for the week .... 30.123 inches.
Maximum for the week at 9 a. m., Oct. 11th .... 30.478 "
Minimum " at 3 p. m., Oct. 9th .... 29.862 "
Range .... .616 "

	Dry.		Wet.
Mean for the week	56.3 degrees		52.8 degrees.
Maximum for the week, at 4 pm. 13th	73. "	at 4 pm 13th,	67. "
Minimum " " 5 am. 11th	37. "	at 7 am 11th,	37. "
Range " "	36. "		30. "

OCTOBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 9	n. w.	w. n. w.	w. n. w.	133	½	1.00 am	.509	.438	.449	74	68	100	8 cu.	9 cu.	0	-----	-----	-----	--	5
Monday, 10	n.	n. n. w.	n. n. w.	242	7	3.40 pm	.321	.285	.251	74	48	84	0	3 cu.	0	0.30 pm	5.00 pm	4.30	.19	6
Tuesday, 11	n. e.	n. e.	s. s. e.	240	6¼	0.15 am	.207	.199	.273	90	57	85	0	0	0	-----	-----	-----	--	0
Wednesday, 12	s. w.	s.	w. s. w.	158	4¾	1.50 pm	.297	.378	.420	85	81	93	9 cu.	10	10	-----	-----	-----	--	0
Thursday, 13	w. s. w.	w. s. w.	w.	146	8¼	9.00 pm	.420	.559	.599	93	71	84	6 cir. cu.	8 cu.	10	0.15 pm	4.00 pm	3.45	.09	7
Friday, 14	n. e.	e.	s. e.	198	4	9.10 am	.249	.257	.308	77	66	79	0	0	5 cu.	-----	-----	-----	--	1
Saturday, 15	s. s. w.	s. s. w.	s. s. w.	197	3¼	10.20 pm	.349	.457	.529	80	69	89	10	1 cu. s.	0	-----	-----	-----	--	0

Distance traveled during the week .... 1,314 miles.
Maximum force .... 8¼ lbs.

Total amount of water for the week .... .28 inch.
Duration of rain .... 8 hours, 15 minutes.

DANIEL DRAPER, Ph. D.
Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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SATURDAY, OCTOBER 29, 1881.

TO OUR ENGLISH READERS.

We have received from Messrs. Deacon & Co., of 150 Leadenhall street, London, England, a standing order for a large supply of "SCIENCE," which will be forwarded weekly. We shall be obliged if our English readers will make this fact known to their friends.

THE WARNER PRIZES.

WE afforded to Professor Swift ample space in our last week's issue, to reply to our strictures on his disposition of Mr. Warner's prize for Comet *b*, 1881. Our readers have now the facts before them and can judge for themselves on the merits of this matter. For ourselves we would say that, realizing the benefits that may accrue from Mr. Warner's gifts, we are not disposed to be too critical in regard to the benefactor nor to the dispenser, and we are far from supposing that either are knowingly walking in the paths of what Professor Swift calls "crookedness." But reading Professor Swift's reply, we cannot interpret it otherwise than as a confirmation of our objections to the course he has taken.

We admitted that, in this instance, under the conditions of the Warner prizes, no claimant could justly claim the prize. We followed by asserting, that as Mr. Warner waived the special conditions and told Mr. Swift to give the $200 to the man who first saw the Comet, it was his duty to have carried out his instructions to the letter.

Professor Swift confirms the position we took on this subject; in his letter he says: "all conceded that no just demands could be made on Mr. Warner" in regard to Comet *b*. Then Mr. Warner said, "inasmuch as the Comet was such a large and brilliant one, and as so many people seemed not to have understood the conditions imposed, *he would offer a special prize of* $200 TO THE ONE WHO I, after an examination of claims, should decide HAD FIRST SEEN IT."

Now comes the *muddle*. Mr. Warner admits that *under his conditions no one can claim the prize;* and therefore offers a special prize for THE ONE WHO FIRST SAW THE COMET. And yet Professor Swift in his letter of explanation says: "*the conditions of the original prize* were, neither in this nor in any other, *to be deviated from;*" and on this account concludes that "not an astronomer in the world would have awarded it."

What can be said or done with men who are so thoroughly and flagrantly inconsistent? Mr. Warner's course throughout appears to have been thoroughly practical; he saw the difficulty in awarding this particular prize, and met it in a most liberal spirit, and had his intentions been carried out, the thanks of the community would have been the unanimous response.

Passing over Professor Swift's apparent misinterpretation of Mr. Warner's instructions, the question may be asked: could "the one who had first seen it" be named? Waiving the claim of the "1000 persons with affidavits" who claimed to have seen the Comet in the United States before its possible appearance, and the 2000 other clod-hoppers and rustics whose claims appeared to have clouded the judgment of Professor Swift, we offer a few simple facts in regard to the first discoverer of Comet *b*, which would have influenced our judgment if called upon to decide on this matter:—

We believe that the first person in the United States who saw the Comet in question, noted its position, and duly reported the fact to Professor Swift was Mr. Edgar L. Larkin, of New Windsor, Ill. If Mr. Warner, however, prefers to award the prize to the *first person* who saw the Comet, irrespective of locality, then we are advised that the following facts bear on the subject:—

Dr. Gould's name was mentioned prominently in connection with its discovery, but according to his own statement, his attention was directed to it by his assistant, Mr. Wilson. But prior to this date it had been observed by Cruls, in Brazil, and also by several English astronomers at Melbourne. It now appears that Mr. John Tebbutt, of Windsor, New South Wales, is credited as the first astronomer to get an observation of this Comet; so that if the prize is to be awarded to the first discoverer, Tebbutt appears to be the man.

The assertion in Professor Swift's letter that Mr. Warner, without consultation with any, pays the prize in certain cases, causes us some surprise, as we thought that his previous experiences hardly warranted him to decide on matters astronomical, and that he delegated the task to others.

In regard to the prize essay, we would advise Mr. Warner to postpone the time of entry until January the 1st next, which will give a reasonable time for some creditable work to be done. We would also propose that the judges be named immediately. Professor Swift says in his letter, "as to who will appoint the judges I am as ignorant as are you." Who does know? Surely Mr. Warner will not propose to decide this matter.

In making these remarks we are far from desiring to disparage the value of such prizes as those offered by Mr. Warner. We understand that Mr. E. E. Barnard, who secured the last prize, is a young man under twenty-five years of age, and a self-taught astronomer. Under very discouraging financial circumstances he provided himself with a good five-inch telescope, with which he has done excellent work. His Warner prize will be turned to good account, as he writes to inform us that the $200 will enable him to purchase a plot of ground on which to build a house for his family; we need not add that an observatory will be a leading feature in Mr. Barnard's new house.

We feel a pleasure in showing the practical good Mr. Warner is doing by providing these scientific prizes, and we trust he may continue them during the following year. Our criticism is of a perfectly friendly character and made with some regret. We have received letters from subscribers confirming our view of the case, which will remain unpublished, as we desire to close the discussion.

ON THE DISCOVERIES OF THE PAST HALF-CENTURY RELATING TO ANIMAL MOTION.

By J. BURDON-SANDERSON, M. D., L.L.D., F.R.S.

[*Concluded from Page* 486.]

The living muscle of a frog is placed in a closed chamber, which is vacuous—*i. e.* contains only aqueous vapor. The chamber is so arranged that the muscle can be made to contract as often as necessary. At the end of a certain period it is found that the chamber now contains carbonic acid gas in quantity corresponding to the number of contractions the muscle has performed. The water which it has also given off cannot of course be estimated. Where do these two products come from? The answer is plain. The muscle has been living all the time, for it has been doing work, and (as we shall see immediately) producing heat. What has it been living on? Evidently on stored material. If so, of what nature? If we look for the answer to the muscle, we shall find that it contains both proteid and sugar-producing material, but which is expended in contraction we are not informed. There is, however, a way out of the difficulty. We have seen that the only chemical products which are given off during contraction are carbonic acid gas and water. It is clear, therefore, that the material on which it feeds must be something which yields, when oxidized, these products, and these only. The materials which are stored in muscle are oxygen and sugar, or something resembling it in chemical composition.

[1] Ludwig's first important research on this subject was published in 1881.

And now we come to the last point I have to bring before you in connection with this part of my subject. I have assumed up to this moment that heat is always produced when a muscle does work. Most people will be ready to admit as evidence of this, the familiar fact that we warm ourselves by exertion. This is in reality no proof at all.

The proof is obtained when, a muscle being set to contract, it is observed that at each contraction it becomes warmer. In such an experiment, if the heat capacity of muscle is known, the weight of the particular muscle, and the increase of temperature, we have the quantity of heat produced.

If you determine these data in respect of a series of contractions, arranging the experiments so that the work done in each contraction is measured, and immediately thereupon reconverted into heat, the result gives you the total product of the oxidation process of heat.

If you repeat the same experiment in such a way that the work done in each contraction is not so reconverted, the result is *less* by the quantity of heat corresponding to the work done. The results of these two experiments have been found by Prof. Fick to cover each other very exactly. I have stated them in a table[1] in which we have the realization as regards a single muscle of the following forecast of Mayer's as regards the whole animal organism. "Convert into heat," he said, "by friction or otherwise, the mechanical product yielded by an animal in a given time, add thereto the heat produced in the body directly during the same period, and you will have the total quantity of heat which corresponds to the chemical processes." We have seen that this is realizable as regards muscle, but it is not even yet within reach of experimental verification as regards the whole animal.

I now proceed abruptly (for the time at our disposal does not admit of our spending it on transitions) to the consideration of the other great question concerning vital motion, namely, the question how the actions of the muscles of an animal are so regulated and coordinated as to determine the combined movements, whether rhythmical or voluntary, of the whole body.

As every one knows who has read the "Lay Sermons," the nature and meaning of these often unintentional but always adapted motions, which constitute so large a part of our bodily activity, were understood by Descartes early in the seventeenth century. Without saying anything as to his direct influence on his contemporaries and successors, there can be no doubt that the appearance of Descartes was coincident with a great epoch—an epoch of great men and great achievements in the acquirement of man's intellectual mastery over nature. When he interpreted the unconscious closing of the eyelids on the approach of external objects, the acts of coughing, sneezing, and the like as mechanical and reflected processes, he neither knew in what part of the nervous system the mechanisms concerned were situated, nor how they acted.[2] It was not until a hundred

[1] RELATION OF PRODUCT AND PROCESS IN MUSCLE.
(Result of one of Fick's Experiments.)

Mechanical product	6670	grammemillimetres.
Its heat value	15.6	milligrammeunits.
Heat produced	30.0	"
Total product reckoned as heat	54.6	"

[2] Descartes' scheme of the central nervous mechanism comprised all the parts which we now regard as essential to "reflex-action." Sensory nerves were represented by threads (filets) which connected all parts of the body to the brain ("Œuvres," par V. Cousin, vol. iv., p. 359); motor nerves by tubes which extended from the brain to the muscles; "motor centres" by "pores" which were arranged on the internal surface of the ventricular cavity of the brain, and guarded the entrances to the motor tubes. This cavity was supposed to be kept constantly charged with "animal spirits" furnished to it from the heart by arteries especially destined for the purpose. Any "incitation" of the surface of the body by an external object which affects the organs of sense does so, according to Descartes, by producing a *motion* at the incited part. This is communicated to the pore by the thread and causes it to open, the consequence of which is that the "animal spirit" contained in the ventricular cavity enters the tube and is conveyed by it to the various muscles with which it is connected, so as to produce the appropriate motions. The whole system,

years after that Whytt and Hales made the fundamental experiments on beheaded frogs, by which they showed that the involuntary motions which such preparations execute cease when the whole of the spinal cord is destroyed—that if the back part of the cord is destroyed, the motions of the hind limbs, if the fore part, those of the fore limbs cease. It was in 1751 that Dr. Whytt published in Edinburgh his work on the involuntary motions of animals. After this the next great step was made within the recollection of living physiologists: a period to which, as it coincided with the event which we are now commemorating—the origin of the British Association—I will now ask your special attention.

Exactly forty-nine years ago, Dr. Marshall Hall communicated to the Zoological Society of London, the first account of his experiments on the reflux function of the spinal cord. The facts which he had observed, and the conclusions he drew from them, were entirely new to him, and entirely new to physiologists to whom his communication was addressed. Nor can there be any reason why the anticipation of his fundamental discovery by Dr. Whytt should be held to diminish his merit as an original investigator. In the face of this historical fact it is impossible to regard him as the discoverer of the "reflex-function of the spinal cord," but we do not the less owe him gratitude for the application he made of the knowledge he had gained by experiments on animals to the study of disease. For no one who is acquainted with the development of the branch of practical medicine which relates to the disease of the central nervous system will hesitate in attributing the rapid progress which has been made in the diagnosis and treatment of these diseases, to the impulse given by Dr. Marshall Hall to the study of nervous pathology.

In the mind of Dr. Marshall Hall the word reflex had a very restricted meaning. The term "excito-motary function," which he also used, stood in his mind for a group of phenomena of which it was the sole characteristic that a sensory impression produced a motor response. During the thirty years which have elapsed since his death, the development of meaning of the word reflex has been comparable to that of a plant from a seed. The original conception of reflex action has undergone, not only expansion, but also modification, so that in its wider sense it may be regarded as the empirical development of the philosophical views of the animal mechanism promulgated by Descartes. Not that the work of the past thirty years by which the physiology of the nervous system has been constituted can be attributed for a moment to the direct influence of Descartes. The real epoch-maker here was Johannes Müller. There can be no doubt that Descartes' physiological speculations were well known to him, and that his large acquaintance with the thought and work of his predecessors conduced, with his own powers of observation, to make him the great man that he was; but to imagine that his ideas of mechanism of the nervous system were inspired, or the investigations by which, contemporaneously with Dr. Marshall Hall, he demonstrated the fundamental facts of reflex action, were suggested by the animal automatism of Descartes, seems to me wholly improbable.

I propose, by way of conclusion, to attempt to illustrate the nature of reflex action in the larger sense, or, as I should prefer so call it, the Automatic Action of Centres, by a single example—that of the nervous mechanism by which the circulation is regulated.

although it was placed under the supervision of the "*âme raisonable*" which had its office in the pineal gland, was capable of working independently. As instances of this mechanism Descartes gives the withdrawal of the foot on the approach of hot objects, the actions of swallowing, yawning, coughing, etc. As it is necessary that, in the performance of these complicated motions, the muscles concerned should contract in succession, provision is made for this in the construction of the system of tubes, which represent the motor nerves. The weakness of the scheme lies in the absence of fact basis. Neither threads nor pores nor tubes have any existence.

The same year that J. R. Mayer published his memorable essay, it was discovered by E. H. Weber that, in in the vagus nerve, which springs from the medulla oblongata and proceeds therefrom to the heart, there exists channels of influence by which the medulla acts on that wonderful muscular mechanism. Almost at the same time with this, a series of discoveries[1] were made relating to the circulation, which, taken together, must be regarded as of equal importance with the original discovery of Harvey. First, it was found by Henle that the arterial blood-vessels by which blood is distributed to brain, nerve, muscle, gland, and other organs, are provided with muscular walls like those of the heart itself, by the contraction or dilation of which the supply is increased or diminished according to the requirements of the particular organ. Secondly, it was discovered simultaneously, but independently, by Brown-Séquard and Augustus Waller, that these arteries are connected by nervous channels of influence with the brain and spinal cord, just as the heart is. Thirdly, it was demonstrated by Bernard that what may be called the heart-managing channels spring from a small spot of gray substance in the medulla oblongata, which we now call the "heart-centre;" and a little later by Schiff, that the artery-regulating channels spring from a similar head central office, also situated in the medulla oblongata, but higher up, and from subordinate centres in the spinal cord.

If I had the whole day at my disposal, and your patience were inexhaustible, I might attempt to give an outline of the issues to which these five discoveries have led. As it is, I must limit myself to a brief discussion of their relations to each other, in order that we may learn something from them as to the nature of automatic action.

Sir Isaac Newton, who, although he knew nothing about the structure of nerves, made some shrewd forecasts about their action, attributed to those which are connected with muscles an alternative function. He thought that by means of motor nerves the brain could determine either relaxation or contraction of muscles. Now as regards ordinary muscles, we know that this is not the case. We can will only the shortening of a muscle, not its lengthening. When Brown-Séquard discovered the function of the motor nerves of the blood-vessels, he assumed that the same limitation was applicable to it as to that of muscular nerves in general. It was soon found, however, that this assumption was not true in all cases—that there were certain instances in which, when the vascular nerves were interfered with, dilatation of the blood-vessels, consequent on relaxation of their muscles, took place; and that, in fact, the nervous mechanism by which the circulation is regulated is a highly-complicated one, of which the best that we can say is that it is perfectly adapted to its purpose. For while every organ is supplied with muscular arteries, and every artery with vascular nerves, the influence which these transmit is here relaxing, there constricting, according (1) to the function which the organ is called upon to discharge; and (2) the degree of its activity at the time. At the same time the whole mechanism is controlled by one and the same central office, the locality of which we can determine with exactitude by experiment on the living animal, notwithstanding that its structure affords no indication whatever of its fitness for the function it is destined to fulfill. To judge of the complicated nature of this function we need only consider that in no single organ of the body is the supply of blood required always the same. The brain is during one hour hard at work, during the next hour

[1] The dates of the discoveries relating to this subject here referred to are as follows:—Muscular Structure of Arteries, Henle, 1841; Function of Cardaic Vagus, E. H. Weber, 1845; Constricting Nerves of Arteries, B. Séquard, 1858; Aug. Waller, 1853; Cardiac Centre, Bernard, 1858; Vascular Centre, Schiff, 1858; Dilating Nerves, Schiff, 1854; Eckhard, 1864; Lovén, 1866. Of the more recent researches by which the further elucidation of the mechanism by which the distribution of blood is adapted to the requirements of each organ, the most important are those of Ludwig and his pupils and of Heidenhain.

asleep; the muscles are at one moment in severe exercise, the next in complete repose; the liver, which before a meal is inactive, during the process of digestion is turgid with blood, and busily engaged in the chemical work which belongs to it. For all these vicissitudes the tract of grey substances which we call the *vascular centre* has to provide. Like a skilful steward of the animal household, it has, so to speak, to exercise perfect and unfailing foresight, in order that the nutritive material which serves as the oil of life for the maintenance of each vital process, may not be wanting. The fact that this wonderful function is localized in a particular bit of grey substance is what is meant by the expression "automatic action of a centre."

But up to this point we have looked at the subject from one side only.

No state ever existed of which the administration was exclusively executive—no government which was, if I may be excused the expression, absolutely absolute. If in the animal organism we impose on a centre the responsibility of governing a particular mechanism or process, independently of direction from above, we must give that centre the means of being influenced by what is going on in all parts of its area of government. In other words, it is essential that there should be channels of information passing inwards, as there should be channels of influence passing outwards. Now what is the nature of these channels of information? Experiment has taught us not merely with reference to the regulation of the circulation, but with reference to all other automatic mechanisms, that they are as various in their adaptation as the outgoing channels of influence. Thus the vascular centre in the medulla oblongata is so cognizant of the chemical condition of the blood which flows through it, that if too much carbonic acid gas is contained in it, the centre acts on information of the fact, so as to increase the velocity of the blood-stream, and so promote the arterialization of the blood. Still more strikingly is this adaptation seen in the arrangement by which the balance of pressure and resistance in the blood-vessels is regulated. The heart, that wonderful muscular machine by which the circulation is maintained, is connected with the centre, as if by two telegraph wires—one of which is a channel of influence, the other of information. By the latter the engineer who has charge of that machine sends information to headquarters whenever the strain on his machine is excessive, the certain response to which is relaxation of the arteries and diminution of pressure. By the former he is enabled to adapt its rate of working to the work it has to do.

If Dr. Whytt, instead of cutting off the head of his frog, had removed its brain—*i. e.*, the organ of thought and consciousness—he would have been more astonished than he actually was at the result; for a frog so conditioned exhibits, as regards its bodily movements, as perfect adaptiveness as a normal frog. But very little careful observation is sufficient to show the difference. Being incapable of the simplest mental acts, this true animal automaton has no notion of requiring food or of seeking it, has no motive for moving from the place it happens to occupy, emits no utterance of pleasure or distress. Its life processes continue so long as material remains, and are regulated mechanically.

To understand this all that is necessary is to extend the considerations which have been suggested to us in our very cursory study of the nervous mechanism by which the working of the heart and of arteries is governed, to those of locomotion and voice. Both of these we know, on experimental evidence similar to that which enables us to localize the vascular centre, to be regulated by a centre of the same kind. If the behavior of the brainless frog is so natural that even the careful and intelligent observer finds it difficult to attribute it to anything less than intelligence, let us ask ourselves whether the chief reason of the difficulty does not lie in this, that the motions in question are habitually performed intelligently and consciously. Regarded as mere mechanisms, those of locomotion are no doubt more complicated than those of respiration or circulation, but the difference is one of degree, not of kind. And if the respiratory movements are so controlled and regulated by the automatic centre which governs them, that they adapt themselves perfectly to the varying requirements of the organism, there is no reason why we should hesitate in attributing to the centres which preside over locomotion powers which are somewhat more extended.

But perhaps the question has already presented itself to your minds. What does all this come to? Admitting that we are able to prove (1) that in the animal body, Product is always proportional to Process, and (2) as I have endeavoured to show you in the second part of my discourse, that Descartes' dream of animal automatism has been realized, what have we learnt thereby? Is it true that the work of the last generation is worth more than that of preceding ones?

JURASSIC BIRDS AND THEIR ALLIES.*

By Professor O. C. Marsh.

About twenty years ago, two fossil animals of great interest were found in the lithographic slates of Bavaria. One was the skeleton of *Archæopteryx*, now in the British Museum, and the other was the *Compsognathus* preserved in the Royal Museum at Munich. A single feather, to which the name *Archæopteryx* was first applied by Von Meyer, had previously been discovered at the same locality. More recently, another skeleton has been brought to light in the same beds, and is now in the Museum of Berlin. These three specimens of *Archæopteryx* are the only remains of this genus known, while of *Compsognathus* the original skeleton is, up to the present time, the only representative.

When these two animals were first discovered, they were both considered to be reptiles by Wagner, who described *Compsognathus*, and this view has been held by various authors down to the present time. The best authorities, however, now agree with Owen that *Archæopteryx* is a bird, and that *Compsognathus*, as Gegenbaur and Huxley have shown, is a Dinosaurian reptile.

Having been engaged for several years in the investigation of American Mesozoic birds, it became important for me to study the European forms, and I have recently examined with some care the three known specimens of *Archæopteryx*. I have also studied in the Continental Museums various fossil reptiles, including *Compsognathus*, which promised to throw light on the early forms of birds.

During my investigation of *Archæopteryx*, I observed several characters of importance not previously determined, and I have thought it might be appropriate to present them here. The more important of these characters are as follows:—

1. The presence of true teeth, in position, in the skull.
2. Vertebræ biconcave.
3. A well-ossified, broad sternum.
4. Three digits only in the manus, all with claws.
5. Pelvic bones separate.
6. The distal end of fibula in front of tibia.
7. Metatarsals separate, or imperfectly united.

These characters, taken in connexion with the free metacarpals, and long tail, previously described, show clearly that we have in *Archæopteryx* a most remarkable form, which, if a bird, as I believe, is certainly the most reptilian of birds.

If now we examine these various characters in detail, their importance will be apparent.

The teeth actually in position in the skull appear to be

*Read before Section D., British Association for the Advancement of Science, at York, Sept. 2, 1881.

in the premaxillary, as they are below or in front of the nasal aperture. The form of the teeth, both crown and root, is very similar to the teeth of *Hesperornis*. The fact that some teeth are scattered about near the jaw would suggest that they were implanted in a groove. No teeth are known from the lower jaw, but they were probably present.

The presacral vertebræ are all, or nearly all, biconcave, resembling those of *Ichthyornis* in general form, but without the large lateral foramina. There appear to be twenty-one presacral vertebræ, and the same, or nearly the same, number of caudals. The sacral vertebræ are fewer in number than in any known bird, those united together not exceeding five, and probably less.

The scapular arch strongly resembles that of modern birds. The articulation of the scapula and coracoid, and the latter with the sternum is characteristic; and the furculum is distinctly avian. The sternum is a single broad plate, well ossified. It probably supported a keel, but this is not exposed in the known specimens.

In the wing itself the main interest centres in the manus and its free metacarpals. In form and position these three bones are just what may be seen in some young birds of to-day. This is an important point, as it has been claimed that the hand of *Archæopteryx* is not at all avian, but reptilian. The bones of the reptile are indeed there, but they have already received the stamp of the bird.

One of the most interesting points determined during my investigation of *Archæopteryx* was the separate condition of the pelvic bones. In all other known adult birds, recent and extinct, the three pelvic elements, ilium, ischium and pubis, are firmly anchylosed. In young birds these bones are separate, and in all known Dinosaurian reptiles they are also distinct. This point may perhaps be made clearer by referring to the two diagrams before you, which I owe to the kindness of my friend Dr. Woodward, of the Britism Museum, who also gave me excellent facilities for examining the *Archæopteryx* under his care. In the first diagram we have represented the pelvis of an American Jurassic Dinosaur allied to *Iguanodon*, and here the pelvic bones are distinct. The second diagram is an enlarged view of the pelvis of the *Archæopteryx* in the British Museum, and here too the ilium is seen separate from the ischium and pubis.

In birds the fibula is usually incomplete below, but it may be coossified with the side of the tibia. In the typical Dinosaurs, *Iguanodon*, for example, the fibula at its distal end stands in front of the tibia, and this is exactly its position in *Archæopteryx*, an interesting point not before seen in birds.

The metatarsal bones of *Archæopteryx* show, on the outer face at least, deep grooves between the three elements, which imply that the latter are distinct, or unite late together. The free metacarpal and separate pelvic bones would also suggest distinct metatarsals, although they naturally would be placed closely together, so as to appear connate.

Among other points of interest in *Archæopteryx* may be mentioned the brain-cast, which shows that the brain, although comparatively small, was like that of a bird, and not that of a Dinosaurian reptile. It resembles in form the brain-cast of *Laopteryx*, an American Jurassic bird, which I have recently described. The brain of both these birds appears to have been of a somewhat higher grade than that of *Hesperornis*, but this may have been due to the fact that the latter was an aquatic form, while the Jurassic species were land birds.

As the *Dinosauria* are now generally considered the nearest allies to Birds, it was interesting to find in those investigated many points of resemblance to the latter class. *Compsognathus*, for example, shows in its extremities a striking similarity to *Archæopteryx*. The three clawed digits of the manus correspond closely with those of that genus; although the bones are of different proportions. The hind feet also have essentially the same structure in both. The vertebræ, however, and the pelvic bones of *Compsognathus* differ materially from those of *Archæopteryx*, and the two forms are in reality widely separated. While examining the *Compsognathus* skeleton, I detected in the abdominal cavity the remains of a small reptile which had not been previously observed. The size and position of this inclosed skeleton would imply that it was a fœtus; but it may possibly have been the young of the same species, or an allied form, that had been swallowed. No similar instance is known among the Dinosaurs.

A point of resemblance of some importance between Birds and Dinosaurs is the clavicle. All birds have these bones, but they have been considered wanting in Dinosaurs. Two specimens of *Iguanodon*, in the British Museum, however, show that these elements of the pectoral arch were present in that genus, and in a diagram before you one of these bones is represented. Some other *Dinosauria* possess clavicles, but in several families of this subclass, as I regard it, they appear to be wanting.

The nearest approach to Birds now known would seem to be in the very small Dinosaurs from the American Jurassic. In some of these the separate bones of the skeleton cannot be distinguished with certainty from those of Jurassic Birds, if the skull is wanting, and even in this part the resemblance is striking. Some of these diminutive Dinosaurs were perhaps arboreal in habit, and the difference between them and the Birds that lived with them may have been at first mainly one of feathers, as I have shown in my Memoir on the *Odontornithes*, published during the past year.

It is an interesting fact that all the Jurassic birds known, both from Europe and America, are land birds, while all from the Cretaceous are aquatic forms. The four oldest known birds, moreover, differ more widely from each other than do any two recent birds. These facts show that we may hope for most important discoveries in the future, especially from the Triassic, which has as yet furnished no authentic trace of birds. For the primitive forms of this class we must evidently look to the Palæozoic.

THE LIMITED BIOLOGICAL IMPORTANCE OF SYNTHETIC ACHIEVEMENTS IN ORGANIC CHEMISTRY.*

By Professor Albert B. Prescott.

The solicitude shown for half a century as to the biological import of chemical synthesis arises from a misapprehension of the scope of chemical action. From all we know of chemism, it must be accepted, (1) that all the matter of protoplasm and cell is carried strictly in a state of chemical combination, but (2) it cannot therefore be accepted that chemical composition supplies the essential conditions or impulses for organization or other vital functions. The synthesis of all the chemical compounds of the living body may or may not be attainable in the laboratory, but its success would give no whit of promise for the development of organization. Chemical action is distinct from cell organization as it is from heat, cohesion, etc., and its corelations with all these forces have to await demonstration by experiment. Cell growth appears to be a necessary factor in the simple splitting of sugar into alcohol and carbon dioxide, and it may or may not be an essential factor in the chemical synthesis of proteids or of cellulose.

A gentleman of Milan, Signor Lorin, deserves high credit, for the public spirit of philanthropy he has shown in offering 20,000 francs to the municipal authorities to maintain a mortuary and post mortem room wherein the bodies of all persons dying of unexplained causes shall be rigidly examined before they are cremated.

* Read before the A. A. A. S., Cincinnati, 1881.

THE TERRA DEL FUEGIANS AT THE GARDEN OF ACCLIMATION.

The whole world has heard of the savages, who are at present exhibited at the Zoological Garden of Acclimation of Bois de Boulogne; many have gone to see them, and have been well repaid, for they present an interesting spectacle to the observer. They are seen lying or squatting about the fire kindled under the trees of the large lawn, motionless for whole hours at a time, gazing with vacant eye at the astonished crowd which presses against the railings as though they contained remarkable animals. Do they think? We cannot tell this. Do they speak? Yes, they do speak, if we can call the gutteral sounds, the cluckings which at long intervals, they exchange with each other, a language. They remain there, indifferent, having no longer in operation the only cause which can agitate them, hunger; for they are fed. It is a curious sight, but also a sad one. A man at this stage of brutishness is not wholly an animal; but he is no longer a man. The Fuegians, for that is the name which Captain Weddel gave them in 1822, and which has been applied to them since that time, inhabit Terra del Fuego. When we read in the works of travelers the description of their country, we are no longer astonished at their profound degradation.

Terra del Fuego is a mountainous archipelago, separated from Patagonia by the straits of Magellan, and formed of enormous masses of steep rocks, which leave only the coast bordering upon the straits, upon which man can settle. In the parts where the rock is not absolutely bare, a thick and impenetrable forest of beeches covers the side of the mountain, and descends as far as the sea. No animal, with the exception of some foxes and birds, inhabits this country. The climate here is horrible. The mean temperature of summer, according to King and Darwin, is 10°C., and that of winter o.°6C. Mist is perpetual here, and tempests unceasing. Scarcely a day passes without the fall of rain, and even of snow. The habitable portion is only on the rocks of the shore. In the whole country, but a few acres of plain can be found.

For a long time these Fuegians have been known, and many descriptions of them have been given. Sebold, of Weert, who accompanied Simon, of Cord, made giants of them, eleven to twelve feet high. We see from the samples which we have under our eyes, that there is a certain exaggeration in that statement. We borrow from Orbigny the description which he gives of them; in our opinion, there is nothing to be changed in it, it is absolutely applicable to our savages.

Their head, says Orbigny, is tolerably large, their face is rounded; they have a short nose, a little broadened, open nostrils, small eyes, black and horizontal; a large mouth, thick lips, white teeth, well arranged; small ears, and the cheek bones a little prominent. They have but little beard, and this they pluck out. Their hair, like that of all the Americans, is black, long, and dull. Their body is massive, their chest large, and their bow-legs are relatively rather short. The women present the same characteristics as the men, and they will return with difficulty to the proportions exacted by European æsthetics. Their mean height is from 1.56 m., to 1.68 m.

Their language, as we have before stated, is guttural, and it has been compared by Cook to the utterance of a man who is gargling. This comparison expresses well the impression that is felt on hearing them.

The great naturalist, Charles Darwin, was able, during the many months which he passed in the country which they inhabit, to observe their habits, and he has given us a picture which, in order to be just, is not very attractive: it is from him that we borrow the particulars which follow. "Forced continually to move from one region to another, according as the resources of their settlement are exhausted, the Fuegians have no fixed abode. They construct a sort of hut by planting several branches in the ground and covering them with other branches intertwined on the side where the wind blows. Their dress consists of a piece of skin, which they carry over their shoulders, and which they pass from one shoulder to the other, according to the direction of the wind. It was necessary for ceremony to persuade the Fuegians at the Garden of Acclimation to put on a pair of drawers. Often they are completely nude. Their nourishment consists chiefly of shell-fish, and now and then of the rotten flesh of a seal or of a whale. At low tide, which may be in winter or in summer, in the night or in the day, they must get up to seek the shell-fish on the rocks; the women dive to obtain the eggs from the sea or remain patiently seated in their boats for several hours until they have caught several small fish with lines without hooks. If they happen to kill a seal, or if they happen to discover the half-rotten carcass of a whale, it is the signal for an immense feast. They then gorge themselves with the horrible food, and, to complete the feast, they eat several berries or several mushrooms which have no taste."

When the different tribes go to war they become cannibals. Besides, when in winter they are strongly pressed by hunger, they eat the old women before they do the dogs, because, they say, the latter capture otters, and the old women cannot. In this regard, it is to be regretted that they did not bring some of their dogs with them. The only domestic animals of these savages ought certainly to present a precious subject of observation.

Do these savages believe in another life, have they any rudimentary religion whatever? We are not able to pronounce on this, for it is impossible to draw any explanation from the savages themselves; they are incapable of comprehending an alternative, and we can never surely know if we understand them ourselves. All that we can say is, that each tribe or family possesses a magician whose functions have not yet been exactly defined by travelers, and that the Fuegians generally bury their dead.

It has been pretended that the family tie does not exist among them. Yet, we see in the account of Darwin that York Minster, one of the Fuegians brought back by Captain Fitz-Roy to his country, took as his wife the young girl who had accompanied him to Europe, and that the other returned Fuegian also had his wife when the expedition returned to the place inhabited by the tribe with which he had been left. Is not this a proof of the existence of a family relation, rudimentary if you wish, yet a real home among these savages.

As regards property, it is an unknown thing among them. Apart from arms and utensils, no Fuegian possesses anything of his own. If he kills a seal, it is shared among all the members of the tribe. If a present is made to one of them, he breaks it and divides the pieces. It is communism in all its beauty.

The different tribes have neither government nor chief. Each of them is, however, surrounded by other hostile tribes speaking different dialects. They are separated, the one from the other, by a neutral territory which remains absolutely deserted. The perpetual wars which these tribes have, seem to have for a cause the difficulty of obtaining food. The land is so steep that they cannot change their abode except by water; and necessity has forced them to become navigators and to build boats. Those who inhabit the shores of the Straits of Magellan pass, from time to time, into Patagonia to chase the guanacos in order to renew their clothes and their provisions. But even there they encounter enemies. The Patagonians, from whom they are distinguished by race and language, as well as by habits, pursue them with ardor, and seek to reduce them to slavery. A Fuegian slave is very highly estimated by the Patagonians, who value him among themselves, according to the quality, up to $200.

When we consider the few resources which the archi

Inhabitants of Terra del Fuego at the Garden of Acclimation in Paris (After a Photograph by Pierre Petit).

pelago of Terra del Fuego offers for the existence of man, even compared to the neighboring regions on the American continent, we ask what cause has persuaded the Fuegians to establish themselves there. To-day it is beyond doubt that these people are not negroes, as Bory Saint-Vincent believed, but that they belong to an Ando-Peruvian race which inhabits the Andes and a part of the pampas of Chili. They probably occupied, in olden times, the northern banks of the straits of Magellan, and are but a remnant of the Aucas and the Araucanos of Chili. Attacked by the Patagonians of the pampian race, not as strong and more poorly armed than their adversaries, they were obliged, at a time more or less remote, to yield the place to their redoubtable enemies and to take refuge in the inhospitable regions on the other side of the strait, where the Patagonians, detestable navigators, left them in quiet.

Then little by little have acted the forces of adaptation, which all-powerful habit, in returning their hereditary effects, have adapted the Fuegians to the climate and productions of their miserable country.

Their industry is modified in the same way, and today it is reduced to the construction of miserable boats, and to the manufacture of several weapons and utensils necessary to their sad existence. The boat built of a mass of shapeless pieces of wood, covered with canvas in the shape of the skins which they customarily employ, the boat which can be seen on the basin in the neighborhood of their enclosure, makes us shudder when we think that these savages venture in this frail machine on the agitated waters which wash their country. In regard to the collection of arms and utensils which can be seen in a neighboring shed, it indicates a certain ingenuity, but shows well to what a miserable condition these poor creatures are reduced.

These Fuegians, eleven in number, four men, four women, and three children, have been brought to Europe by M. Waalen, established for many years at Punta-Arenas, capital of Patagonia.

M. Waalen, who goes to fish for seals in the waters of Terra del Fuego, finds himself in connection with these savages. He was able, by gorging them with food, by treating them with prudence, for they are not always tractable and would be able to cause great obstructions, to induce them to remain on his ship, from which they were transshipped on a Hamburg steamer which makes the passage between Valparaiso and Europe. It was while the ship touched at Havre that M. Geoffroy Saint Hilaine, informed by a despatch, saw them and bronght them here. M. Waalen deposited in the hands of the Chilian Governor of Punta-Arenas, a sum of 12 to 15,000 francs, as security, binding himself to return these savages to their country after they had made a tour through the principal capitals of Europe.

What impression will they carry back of their sojourn among civilized people? If we are to judge of this by the Fuegians that Captain Fitz-Roy returned after a sojourn of three years in Europe, the impression will be a very fleeting one. These natives, three in number, two men, York Minster and Jemmy Button, and a young girl, Fuégia, seemed almost entirely civilized. Captain Fitz-Roy landed them in the middle of their tribes, furnished them with implements and tools of all sorts, built them a house, cleared up a corner of ground, and left them in the company of a missionary. When he returned, several months after, he found no trace of their installment, and had to take on board the poor missionary, who ran the greatest danger. Of his three pioneers, two, York Minster and Fuégia who became his wife, parted in plundering their comrade, and the latter, who had taken a wife in his tribe, became a filthy and disgusting savage, delighted with his condition, scarcely knowing how to speak English, and who showed with pride to the officers of the expedition the implements of bone and of flint which he had manufactured.

It seems, after this experience, that it is impossible to draw these savages from their debasement, and yet they have an intellectual capacity, latent, it is true, which appears superior to that of Australians. They learn languages with remarkable facility, and have a spirit of imitation carried to extremes, which ought to be utilized in order to teach them things well. The future will tell us if those who are at present in the Garden of Acclimation, will derive any profit from their sojourn among us. Our opinion is that they will be delighted at finding themselves in their own homes, and the remembrance of all that they will have seen will remain in their minds as a dream which will not perhaps be wholly agreeable.—(*Translated from La Nature.*)

ON A NEW SYSTEM OF BLOWPIPE ANALYSIS.*

By Lieut.-Colonel W. A. Ross (late R. A.)

(1) THE USE OF ALUMINIUM PLATE FOR VOLATILIZING SUBSTANCES.

Volatile metals and sulphur compounds, &c., are, in the old system, treated before the blowpipe, as is well known, upon the support of a parallelopiped of charcoal held horizontally in the direction of the blast from the blowpipe, the disadvantages of which are: (*a*) that *black* sublimates as those now known to be obtainable from arsenic, antimony, lead, &c., are undistinguishable on the black charcoal. (*b*) The greater part of the sublimate from most volatile metals is blown away by the blast—a serious objection when, as is often the case, there is only a trifling proportion of such metals present in a mineral or compound. (*c*) When the charcoal becomes incandescent, the most interesting portion of the sublimate (that next the assay) is often thus resublimed and lost. (*d*) The white charcoal ash is so mixed up with sublimates as often to conceal them, and, in cases of minute quantities, to mislead the operator into supposing there is a sublimate at all. (*e*) In the treatment of a compound containing two or more volatile metals, sulphides, or oxides, the sublimates obtained therefrom are mechanically, and perhaps sometimes chemically, combined, and then cannot be separated, so as to be distinguished from each other, by means of the blowpipe, or in any other way at the time, on the spot. (*f*) It is impossible to obtain a blowpipe sublimate from charcoal free from the silica, &c., of the ash, by scraping it off for supplementary examination. (*g*) Most charcoals, after blowpipe treatment for any length of time, split up into cracks and deep fissures, into which the sublimate or the assay falls and is lost.

Here are several objections to the use of charcoal as a blowpipe support; most of them serious, some fatal to a thorough pyrological examination of volatile substances; and yet it has obtained ever since Von Swab invented the chemical employment of the blowpipe in 1738 (in which year he thus treated an ore of zinc at Delarne in Sweden), and is still used at Freiberg.

In 1869 Napoleon III had offered, or I understood him to have offered, a premium of £1000 to any one who could discover an efficient solder for aluminium, and being then on sick-leave in India, I thought of employing my leisure in attempting this discovery.†

After investigation, I imagined (from burning my fingers so often), that the reason an aluminium solder could not be made, was the enormous heat-conducting powers of the metal, which transferred the heat from a blowpipe-flame so quickly away over the entire substance of a fragment of given bulk, that no one part of it could

*British Association, York, 1881.

† In reply to a question, Col. Ross answered that he had not discovered a new solder, but that on one occasion last year (1880) he actually did succeed in soldering two small pieces of aluminium together, and that he has a description of the process in his notes.

be raised to the fusing-point, so that, although small portions of almost every other metal or alloy could be readily fused upon it, even the most fusible, such as antimony, bismuth, &c., could not be made to combine with the aluminium.

As I had then studied blowpipe analysis on the Freiberg system for ten years, it was obvious to me that, although I had no chance of otaining the £1000, the facts thus ascertained might be utilized so as to make aluminnum plate or foil remedy in part, at all events, the disadvantages above described of charcoal as a blowpipe support.

I found that arsenic, antimony, bismuth, &c., the fusion of the smallest particle of which upon platinum is so fatal to it, could be treated without the slightest danger before the blowpipe upon aluminium, which metal also, probably from the reason above given, withstood heat concentrated upon any point, in direct proportion to the bulk of the fragment used as a support.

I found that some volatile metals, as *e. g.*, antimony, would not yield a sublimate when treated before the blowpipe upon the bare aluminium plate, but readily did so when a small slip or lozenge of charcoal was placed between the assay and the aluminium. Here, then, was a rapid and effectual means of *separating* the pyroxides or sublimates obtainable from a compound, for instance, of antimony and arsenic; the latter subliming readily upon the bare aluminium plate; the former only after treatment upon a charcoal slip.

The horizontal charcoal support was, of course, changed into a perpendicular one, in direct opposition to the blast from the blowpipe, so as to catch all sublimates of every kind; the grey-colored, shining aluminium betrayed at once the faintest sublimates, whether black or white; these, again, could be readily treated by the oxidizing or redueing flame of the blowpipe on the aluminium, where they thus afforded, in most cases, new and characteristic reactions; the perpendicular aluminium could be graduated by a scale showing the different specific gravities of sublimates by their mean ascension on the plate, unacted upon by the blast as in the case of charcoal; and finally, any portion of a sublimate could be easily and cleanly scraped off with a penknife, so as to be afterwards examined in any way desired.

Another advantage I found, referrable, I presume, to the same cause (of superior heat-conduction in the support) is that the alkaline carbonates, so often used in blowpipe analysis, as in the detection of manganese, for instance, assume, when treated before the blowpipe on aluminium, a globular shape, and that the resulting bead or ball of sodium or potassium carbonate, can be readily picked, when cool, off the plate with forceps, instead of lying in a kind of pool and sticking to the metal as they do in the case of platinum foil.

To other uses of aluminium plates, as in flattening blowpipe beads and their contents for microscopical purposes, I have not time to allude.

(2) A NEW AIR-RESERVOIR MOUTH BLOWPIPE (CALLED BY ME A "PYROGENE.")

A member of the Royal Geological Survey of England told me in Jermyn street, that he believed many geologists and mineralogists were deterred from using this important little instrument by the trouble if not difficulty of blowing, and for a long time I tried to discover some means of obviating this difficulty in vain. At last, one day, in the Zoological Gardens of London, looking reflectively at the antics of some anthropoid types of our ancestors there, I could not help feeling a kind of regret that the process of "Natural Selection" should have eventually deprived my race of the pouch under the jaw, no doubt at one time possessed by them, which would have served so admirably as an air-reservoir in using the blowpipe, and it suddenly struck me that I could partially remedy the defects of specific development in this matter, by applying an elastic air-reservoir of indiarubber to the ordinary mouth-blowpipe.

Here is the result. I have made it of a simple tube-like form, instead of the usual tapering one, as seen in Black's blowpipe, because I had to adapt it to be packed in a cigar-case like this, the only way of effecting which was to have it in a telescopic arrangement, opening and shutting thus: and this arrangement had another advantage, that, namely, of adapting the length of the instrument to the differing optical focus of differing vision.

For the jet I took Wollaston's ingenious idea of passing the stem of the blowpipe through the arm of the jet; only instead of doing that, it suited my purpose better to pass the jet through the stem of the blowpipe thus. Of course, in either case, the inserted tube must fit air-tight —an easy matter to effect. Over the throat of the mouthpiece is tied a piece of oiled silk, which acts as a valve, preventing the return of the breath into the cheeks. In this manner all difficulty in blowing is entirely removed, and even a child can use this blowpipe, because all he has to do is to blow through the valve till the air-bag is filled; then he can stop until the pressure of the blast begins to slacken, when a few more breathings will refill the bag. The blast pressure from the bag may also be increased by the operator placing it between himself and the table, and gently pressing the bag with his body, which he can easily do while using this apparatus.

I have only to add that, as you observe, the jet and air-bag fit for packing into the tube of the blowpipe itself, for which purpose there is no necessity, as in the one I have here, to make the end screw off, as all one has to do is to draw the telescopic arrangement out altogether, and, slipping in the jet and bag, to shut it up again; this, of course, would make the article cheaper. Griffin makes them (with the screw end) for, I believe, half a crown, but, of course, any ordinary mechanic could make such a blowpipe for himself for a few pence.

(3) THE PYROLOGICAL CANDLE.

I begin a brief description of this fuel with the remark that it is practically impossible for the traveller to use gas of any kind—not even petroleum gas—as fuel, on account of the difficulties of carriage. The same remark applies, but in another way, to oil of any description. A bottle of this is no doubt, easily carried, but is very apt to leak at the cork, and so to spoil any or most articles near it in a box.

Considerations of this kind led me, in 1871, to look to the modern composite candle as a substitute for the Berzelius blowpipe lamp, supplied with Plattner's Freiberg apparatus, which I had used for twelve years. The candles then used for blow-pipe operations were, indeed, in no respect different from those used for illuminating purposes. How Von Engeström, Bergmann, and the more modern pyrologists who are said to still use common candles for blowpipe work, contrived to do anything useful with them, I fail to understand. With even a small wick in the *centre* of the candle, which, of course, must be turned on one side to prevent it from stopping the blast, the heat radiation from the blowpipe-pyrocone melts the tallow or wax from that side more rapidly than the remainder of the circumference melts, so that a deep channel is soon formed, down which the fluid fuel runs, leaving the wick "high and dry." The consequence is that the pyrocone becomes "thready," from the burning of dry carbonaceous particles eliminated from the wick, and when it is cut down a mass of unconsumed tallow almost covers it at one side.

I therefore adopted the plan of having the candle made with a thick, and even double, wick, placed at one side instead of in the middle of the fuel, and in order to supply more of the latter, I had my candles made a prismatic instead of a round shape. I placed a thick collar of a good conducting metal, such as zinc, round the

edge of the candle, just under the wick, in order to conduct away and diffuse through itself the vibrations of heat. At first I had a series of these metallic collars, and proposed to remove them as the candle burned down; but I afterwards found that one or two good thick zinc collars would be sufficient.

Here is a candle from my cigar blow-pipe case which I am at present using, and another unused one, as made for me by Price & Co. of Battersea.

(4) CANDLE SCISSORS.

In Plattner's apparatus scissors are supplied for cutting the lamp-wick, which of course can also be used for other purposes, *and also* a pair of pliers for squeezing the wick together, and pressing it in any direction; these latter cannot be used, from the dirty state into which they get, for anything else. I use these two articles combined into one—*i. e.*, a pair of ordinary scissors with knobs at the end. This also goes into my cigar blowpipe-case.

(5) ORDINARY WATCHMAKER'S PLIERS,

with a piece of wire-strapping round them, to enable them to act as holders of platinum wire supports, and they also act as the best cleaners of the wire by drawing the latter from between the pressed flat sides.

(6) TWO AGATE SLABS FOR GRINDING POWDERS.

I have here got instead, a small Freiberg agate mortar, with a pestle made from an agate pen, as I had no slabs small enough to pack away in this cigar-case.

(7) REAGENTS. BORIC ACID.

It has always seemed to me as though blow-pipe workers, or, as I call them, "Pyrologists," could no more profess to begin analytical operations by using a *salt* as reagent, than the analytical chemist could say he intended to begin his solution-work by using sodium nitrate instead of nitric acid. By employing boric acid instead of borax, therefore, in 1869, I at once obtained a series of new, very pretty, and important reactions, especially in the case of the alkaline earths, which formerly used to be the weakest part of blowpipe analysis; now, they are one of the easiest. Space and time do not allow me to describe these reactions here; and, unfortunately, I have brought no boric acid with me here in order to illustrate them; but here is a little German-silver cigar-light box in which the acid is kept, as it does not thus deteriorate. This also goes into the cigar-case.

Phosphoric acid is another of my new reagents (when I say "new," I mean that they are now 12 years old, but new in the sense that they have not been as yet generally adopted.) I use it instead of the old reagent "microcosmic salt." It affords, with several oxides before the blowpipe, new and interesting colors, as in the case of cobalt oxide, which imparts to it a very fine and pure violet instead of the ordinary blue. Of course, when a sufficient quantity of soda to form metaphosphate of sodium, or microcosmic salt after the ammonia has been driven off, has been added, the bead becomes blue, and this fact enables it to be used as an alkalimeter. It is the only reagent which requires to be kept in a stoppered bottle; and is such a powerful acid before the blowpipe that gold leaf is rapidly dissolved in it, yielding a brilliant purple bead. It affords, with iron oxide, a bead the color of watery blood. This ends the list of things packed in the cigar-case.

(8) A COMPASS IN WHICH THE NEEDLE POINTS E. AND W.

This is made by bending an ordinary magnetized needle in the centre until the points are opposite, like a lady's hairpin. It is, in fact, an ordinary horseshoe-magnet suspended, and such a magnet suspended swings E. and W. for a very obvious reason. It might prove useful in Arctic voyages, as such a needle would probably possess little or no "dip." If you bend an ordinarily magnetized needle at a right or any other angle, and suspend it from or on its centre of gravity, a line bisecting the angle will point E. and W., and it was such a needle I first made in order to find a very delicate test for traces of iron in ores. The more open or obtuse the angle, the more delicate this test is. I call it the "Equatorial Needle.' With a right-angled equatorial needle you can detect the mere trace of iron in the ore *Molybdenite*.

(9) AN ALLOY-BUTTON OF GOLD AND SILVER IN WHICH THESE METALS HAVE BEEN PARTLY SEPARATED BY THE BLOWPIPE ALONE.

Many years ago I found that, if you heat an alloy of two or more metals very gently with the blowpipe, so as not to promote fusion, in which case the ball spins round, and all the component metals are mixed again—that one nearly pure metal invariably leaves the others, and approaches the source of heat. This is a case of gold and silver alloy, in which the silver has approached the source of heat, but the process can be admirably illustrated in the case of a common bronze pin, in which the tin approaches the source of heat, while the copper remains in the background. Such a process might obviously be found useful in metallurgy on the large scale.

ASTRONOMY.

To the Editor of "SCIENCE."

On the early morning of June 30, 1881, the definition was very good. On no other occasion was *Comet B*, 1881, seen so clearly. As it appeared in our 8¼-inch refractor, it presented some peculiarities which I have not noticed in any published drawings, and therefore mail you the enclosed.

The prominent features were an unsymmetrical pear shaped coma surrounding the nucleus, two streams on either side, and one directly opposite the tail, which blended with the envelope. Around the whole was a very faint secondary envelope.

Very respectfully,

ISAAC SHARPLESS.

HAVERFORD COLLEGE OBSERVATORY, *September* 1, 1881.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of SCIENCE.

The "Ononid" meteors were watched this morning from 12.20 to 3.05 by four observers. The shower seemed quite abundant, 190 meteors being mapped during the time of observation. About one-half of these undoubtedly belonged to a common system. The radiant point as deduced from these, and which, considering their number cannot be greatly in error, was R. A.—86°, Dec. + 16° which brings it just outside the limits of the constellation Onon. No stationary meteors were observed and but very few with short paths near the radiant point. This may be due to the fact that they were so faint (mostly about equal in brightness to a fourth magnitude star) that the short paths were not sufficiently conspicuous to call our attention to them. An auroral light was visible in the north and east during the early part of the watch. Chambers gives 85°, + 16 as the radiant point, and adds that Tupman makes it 90°, + 11.

Respectfully,

ISAAC SHARPLESS.

HAVERFORD COLLEGE OBSERVATORY, PA., 10*mo*, 19*th*, 1881.

DR. H. RAYMOND ROGERS AND HIS CRITICS.

To the Editor of "SCIENCE."

Prof. Merriam, in your journal, page 495, writes as follows: "I do not like to see so great an authority as Faraday misunderstood, as he evidently is by your correspondent on page 459 of your journal, and that, too, in a way which he took particular care to caution against—as to the law of gravitating action. That it acts inversely as the square of the distance he fully believed and admitted; or, to use his own words, 'I know it is so.'"

The quotation objected to was made verbatim from Faraday's writings, and the sentiments contained therein were frequently expressed by him, and with emphasis. In the work entitled "Correlation and Conservation of Force," page 363, is an essay by Faraday entitled "The Conservation of Force," in which we read the following, viz.: "I believe I represent the received idea of the gravitating force aright in saying that it is a simple attractive force exerted between any two or all the particles or masses of matter, at every sensible distance, but with a strength varying inversely as the square of the distance. The usual idea of the force implies *direct* action at a distance: and such a view appears to present little difficulty except to Newton, and a few, including myself, who in that respect, may be of like mind with him. This idea of gravity appears to me to ignore entirely the principle of the conservation of force; and by the terms of its definition, if taken in an absolute sense, '*varying* inversely as the square of the distance,' to be in direct opposition to it." Again, in the same essay, page 366, "the assumption which we make for the time with regard to the nature of a power (as gravity, heat, etc.,) and the form of words in which we express it, that is, its definition, should be consistent with the fundamental principles of force generally. The conservation of force is a fundamental principle; hence the assumption with regard to a particular form of force ought to imply what becomes of the force when its action is *increased* or *diminished*, or its *direction changed;* or else the assumption should admit that it is deficient on that point, being only half competent to represent the force; and, in any case, should not be opposed to the principle of conservation. The usual definition of gravity as ***an attractive force between the particles of matter varying inversely as the square of the distance***, whilst it stands as a full ***definition*** of the power, is inconsistent with the principle of the conservation of force."

Faraday is here laboring to show the incompetency of that definition *alone*. He thinks the natural philosopher ought to look for effects and conditions as yet unknown; and so virtually calls aloud for some one to fill up what to him appears a serious deficiency. He called the old definition only a *half*-assumption, and felt the necessity of some enlargement of it, that it might stand secure. He says: "the half-assumption is, in my view of the matter, more dogmatic and irrational than the whole, because it leaves it to be understood that power can be created and destroyed almost at pleasure."

Faraday called for, what we believe, the electric theory amply supplies. Not only so, but he also indicated this very source of supply. For example, a "grain of water" having a given force of gravity has also "electric relations equivalent to a very powerful flash of lightning." He says, "It may, therefore, be supposed that a very large apparent amount of the force causing the phenomena of gravitation, may be the equivalent of a very small change in some unknown condition of the bodies, whose attraction is varying by change of distance. For my own part, many considerations urge my mind toward the idea of a cause of gravity, which is not resident in the particles of matter merely, but constantly in them, and all space."

We have been led to think that it was not impossible to *find* such "cause of gravity, not resident in the particles of matter merely," but which by means of a "very small change in some [formerly] unknown condition of the bodies," shall bring the whole subject of gravitation out from the shadowy realms of darkness into abiding sunlight.

In brief, Faraday insists that the totality of the force of gravity is not expressed by the definition that "gravity acts directly as the mass and inversely as the square of the distance." Indeed, he says as pithily as when he uttered your correspondent's quotation, "I know it is so." "That the *totality* of a force can be employed according to that law *I do not believe!*"

It might, by the way, be of interest to learn a little more definitely as regards what it was that Faraday knew was so. The following are his words: "That the result of one exercise of a power may be inversely as the square of the distance I believe and admit; and I know it is so in case of gravity." The same sentence, however, continues: "but that the *totality* of a force can be employed according to that law I do not believe either in relation to gravitation or electricity or magnetism, etc."

It may be asked what can be correctly known of the action of electricity or magnetism where the item *polarity* is left out? "What I object to," says Faraday, "is the pretence of knowledge which the definition sets np when it assumes to describe, not the partial effects of the force, but the nature of the force as a whole."

Satisfied with the old definition as your correspondent may be, Faraday looked for a "missing link." We may say that he pointed it out in saying:—"when we remember that the earth itself is a magnet, pervaded in every part by this mighty power, universal and strong as gravity itself, we cannot doubt that it is exerting an appointed and essential influence over every particle of matter, and in every place where it is present. What its great purpose is seems to be looming up in the distance before us:—the clouds which obscure our mental sight are daily thinning, and I cannot doubt that a glorious discovery in natural knowledge and in the wisdom and power of God in the creation is awaiting our age."

I would conclude this part of my reply to your correspondent, with the recommendation that he study Faraday, for "I do not like to see so great an authority as Faraday misunderstood."

Again, as regards the earth's return from aphelion to perihelion:—

It is admitted that my reply (p. 459) to Mr. Hendricks

was left open to objection. This may be accounted for by the fact that there was shown to me his article *minus two paragraphs*—the last paragraph on page 458 and first on 459. Therein *inertia* alone was represented as *bringing back* the planet from aphelion to perihelion. That the planet, traveling its orbit from perihelion to aphelion, as it were *diagonally against* the central attraction of the sun, would find its velocity and momentum diminished sufficiently to be made to return, I do not doubt; but that on the *second* round, it would reach the farthest limits of its first round, I do not think there is any reason to believe. The tendency would be to bring the orbit into a perfect circle very speedily. In the polarity, which is a factor of magnetism, we find a needed *regulative* agency. Do we say that this agent is too insignificant? Nevertheless may it not be, in the words of Faraday, the "*very small change* in some unknown condition of the bodies" involved in the operation, which is all-sufficient for what is required of it? We recognize the force of Faraday's objection to the popular definition of gravity, viz.: that *alone* it is incompetent, and contradicts the law of conservation—except as we add to it *something more*. That something more we fully believe to be that electrical or magnetic constituent which Faraday says "exerts an appointed and essential influence over every particle of matter."

QUERY.—Is it wise or philosophical to recognize a cosmical force of incalculable energy, and yet in our theory of the cosmas make no practical account of it whatsoever?

H. RAYMOND ROGERS.

DUNKIRK, N. Y.

The death of Mr. Charles A. Spencer, of Geneva, N. Y., has caused universal regret, and in many respects it may be considered a national loss, for as a representative of America's skilled opticians his position was unique. As a pioneer he was the first to manufacture Microscope objectives in the United States, and at once developed a skill in the manufacture of these minute and delicate glasses, which he maintained to the last. Spencer was no copyist, his inventive genius and thorough knowledge of the optical principles involved in the making of objectives enabled him to keep in the van of all those who devoted themselves to the same art.

The greatest triumph of Spencer was in the enlargement of the angle of aperture of his objectives, in which respect he was always in advance of the best European makers, but he will always be remembered as a conscientious worker, who never permitted an objective to leave his hands which was not worthy of the maker.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 22, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 16	30.116	30.218	10 p. m.	30.000	2 a. m.	68.0	63.0	76	2 p. m.	67	2 p. m.	62	12 p. m.	56	12 p. m.	138.
Monday, 17	30.104	30.212	0 a. m.	29.918	12 p. m.	63.0	60.3	66	12 p. m.	65	12 p. m.	59	4 a. m.	54	4 a. m.	105.
Tuesday, 18	29.760	29.918	0 a. m.	29.678	2 p. m.	67.0	62.6	78	2 p. m.	68	2 p. m.	51	12 p. m.	48	12 p. m.	131.
Wednesday, 19	30.092	30.118	11 a. m.	29.908	0 a. m.	50.0	45.6	55	2 p. m.	45	2 p. m.	45	6 a. m.	44	7 a. m.	120.
Thursday, 20	30.031	30.098	0 a. m.	30.000	2 p. m.	51.3	49.3	56	3 p. m.	52	3 p. m.	47	1 a. m.	45	1 a. m.	125.
Friday, 21	30.179	30.200	9 a. m.	30.068	0 a. m.	53.7	50.3	61	3 p. m.	55	5 p. m.	44	7 a. m.	44	7 a. m.	128.
Saturday, 22	30.142	30.208	9 a. m.	30.100	5 p. m.	57.0	52.7	67	4 p. m.	59	5 p. m.	46	7 a. m.	46	7 a. m.	122.

Mean for the week .... 30.060 inches.
Maximum for the week at 10 p. m., Oct. 16th .... 30.218 "
Minimum " at 2 p. m., Oct. 18th .... 29.678 "
Range .... .540 "

	Dry.		Wet.
Mean for the week	58.5 degrees		54.8 degrees.
Maximum for the week,	at 2 pm. 18th 78. "	at 2 pm 18th,	68. "
Minimum "	" 7 am. 21st 44. "	at 7 am 21st,	44. "
Range "	" 34. "		24. "

OCTOBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of Rain and Snow in Inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 16	w. s. w.	n. n. w.	n. e.	178	2½	1.50 am	.542	.542	.451	94	60	73	0	7 cir. cu.	3 cu.	-----	----	-----	--	3
Monday, 17	e. n. e.	e. n. e.	s.	138	1¼	6.00 am	.412	.497	.562	77	83	94	8 cir. cu.	10	10	-----	-----	-----	--	0
Tuesday, 18	s. e.	w. n. w.	n. n. w.	240	12½	11.00 pm	.612	.550	.404	89	57	93	8 cu.	9 cu.	0	-----	-----	-----	--	6
Wednesday, 19	n. n. w.	n. e.	e. n. e.	234	6¼	5.50 am	.262	.243	.247	84	56	71	0	6 cir. cu.	8 cu.	-----	-----	-----	--	0
Thursday, 20	n. e.	e. n. e.	s. s. e.	111	1½	2.30 pm	.284	.349	.348	85	80	93	9 cu.	5 cir. cu.	0	-----	-----	-----	--	0
Friday, 21	n. w.	n. e.	s. w.	65	¼	1.15 pm	.288	.297	.391	100	55	87	0	2 cir. cu.	0	-----	-----	-----	--	2
Saturday, 22	w. n. w.	s. w.	s. w.	134	2¼	3.20 pm	.311	.330	.396	100	53	76	0	0	0	-----	-----	-----	--	0

Distance traveled during the week .... 1,100 miles.
Maximum force .... 12¼ lbs.

Total amount of water for the week .... .00 inch.
Duration of rain .... 0 hours, 00 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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SATURDAY, NOVEMBER 5, 1881.

SCIENCE AND MEDICINE.

A few words on the relation of the natural sciences to medicine, as one of the greatest aids for the achievement of success, should be welcome reading to all members of the medical profession whose aspirations are above the dead level of mediocrity.

The physician may at first sight desire to stifle all discussion on this point by saying, that the requirements of study involved in acquiring a knowledge of medical practice *per se* occupy too much of his time to admit of his taking up outside issues, which he considers mere refinements of practice. There are others who take the absurd view that, to add a knowledge of the natural sciences is to become in the highest sense of the word a Chemist, a Physicist, or a Biologist. Seeing that the attainment of a complete knowledge of either of these sciences, is a work of a life time, it is argued, that they are to be shunned as impossibilities.

The path of the would-be scientific medical man is made clear by the encouraging words of one of his own profession, Dr. G. Vivian Poore, M.R.C.P., who says, there is a minimum of knowledge in this respect which is sufficient to endow the physician with a *scientific grasp of his art.* What is really wanted, is sufficient knowledge to enable a medical man to read these various sciences with intelligible results for himself, *when he needs* and as often as he desires to consult them, to show him as objectively as possible, those great principles which have already found application in his healing art. This will lead him to think and enable him to act with precision in any great emergency.

Let it be understood that there is no necessity for cramming the head with a mass of details, and that our object is to enrich and not encumber the mind of the medical practitioner.

To those who are ignorant of the advantages of some knowledge of the natural sciences in medical practices, the following observations of Dr. Poore may be read with interest.

"There are those who hold that the student of medicine has but little need of special training in the natural sciences, but such a position I believe to be untenable, and if I have to say one thing more emphatically than another to the first year's students, it is to advise them, not on any account to neglect their purely scientific studies. They are the very foundation of your professional knowledge, and without a solid foundation, no firm or worthy superstructure can be raised.

How can a man hope to rightly comprehend that most complicated of all machines, the human body, with its levers, pumps, and elastic canals, unless he be first furnished with the principles of mechanics and hydraulics? Who will say that a proper knowledge of the eye, or of the many optical instruments used in medicine, is attainable without some acquaintance with the laws of light; or that the intricacies of the ear, and the art of auscultation can at all be understood by him, who knows nothing of the laws of sound. The laws of heat must be studied in order to appreciate the difficult problems afforded by the animal temperature, its variations in health and disease, and the means of influencing it by therapeutic agents. Without the principles of chemistry we should be intellectually lost in the human laboratory, and unable to employ chemical agencies in the treatment of disease; and electricity is so correlated with the other physical sciences, and of so much service both in diagnosis and treatment, that its separate study has also become essential. Neither can we altogether neglect geology and meteorology, since conditions of soil and atmosphere are now recognized as important factors in the causation and relief of suffering.

It is scarcely necessary to insist on a knowledge of those sciences which are called "biological." *Anatomy* and *Histology*, formerly the mere handmaids of medicine, but now recognized as sciences worthy of independent study, are as necessary to us as is a chart to the navigator; while *Physiology*, which teaches us the use and mode of action of the anatomical and histological elements, is the medical practitioner.

Zoology and botany are not so absolutely necessary for us as are the other sciences, but it is evident that they are very necessary as preliminary studies for the biologist, to whom we look for instruction, for without a study of the simple forms and conditions of life a proper understanding of human anatomy and physiology is not attainable, and in so far as they teach us the conditions of existence of the various vegetable and animal parasites which affect the human body, from micrococci upwards, they are necessary for us as *surgeons and physicians.* This list of sciences is truly formidable, but I nevertheless assert that there can be no true study of medicine without a knowledge of the principles of all of them; and, for my own part, I have never had any difficulty, as a teacher of clinical medicine, in discriminating easily, by a perusal of their clinical reports, between those students who have, and those who have not, had an insight into the principles of pure science.

Scientific principles are to the physician and surgeon what the sextant and compass are to the navigator. Without them he cannot rise above the rank of a lighterman or a ferryman, but must be content to remain a mere "pill-monger," or a chirurgeon of a base mechanic sort. With them he may fearlessly launch his bark upon unknown seas, and may have the good fortune to extend the frontiers of science, or discover, as it were, new continents, to give a wider scope to the art which he professes."

To the medical man who would reap the advantages held out by Dr. Poore, *we confidently suggest the value of this journal* as a means of accomplishing the ends desired, at the least cost, and most convenient form. The impecunious can thus avoid the purchase of the mass of scientific literature with which the market is flooded, and the overworked practitioner receiving the journal weekly is not embarrassed by redundancy, and yet can safely rely on passing nothing of importance, while articles of special interest to the profession will be constantly brought before his notice.

In the previous numbers of "SCIENCE" may be found valuable articles by Professors Burt G. Wilder and Sage, of Cornell; Drs. Hammond and Spitzka, of New York; Dr. Clemenger, of Chicago; Dr. J. A. Mason, of Newport, and many other specialists of equal merit.

Now the value of a knowledge of science, as a means of "getting on" as Huxley terms it, is indubitable, and while there are few trades in which some knowledge of *science* may not be profitably applied to the pursuer of his occupation, we think that the words of Dr. Poore must carry conviction, that the *student* or Physician who would attain the higher stages of development of his art, must be kept "*au courant*" with such facts and principles, which are weekly published in "SCIENCE," for they will probably find their application in every intelligible diagnosis and discussion on medical practice.

"SCIENCE," November 5th, 1881.

WE learn with regret that Dr. Ed. C. Spitzka, who has been requested to appear in the Guiteau case, by both the Government prosecutor and counsel for the defence, has declined to attend.

The question of mal-practice is not likely to be seriously entertained at the trial, and the whole issue will probably rest upon the evidence touching the insanity of the prisoner.

We should judge from the published papers of Dr. Spitzka that his evidence would be in favor of the prisoner's insanity; it becomes, therefore, the more important that he should attend, as it would avoid the suspicion, in case of conviction, that the assassin had not received a fair trial.

NEW YORK ACADEMY OF SCIENCES.*

October 3, 1881.

REGULAR BUSINESS MEETING.

Vice-president Dr. B. N. Martin in the Chair.

Twenty-five members present.

After the transaction of business, the members were invited, in accordance with the usual custom at the first meeting of the season, to present notes and observations gathered during the summer, and responses were made by Mrs. E. A. Smith, Prof. C. A. Seeley, and others.

Mr. W. L. Chamberlain referred to the gold deposits recently opened in Fulton and Saratoga counties, N. Y. The ore consists of auriferous pyrites and is contained in the gneiss of the foothills of the Adirondacks.

Remarks were made, by a member, on a visit to the sandstone quarries at Portland, Conn.: by Mr. Todd, on a peculiar atmospheric phenomenon, a vaporous band stretching across the sky, apparently not auroral, observed in the Adirondacks: and by Dr. Martin, on a remarkable atmospheric coloration, luminous brilliance of the clouds, etc. observed last month at Saratoga, in the early morning, attributing it to an abundance of a smoky fog produced by the recent forest fires, and calling attention to the fact that this phenomenon has been noticed only in the territory east of the meridian of Saratoga.

Mrs. P. Hanaford described the same appearances as seen during the "Yellow Day" Sept. 6, near Boston, and also on Nantucket: another member, as seen in the Genesee valley, explaining that the strong West and Northwest winds prevailing at the time had wafted high in the air vast volumes of smoke derived from the abundant forest fires throughout Western N. Y.: Messrs. Todd, Chamberlain, and others, describing the electric brilliance of the gas-lights, the strange modification of the green color of foliage, the absence of smoky odor, etc., as observed at Great Barrington, Mass., and in less degree in New York city: Mr. N. L. Britton, on the same facts as observed out at sea, off Fire Island and Montauk Point, Long Island, N. Y.: Prof. D. S. Martin, as observed between Saratoga and Catskill, N. Y., and Prof C. A. Seeley, calling attention to the extremely attenuated character of the carbon particles, produced by their long transportation from distant localities.

Mr. Geo. F. Kunz mentioned that Mt. Mica, at Paris, Maine, the locality so famous for colored Tourmalines for the last fifty years, had been purchased by a Mining Company and was being worked for Cassitterite, Mica and Tourmaline, principally through the efforts of Dr. A. C. Hamlin of Bangor, Maine.

Dr. Hamlin has the finest known collection of American Tourmaline, and he recently reported the finding of a crystal three inches long and one-half inch thick, a transparent gem, of a beautiful blue-green color. This was taken from the new mine, and many more remarkable specimens may be expected as the work advances.

Mr. Kunz said that during the last year a German Agate-hunter returned to his native country after 20 years collecting in Brazil, taking with him a large suite of fine colored Tourmalines, some five inches long and not more than one-eighth of an inch thick, transparent, and of a green color; also many fine green crystals with red, yellow, white, and other colored centres, many of these equalling for variety of color anything yet found, most of which will cut as gems. There is also in this lot one exceptionally fine green crystal over one inch square. This collector brought with him also at least 1000 kilos of transparent yellow Spodumene, the same as that described by A. Pisani of Paris some eighteen months ago, and is dissimilar only in color to the new variety of Spodumene found at Stony Point, North Carolina, described in the February number of the *American Journal of Science* for 1881, by Dr. J. Lawrence Smith, as Hiddenite. Some of the specimens which he brought will cut as fine yellow gems. All these were found in the Minas Geraes district. Recently a new locality for Chrysoberyl has been found in Ceylon, where they occur of gem value in an unusual variety of color. They vary in color from yellow to brown, and from brown to green. The latter color is the variety known as Alexandrite. This gem has heretofore been found but of very inferior size and color, but here it occurs of remarkable size, having in one case afforded a gem weighing 26 kts. They are a beautiful green color by day and a Columbine red, or brownish red, by night. The Chrysoberyl Cat's Eye is found here of the same color, and possessing the same dichroic property as the Alexandrite, viz., changing color, from green to red, and hence might very properly be called an Alexandrite Cat's Eye. Many of the Chrysoberyls are erroneously called and sold as a variety of sapphire.

* Official Report.

SECTION OF CHEMISTRY.

October 10, 1881.

Vice-president Dr. B. N. Martin in the Chair.
Nineteen members present.
A paper was read by Mr. James H. Stebbins, Jr., of which the following is an abstract:

ON SOME NEW SALTS OF THYMOLE SULPHO-ACID, AND SOME NEW FACTS CONCERNING THE SAME.

60 grms. thymole were dissolved in 50 grms. 66° sulphuric, at a temperature of 100 C. The pink crystalline mass so obtained was dissolved in water, and converted into the lime salt.

This salt crystallizes with two molecules of water, in rhombic plates, and shows under the polariscope a beautiful effect of circular polarized light.

FORMULA.

a $(C_6\ H_2\ (C\ H_3)\ (C_3\ H_7)\ (O\ H)\ (S\ O_3)_2\ Ca + 2\ H_2\ O$

a. Calcium salt of alpha thymole sulpho-acid.

AMMONIUM SALT.

This salt was obtained by decomposing the lime salt, with ammonic carbonate. It crystallizes in white rhombic plates, with 2 mols. of water.

FORMULA.

a $C_6\ H_2\ (C\ H_3)\ (C_3\ H_7)\ (N\ H_4\ S\ O_3)$ a $H + 2\ H_2\ O$.

The soda salt has likewise been obtained, and will be described in a subsequent paper.

Remarks were made by Mr. James D. Warner on the nature of the corona of the Sun, etc. Mr. Stebbins reported the yellow coloration of the atmosphere in September at the Thousand Islands in the St. Lawrence.

SECTION OF GEOLOGY AND MINERALOGY.

October 17, 1881.

The President, Dr. J. S. Newberry, in the Chair.
Fifty-one persons present.
Dr. Newberry exhibited specimens of Native Lead and Oxide of Lead from a mine in the Wood River country, Idaho, crystallized gray copper, and fine crystallized Rhodochrosite from the Ulay mines, Southwest Colorado.

Prof. T. Egleston pronounced the crystals of rhodochrosite to be the finest specimens ever yet found. He further called attention to the discovery of the presence of tellurium in merchant copper from Lake Superior. This curious specimen of native copper pig was found to contain about 0.5 per cent. of that element, which has never yet been detected in the copper. In the furnace the pig yields dense white fumes; it is useless for brass, etc., and only fit for the manufacture of cupric sulphate for batteries. With the tellurium are associated a little silver and gold, which are found not to be uniformly distributed, as hitherto supposed, but so variably that the proportion of silver varies widely in portions taken from the furnace within ten minutes of each other.

Dr. J. S. Newberry then remarked on "Geological Facts recently observed in Montana, Idaho, Utah and Colorado."

Idaho and Montana.—The famous placers at Helena and Virginia, which have yielded thirty millions of dollars, are now exhausted, but vein-mining is in successful progress and yielding rich results at Butte, at the Alice, Lexington, Copper Bell, and other mines. These are true fissure veins, traversing a granite formation, and the speaker predicted their abundant yield of silver and copper twenty years hence. These territories have been simply crossed by two government expeditions and their resources have not been at all studied. It is the coming mining region, more discoveries of promising mines having been recently made here than in any other portion of the country. On the east of the mountains in Montana and Wyoming lies a fine agricultural country and excellent stock range, the herds ranging freely throughout the winters, in spite of their severity, with little loss, and grazing upon a native bunch-grass (*Festuca scabrella*) and the buffalo grass (*Buchloe dactyloides*). The climate is salubrious, the country very beautiful in many parts and very promising for emigration. In the adjacent Rocky Mountain range there are also many mining opportunities.

The remarkable lava plain, 400 miles long by 75 miles wide, in Central Idaho, was then described.

Snake River, one of the chief tributaries of the Columbia, flows along its southern border for several hundred miles; its northern tributaries sinking under the lava sheet and flowing in subterranean channels 50 or 60 miles long. The rock is a basalt said to contain everywhere a small quantity of gold and silver. It is generally covered with an impalpable soil that produces a dust excessively annoying to the traveler, and sustains a general growth of sage brush. In places, however, the rock is bare and looks like a congealed stormy sea.

Three buttes are set on the surface of this lava plain, and each has probably been a local volcanic vent; but it is probable that most of this eruptive material has been an overflow from great fissures of which the position is not indicated on the surface.

Snake River crosses a portion of this plain in a cañon at the head of which are the great Shoshone Falls, 208 feet in vertical altitude.

An alluvial plain borders Snake River for 200 miles, abounding in black sand which contains much gold. This is, however, extremely fine, having been transported a long distance from its place or origin, and therefore difficult of separation. New and promising methods and machines are about to be tried in the exploitation of these extensive deposits. A wide mountain belt extends from the north side of the lava plain to and beyond the British line, and is apparently a good mining country throughout. Already a great number of productive and promising mines are opened in the southern portion of this belt. In the Wood River district the veins are not large, but numerous, regular and persistent, and the ore of high grade—mostly argentiferous galena, carrying $100 to $500 in silver to the ton. Near Challis, further north, is the celebrated Ram's Horn mine, located on a true fissure vein, generally not more than five feet wide, but continuous for more than five miles. The wall rocks are slate, the vein stone siderite (carbonate of iron), the ore gray and yellow copper, yielding $100 to $1200 in silver to the ton. A few miles west of Challis is the mining town of Bonanza, where are located the celebrated Charles Dickens and Custer mines, carrying both silver and gold.. Still further west in the Saw-Tooth range, a high and very picturesque mountain chain running north and south, recent discoveries of valuable mines have been made. From this district north to the Canadian line, a broad mountain belt extends over northern Idaho and northwestern Montana, a country which abounds in veins of silver, copper and gold. Among the mines now worked in this region the most celebrated is the Drum Lomond, in Montana. It is opened on a large vein of rich quartz and is owned by an old miner who cannot read, but who is said to have refused a million of dollars for the property. It is probably worth much more than this.

Most of the mountainous districts of Idaho and Montana are covered with coniferous forests, consisting of the Douglas spruce and the northern nut pine, *Pinus*

flexilis. The smaller plants form an Alpine flora of much interest, including many beautiful flowering species; perhaps the most striking being *Bryanthus*, which has a fine fir-like foliage and clusters of beautiful purple flowers. It belongs to the heath family and closely resembles the heather of Scotland.

The streams of this region are clear, cold, and rapid, and abound in fish, chiefly of the salmon family, and these have given the name to Salmon River, the principal water course.

Two species of salmon were running up the Salmon River, one the large Quinnat or Chinook salmon, comparatively rare, and the other the "red fish" (*Oncorhynchus nerka*). This is a small salmon, 15 to 18 inches in length, and weighing 3 to 5 pounds. As seen in their migration their bodies are brick red to purple in color, the heads dark or light green; they were then going up to their spawning ground, Redfish Lake, one of a half dozen of small lakes on the head waters of the Columbia, which are the special breeding places of this interesting fish. Coming all the way from their abode in the ocean, led by an infallible but inscrutable instinct, they push on night and day till they reach their remote birthplaces in these little lakes far up in the mountains and 1000 miles from their starting point. Here they accomplish apparently the great object of their lives, the reproduction of the species, by depositing the spawn in the shallows of the rivulets which fall into the lake.

The always attractive coloring of the fish, during this nuptial season becomes greatly heightened; the body assumes a brilliant, almost luminous red, as bright as that of the gold fish, and where numbers are dashing through the water literally in a blaze of excitement, they produce an exhibition that is strikingly novel and interesting.

When the spawning season is over they probably do not return, as none are seen descending the rivers. The young fish start on their migration to the ocean while yet very small, and within the first year of their lives, remaining away it is supposed some three or four years during which they acquire their full growth when they return to die where they were born.

An active industry has grown up in the capture of the red fish in their annual migrations, but it is pushed with so much energy and unsparing cupidity that their numbers are rapidly diminishing and the species will apparently be soon extirpated in these waters unless protected by legal enactment.

A branch of the Union Pacific Railroad is being constructed from Granger, Wyoming, to the mouth of the Columbia. On this a large amount of traffic is expected, as it will link together many settlements having a considerable resident population and traverse in different portions of the route rich agricultural and mining districts.

Dr. Newberry then briefly described a small but remarkably rich placer gold deposit he visited on the west flank of Mount Wheeler, the highest mountain in Nevada, and mentioned the discovery of an outcrop of lower silurian rocks full of fossils, including several new trilobites discovered by him in Southwestern Utah, but deferred all details till he should make them the subjects of special remark to the Academy.

Colorado.—Reference was made to the general character of Southwestern Colorado, the interesting topography of the region, especially the vast plateau which rises westward from the base of the Rocky Mountains on to the slopes of the Wasatch; the ascent of Marshall's Pass by the Denver and Rio Grande Railroad, the most remarkable feat of railroad engineering performed in the country, and the exceedingly picturesque region about the Pagosa the greatest hot spring on the continent. Where the San Juan river issues from the mountains a prairie occurs, surrounded by picturesque forest-clad hills, and with a beautiful view of snow-clad mountains in the distance. In the centre of the prairie lies a basin 40 by 60 feet across, boiling like a huge caldron, the ebullition being produced by the violent escape of carbonic acid gas. The banks are lined by the remains of beetles, snakes, etc., destroyed by too trustful reliance upon the hot waters, and by interesting mineral deposits. This is one of the most beautiful places in the country and likely to be a famous resort.

Along the route from Pueblo to Gunnison and Lake City, and thence eastward by Del Norte, there are some places of resort for invalids and pleasure-seekers, which are destined to be very well known, being far more beautiful and salubrious than the now celebrated localities at Manitou and Colorado Springs. One of these is Wagon Wheel Gap, on the Rio Grande. The river is a rapid, turbulent stream, and the Gap is seven to ten miles long, just wide enough to permit a wagon-road. Then a wide, open space is reached, the basin of an ancient lake, girdled by a wonderfully beautiful amphitheatre of mountains. Here 8500 feet above the sea, the hot springs, charming rides, fine hunting and fishing, an atmosphere as pure and clear as crystal, constitute the attractions of a resort, which far surpasses any other, and which will be reached by the railroad now being pushed through the Gap about January 1, 1882.

From Gunnison, specimens have been recently brought of magnetite and hematite, which probably represent inexhaustible masses, and at Crested Butte, within twenty-five miles of this locality, is found the best coking coal in the West. The region borders on a volcanic area, and the coking coal is from that portion of the basin, which has mostly escaped the alteration by volcanic heat. It is firm and not affected by the weather, with a small amount of ash and sulphur.

On Anthracite Creek are found many thousand acres of Anthracite of better quality than that of Pennsylvania. Recent analysis made at the School of Mines shows it to contain less than one per cent. of sulphur, and three per cent. of ash.

The forest vegetation of Colorado is very simple. The piñon or nut pine is very common, also the yellow pine (*P. ponderosa*), Douglas' spruce, Menzies' spruce, etc. In the mountains the general vegetation is picturesque but not so varied as in the lowlands. The following plants are among the most characteristic in the lowlands of Colorado and Utah.

The evening primrose (*Œnothera Cæspitosa*) with its large beautiful white flowers.

The wild tobacco (*Nicotiana attenuata.*)

The sun flower (*Helianthus.*)

The bee flower (*Cleome integrifolia*) presenting purple acres by the roadside, and the yellow species (*C. lutea*) less common.

The American primrose (*Primula Parryi.*)

The pasque flower (*Anemone patens, Var. Nuttalliana.*)

The *Eriogonums*, about twenty species, coloring whole mountain sides yellow.

The Oregon grape (*Berberis aquifolium.*)

Phacelia circinata in tufts of purple flowers on rocky slopes.

The lily (*Calochortus Gunnisoni* and *C. Nuttalli*) or "blackeyed Susan" (Indian—"Seego,") very plenty in the moister portion of the sage-plains.

The clematis (*Anemone alpina*) with its purple flowers.

The penstemons, of which 20 or 30 species are peculiar to that country, deep crimson, pink, and purple and blue in color, often very showy, and so abundant that whole acres of ground are colored by them.

The columbine (*Aquilegia canadensis,*) and also a much larger species (*A. cerulea,*) clothing the mountains of Colorado and Utah, with blue, cream-colored, and white flowers. A large number of dried plants were exhibited from a collection of several hundred species just brought on from Colorado, with collections procured from Prof. Marcus Jones of Salt Lake City, and others.

RETARDED DEVELOPMENT IN INSECTS.*

By C. V. Riley, Washington, D. C.

In this paper the author records several interesting cases of retarded development in insects, whether as summer coma or dormancy of a certain portion of a given brood of caterpillars, the belated issues of certain imagines from the pupa, or the deferred hatching of eggs. One of the most remarkable cases of this last to which he calls attention, is the hatching this year of the eggs of the Rocky Mountain Locust, or Western Grasshopper (*Caloptemus spretus*), that were laid in 1876, around the Agricultural College at Manhattan, Kansas. These eggs were buried some ten inches below the surface, in the Fall of 1876, in grading the ground around the chemical laboratory. The superincumbent material was clay, old mortar and bits of stone, and a plank sidewalk was laid above all. In removing and regrading the soil last spring Mr. J. D. Graham noticed that the eggs looked sound and fresh, and they readily hatched upon exposure to normal influences, the species being determined by Prof. Riley from specimens submitted by Mr. Graham. Remarkable as the facts are there can be no question as to their accuracy, so that the eggs actually remained unhatched during nearly four years and a half, or four years longer than is their wont, and this suggests the significant question: How much longer could the eggs of this species, under favoring conditions of dryness and reduced temperature, retain their vitality and power of hatching?

Putting all the facts together Mr. Riley concludes that we are as yet absolutely incapable of offering any satisfactory explanation, based on experiment, of the causes which induce exceptional retardation in development among insects. It is a very general rule that a rising temperature stimulates and accelerates growth, while a falling temperature retards and torpifies, and experiments recorded by the author* show that such is the case with regard to the eggs of *Caloptcnus spretus*. But there are many exceptions to the rule. The eggs of Crustaceans, as *Apus* and *Cypres*, are known to have the power of resisting drought for six, ten or more years without losing vitality, while in some cases they seem actually to require a certain amount of desiccation before they will hatch. Yet the fact remains that different act differently in this respect. In short, nothing is more patent to the observing naturalist than that species, and, even individuals of the same species, or the progeny of one and the same individual, act very differently under like external conditions of existence; or in other words, that temperature, moisture, food, etc., influence them differently. Hence—as has been shown by Semper to be the case with other animals, so it is with insects—changes in the external conditions of existence will not affect the fauna as a whole equally, but will act on individuals. We can understand how this great latitude in susceptibility to like conditions may and does, in the case of exceptional seasons, prove beneficial to the species by preserving the exceptional individuals that display the power to resist the unusual change; but we shall find ourselves baffled when we come to seek a demonstrable explanation of the cause or causes of such retardation, while the principles of evolution afford us the only hypothetical one at all satisfactory. In the innate property of organism to vary, and in the complex phenomena of heredity, we get a glimpse at the cause—a partial explanation—of the facts of retarded development; for the exceptional tendency in the present may be looked upon as a manifestation through atavism of traits which in the past had been more commonly possessed and more essential to the species.

*—Abstract of a paper read before the Entomological Section of the A. A. A. S., at Cincinnati.

*—9th Rep. Ins. Mo., also 1st Rep., U. S. Entomological Commission.

ON THE "LIFE DURATION OF THE HETEROCERA (MOTHS*)."

(Abstract.)

By J. A. Lintner, State Entomologist of New York.

The subject of life duration of our insects, not having been given special study, so little is known upon it, that the present contribution would not be warranted, were it not that the confession of our ignorance upon the point, may serve as an incentive to its examination.

It is a difficult field of study, for the observations should be made upon the insects in the natural conditions—not in confinement. Even of the latter state, our knowledge is quite limited. Entire broods of species have seldom been reared, except in the Bombycidæ and Sphingidæ, where the eggs are easily to be obtained. But in the large family of Noctuidæ, I do not know that an entire oviposition, or even a considerable part of one, has ever been carried through to the perfect stage, nor have I any personal knowledge of the time, place, manner or duration of copulation among them.

In the Attacinæ of the Bombycidæ, the lives of most are brief; that of the female seldom reaches fifteen days, while in the male it is still shorter. It is longer in the Sphingidæ.

We may best obtain an approximation to the life period of the moths, from reference to the dates when they are observed abroad. The lists published of collections "at sugar," furnish us with the best data. From a list prepared by myself, it appears that a large number of species of Noctuidæ were abroad for about one month. Deducting one-third of this time for their probable unequal emergence from the pupæ, there would remain a term of three weeks for their approximate life duration.

Mr. W. L. Devereaux, of Clyde, N. Y., from his observations, also infers, "that most of the species remain for about a month."

As would be expected from so heterogeneous a family as the Noctuidæ, the different groups present different life periods. The genera Xylina, Homoptera and Catocala, are found to have a considerably longer continuance than that above given. Three species of Xylina were observed by me for forty-one, forty-seven and fifty-one days; ten species of Catocala, for an average of forty-five days; and nine species of the same genus, as reported by Mr. Devereaux, for fifty-seven days.

In view of the benefits which would result to Entomology, it is suggested, that in future lists published of our insects, the different dates at which they are observed throughout the year, be included. It would aid us in determining life duration—would indicate the time when to guard against the commencement of insect attack—when to commit our crops to the ground—when to search for specimens for our cabinets—in short, it would furnish an essential part of the life histories of our species.

* Read before the A. A. A. S., Cincinnati, 1881.

Mr. G. Fasoldt says, in a letter to the *American Journal of Microscopy*:—

I have ruled plates up to 1,000,000 lines to the inch, one of which was purchased by the United States Government at Washington.

These plates show lines truly and fairly ruled, as far as lenses are able to resolve, and above this point the *spectral* appearance of the bands in regular succeeding colors (when examined as an opaque object) shows, beyond doubt, that each band contains fairly ruled lines up to the 1,000,000 band.

I do not believe that I will ever attempt to rule higher than 1,000,000 lines per inch, as from my practical experience and judgment, I have concluded that that is the limit of ruling.

ELECTRIC RAILROADS IN PARIS.

The visitors at the Electrical Exhibition can see a very fine model in bronze, surmounted by very beautiful designs, which represents an electric elevated railroad. This model is placed in the large aisle very near the pavilion of the City of Paris. It is in miniature a part of the new system of railroads which should be constructed over all the great streets of the capital in order to lessen the nnmber of encumbrances and to supply the want of locomotive means of which the whole city justly complains. M. J. Chrétien, the inventor of the new system, proposes its immediate application to all the great boulevards.

Electricity has indisputable advantages over other methods of locomotion. It is an economic method which can produce very slow as well as very rapid motion, which causes no noise or smoke, and which only allows the use of light vehicles and, consequently, the laying of unobstructive tracks. The building of electric railroads in places where business is most active, will have the double result of freeing the public roads of obstructions and of giving to the public sufficient means of transportation.

The following details we borrow from a pamphlet published by M. J. Chrétien entitled "Chemin de fer electrique des boulevards." The electric road is a double-railed viaduct supported by a row of columns spaced about forty or fifty metres from each other, and placed in the middle of the road. A central hollow beam rests on the columns and carries all the load; it runs the whole length of the boulevard, at a height varying from five to seven metres above the earth in order to compensate for the irregularities of the ground. On each side of the beam the rails are placed, resting on a metallic platform, so that there is one on the right and another on the left.

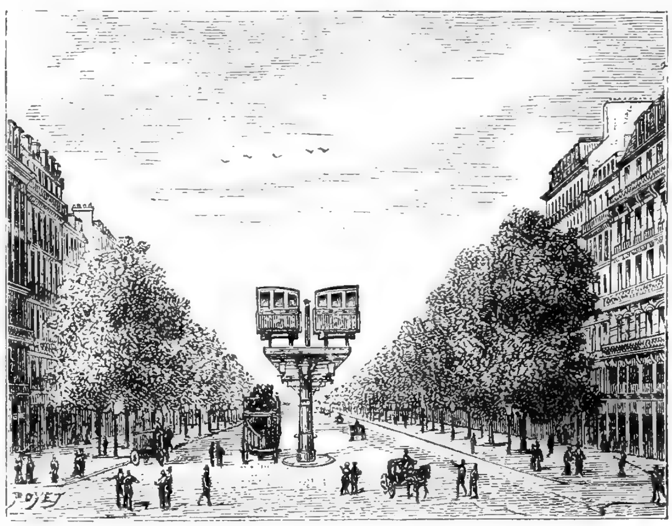

Fig. 3.—SECTION OF THE PROPOSED ROAD.

The stations, twelve in number, are placed about 500 metres from each other, and we can ascend by a very convenient staircase, placed either over the sidewalk or over the pavement. For the station most elevated above the ground, electric elevators are provided for the use of those who wish to ascend. It can even be said that it is easier to take the electric road than to ride in an omnibus.

Two works for the supply of the motive force are placed under the ground. Each of these works consists of steam engines to furnish the motive force and Gramme dynamo-electric machines to produce the electric currents, when they are set in motion by the steam engines.

The electricity thus produced is transmitted through the whole length of the road by conducting wires, and distributed to the various carriages. These are put in motion by means of electric machines, which each of them carries, and which receive, through the conducting wires, the electricity necessary to attain the speed required.

Thanks to very simple means, applications of electricity, there can be no collisions, no accidents of any kind; the motions are easier than in the ordinary railroad, and the carriages can regulate their speed with remarkable precision.

The speed is about 350 to 400 metres a minute, that is to say, the speed of a good trotting horse; with this speed, and reckoning a half minute for the mean time of stoppage at station, the whole length of the road, which is 4500 metres, can be traversed in about 17 or 18 minutes. This is half the time of an ordinary omnibus.

According to the ideas which we draw from the pamphlet spoken of, the capacity for transportation of the electric road is so great, that we with some difficulty accept the given figures, while it is easy to try the exactness of them. Thus, every minute a carriage, with places for 50, passes each station; so that, if the carriages are always full, there will be 100 persons carried each minute in the two directions, and if we take account of the additions during the journey, we will have about twice as much; that is about 200 persons every minute, or 12000 per hour. But it is possible to still increase the carrying capacity, and to reach the maximum corresponding to the trip of two carriages joined together, at intervals of a minute. We will arrive, in this case, to the colossal figure of 24000 persons an hour. Although these figures appear more than sufficient, it is certain, for those who know the activity at certain times upon the boulevards, that there are times when everybody cannot find room without waiting.

From the given estimate, the total expense necessary for the construction of the road, will only be from eight to ten millions, according to the greater or less magnificence necessary to construct a work of this importance in the centre of such a city as Paris.

Assuming an expense of eight millions, it is calculated that the price of a seat can be fixed at ten centimes, to realize profits large enough to pay the city an annual revenue of a million or a million and a half, without asking any subsidy whatever. The electric road has then as its several results, the furnishing the means of an agreeable, easy and economic locomotion, the satisfying the demands of a great traffic, which is growing day by day, and the supplying of an important revenue to the city, while still the price of a seat is kept at ten centimes.

In regard to the appearance of the road, which has a great importance in such a city as Paris, where art has never been too much sacrificed, it will certainly be seen, after a careful examination of the given designs and the engravings, that it is possible to give an artistic character to this work. Such as it is represented, the elevated road lacks neither strength nor magnificence; it is in the modern style, which alone is becoming to a work which our ancestors never dreamed of. It has been suggested besides, that, in order to fully satisfy the artistic demands, a competition should be opened to all architects and artists; and this would certainly lead to excellent results.

The utility and advantage of the electric road cannot be disputed; that it should exist is obvious, and the proposed work leaves nothing to be desired. This splendid work will certainly be accomplished, but perseverance and labor are necessary in order to vanquish all resistance, routine, and inertia, against which it would otherwise be fatally injured.—*Translated from La Nature.*

CHLORAL HYDRATE IN TOOTHACHE.—Dr. Spörer recommends that three to four lumps of hydrate of chloral (0.03–0.06 gram), should be inserted into the hollow and painful tooth, the chloral being allowed to dissolve.—*St. Petersburg, Med. Wochenschrift.*

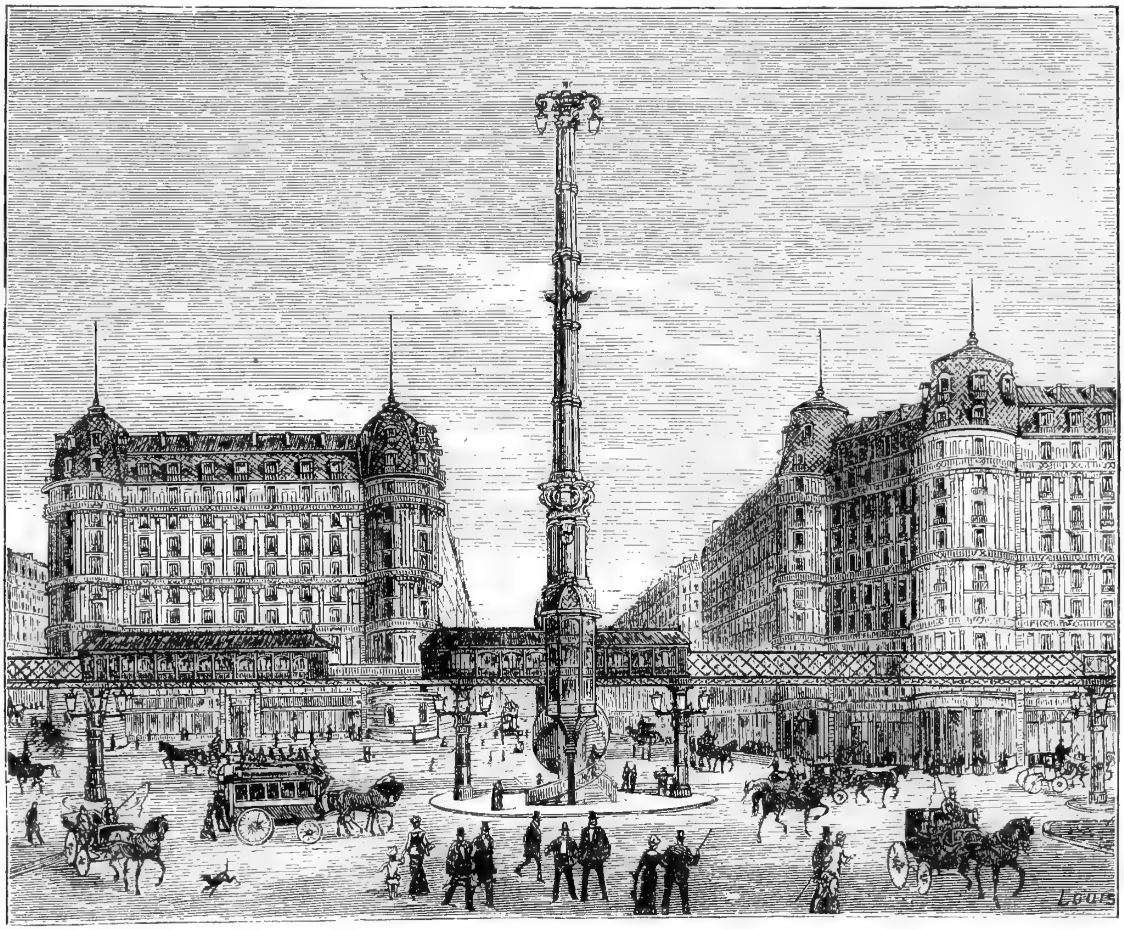

FIG. 1. ELECTRIC ELEVATED RAILROAD FOR THE BOULEVARDS OF PARIS. PLAN OF M. CHRÉTIEN. VIEW OF A STATION.

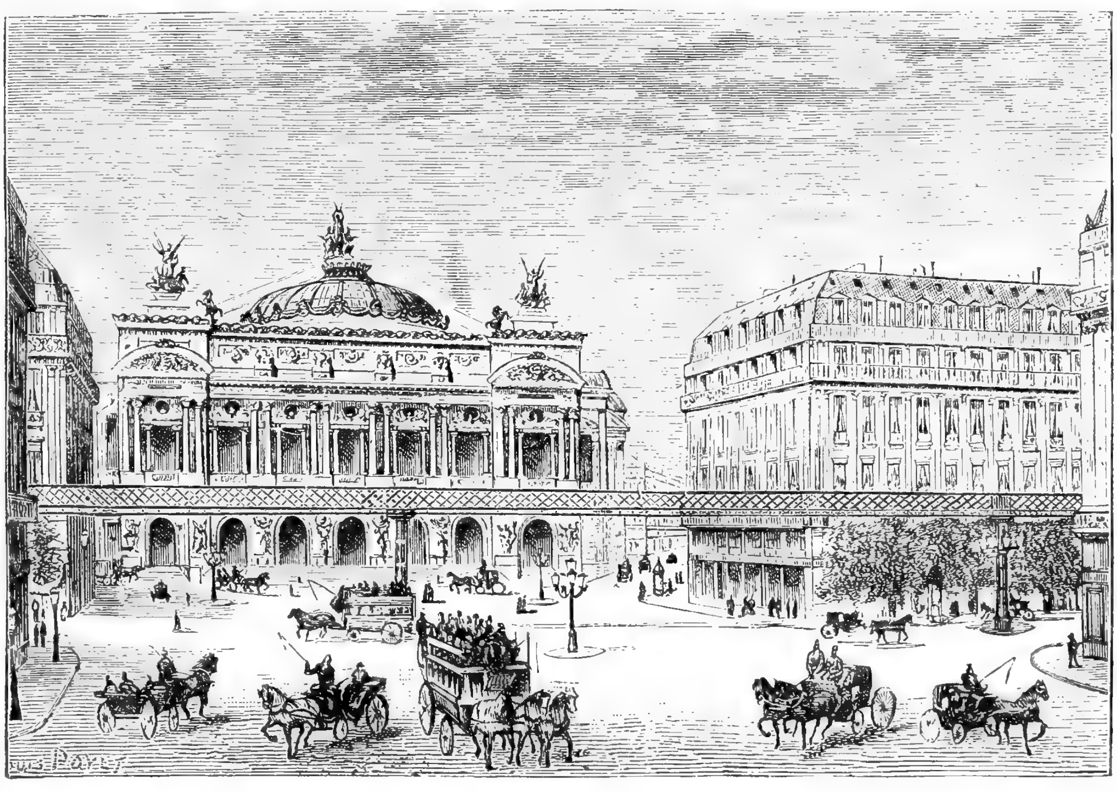

FIG. 2. PLAN OF THE ELECTRIC ELEVATED ROAD. VIEW BEFORE THE GRAND OPERA OF PARIS.

MECHANICAL SCIENCE*

By Sir W. Armstrong, C.B., D.C.L., LL. D., F.R.S.

The astonishing progress which has been made in the construction and application of machinery during the half century which has elapsed since the nativity of the British Association for the Advancement of Science, is a theme which I might with much complacency adopt in this address, but instead of reviewing the past and exulting in our successes, it will be more profitable to look to the future and to dwell on our failures. It is but justice to say that by growing experience, by increasing facilities of manufacture, and by the exercise of much skill and ingenuity, we have succeeded in multiplying and expanding the applications of our chief motor, the steam-engine, to an extent that would have appeared incredible fifty years ago; but the gratulation inspired by this success is clouded by the reflection that the steam-engine, even in its best form, remains to this day a most wasteful apparatus for converting the energy of heat into motive power. Our predecessors of that period had not the advantage of the knowledge which we possess of the true nature of heat, and the conditions and limits affecting its utilization. In their time heat was almost universally regarded as a fluid which, under the name of caloric, was supposed to lie dormant in the interstices of matter until forced out by chemical or mechanical means. Although Bacon, Newton, Cavendish, and Boyle all maintained that heat was only internal motion, and although Davy and Rumford not only held that view, but proved its accuracy by experiment, yet the old notion of caloric continued to hold its ground, until in more recent times Joule, Meyer, Codling, and others, put an end to all doubt on the subject, and established the all-important fact that heat is a mode of motion having, like any other kind of motion, its exact equivalent in terms of work. By their reasonings and experiments it has been definitely proved that the quantity of heat which raises the temperature of a pound of water 1° Fahrenheit, has a mechanical value equal to lifting 772 lbs. one foot high, and that conversely the descent of that weight from that height is capable of exactly reproducing the heat expended.

The mechanical theory of heat is now universally accepted, although a remnant of the old doctrine is displayed in the continued use of the misleading term "latent heat." According to the new theory, heat is an internal motion of molecules capable of being communicated from the molecules of one body to those of another, the result of the imparted motion being either an increase of temperature, or the performance of work. The work may be either external, as where heat, in expanding a gas, pushes away a resisting body, or it may be internal, as where heat pulls asunder the cohering particles of ice in the process of liquefaction, or it may be partly internal and partly external, as it is in the steam engine, where the first effect of the heat is to separate the particles of water into vapor, and the second to give motion to the piston. Internal as well as external work may be reconverted into heat, but until the reconversion takes place, the heat which did the work does not exist as heat, and it is delusive to call it "latent heat." All heat problems are comprised under the three leading ideas of internal work, external work, and temperature, and no phraseology should be used that conflicts with those ideas.

The modern theory of heat has thrown new light upon the theory of the steam-engine. We now know what is the mechanical value in foot pounds of the heat evolved in the combustion of one pound of coal. In practice we can determine how much of that heat is transmitted to the water in the boiler, and we are taught how to calculate the quantity which in the process of vaporisation takes the form of internal work. We can determine how much disappears in the engine in the shape of external work, including friction, and the remainder, with the exception of the trifling quantity saved in the feed-water, we know to be lost. Taking a good condensing engine as an example, we may roughly say that, dividing the whole heat energy into ten equal parts, two escape by the chimney, one is lost by radiation and friction, six remain unused when the steam is discharged, and only one is realized in useful work. It may be fully admitted that the greater part of the aggregate loss is inevitable; but are we to suppose that the resources of science, ingenuity, and skill have been exhausted in the attainment of so miserable a result? Nothing but radical changes can be expected to produce any great mitigation of the present monstrous waste, and without presuming to say what measures are practicable and what are not, I will briefly point out the directions in which amelioration is theoretically possible, and shall afterwards advert to the question whether we may hope to evade the difficulties of the steam-engine by resorting to electrical methods of obtaining power.

To begin with the loss which takes place in the application of heat to the boiler; why is it that we have to throw away, at the very outset of our operations, twice as much heat as we succeed in utilizing in the engine? The answer is, that in order to force a transmission of heat from the fire to the water in the boiler, a certain excess of temperature over that of the water must exist in the furnace and flues, and the whole of the heat below the required excess must pass away unused, except the trifling portion of it which disappears in the production of draught. Further, that since we cannot avoid admitting the nitrogen of the air along with the oxygen, we have to heat a large volume of neutral gas which has no other effect than to rob the fire. Considering what efforts have been made to facilitate the transmission of the heat by augmenting the evaporative surface, and using thin tubes as flues, it is vain to expect any great result from further perseverance in that direction, and unless a method can be devised of burning the fuel inside instead of outside the apparatus, so as to use the heated gases conjointly with the steam as a working medium in the engine, a remedy appears to be hopeless. We already practice internal combustion in the gas-engine, and it is clear that with gaseous fuel, at all events, we could associate such a mode of combustion with the vaporization of water. We may even regard a gun as an engine with internally-burnt fuel, and here I may remark that the action of heat in a gun is strictly analogous to that of heat in a steam-engine. In both cases the heat is evolved from chemical combination, and the resulting pressures differ only in degree. The gun is the equivalent of the cylinder, and the shot of the piston, and the diagrams representing the pressure exerted in the two cases bears a close resemblance to each other. While the powder is burning in the gun we have a nearly uniform pressure, just as we have in the cylinder while the steam is entering, and in both cases the uniform pressure is followed by a diminishing pressure, represented by the usual curve of expansion. If in the steam-engine we allowed the piston to be blown out it would act as a projectile, and if in the gun we oppose mechanical resistance to the shot, we might utilize the effect in a quieter form of motive power. But it is a remarkable fact that such is the richness of coal as a store of mechanical energy that a pound of coal, even as used in the steam-engine, produces a dynamic effect about five times greater than a pound of gunpowder burnt in a gun. I cannot, however, on this account encourage the idea that steam may be advantageously substituted for gunpowder in the practice of gunnery.

And now to turn from the fire which is the birthplace of the motive energy, let us follow it in the steam, to the condenser, where most of it finds a premature tomb. From the point at which expansion commences in the cylinder the temperature and pressure of the steam begin to run down, and if we could continue to expand indefi-

*British Association, 1881.

nitely, the entire heat would be exhausted, and the energy previously expended in separating the water into steam would be wholly given up in external effect; but this exhaustion would not be complete until the absolute zero of temperature was reached (viz., 461° below the zero of Fahrenheit). I do not mean to say that an ideally perfect engine necessarily involves unlimited expansion, seeing that if instead of discharging the steam at the end of a given expansion, we made the engine itself do work in compressing it, we might, under the conditions of Carnot's reversible cycle so justly celebrated as the foundation of the theory of the steam-engine, recommence the action with all the unutilized heat in an available form. But an engine upon this principle could only give an amount of useful effect corresponding to the difference between the whole work done by the engine, and that very large portion of it expended in the operation of compression, and this difference viewed in relation to the necessary size of the engine, would be quite insignificant, and would in fact be wholly swallowed up in friction. Carnot did not intend to suggest a real engine, and his hypothesis therefore takes no cognizance of losses incident to the application of an actual fire to an actual boiler. His ideal engine is also supposed to be frictionless, and impervious to heat except at the point where heat has to be transmitted to the water, and there the condition of perfect conduction is assumed. In short an engine which would even approximately conform to the conditions of Carnot's cycle is an impossibility, and a perfect steam-engine is alike a phantom whether it be sought for in the cyclical process of Carnot, or under the condition of indefinite expansion. Practically we have to deal with a machine which, like all other machines, is subject to friction, and in expanding the steam we quickly arrive at a point at which the reduced pressure on the piston is so little in excess of the friction of the machine as to render the steam not worth retaining, and at this point we reject it. In figurative language we take the cream off the bowl and throw away the milk. We do save a little by heating the feed water, but this gain is very small in comparison with the whole loss. What happens in the condenser is, that all the remaining energy which has taken the form of internal work is reconverted into heat, but it is heat of so low a grade that we cannot apply it to the vaporization of water. But although the heat is too low to vaporize water it is not too low to vaporize ether. If instead of condensing by the external application of water we did so by the similar application of ether, as proposed and practised by M. du Trembley twenty-five years ago, the ether would be vaporized, and we should be able to start afresh with high tension vapor, which in its turn would be expanded until the frictional limit was again reached. At that point the ether would have to be condensed by the outward application of cold water and pumped back, in the liquid state, to act over again in a similar manner. This method of working was extensively tried in France when introduced by M. du Trembley, and the results were sufficiently encouraging to justify a resumption of the trials at the present time, when they could be made under much more favorable conditions. There was no question as to the economy effected, but in the discussions which took place on the subject it was contended that equally good results might be attained by improved applications of the steam, without resorting to an additional medium. The compound engine of the present day does in fact equal the efficiency of Du Trembley's combined steam and ether-engine, but there is no reason why the ether apparatus should not confer the same advantage on the modern engine that attended its application to the older form. The objections to its use are purely of a practical nature, and might very possibly yield to persevering efforts at removal.

I need scarcely notice the advantage to be derived from increasing the initial pressure of the steam so as to widen the range of expansion by raising the upper limit of temperature instead of reducing the lower one. It must be remembered, however, that an increase of temperature is attended with the serious drawback of increasing the quantity of heat carried off by the gases from the fire, and also the loss by radiation, so that we have not so much to gain by increase of pressure as is commonly imagined.

But even supposing the steam-engine to be improved to the utmost extent that practical considerations give us reasons to hope for, we should still have to adjudge it a wasteful though a valuable servant. Nor does there appear to be any prospect of substituting with advantage any other form of thermodynamic engine, and thus we are led to inquire whether any other kind of energy is likely to serve us better than heat, for motive power.

Most people, especially those who are least competent to judge, look to electricity as the coming panacea for all mechanical deficiency, and certainly the astonishing progress of electricity as applied to telegraphy, and to those marvellous instruments of recent invention which the British Post Office claims to include in its monopoly of the electric telegraph, as well as the wonderful advance which electricity has made as an illuminating agent, does tend to impress us with faith in its future greatness in the realm of motive power as well.

The difference between heat and electricity in their modes of mechanical action is very wide. Heat acts by expansion of volume which we know to be a necessarily wasteful principle, while electricity operates by attraction and repulsion, and thus produces motion in a manner which is subject to no greater loss of effect than attends the motive action of gravity as exemplified in the ponderable application of falling water in hydraulic machines. If then we could produce electricity with the same facility and enconomy as heat, the gain would be enormous, but this, as yet at least, we cannot do. At present by far the cheapest method of generating electricity is by the dynamic process. Instead of beginning with electricity to produce power, we begin with power to produce electricity. As a secondary motor an electric engine may, and assuredly will, play an important part in future applications of power, but our present inquiry relates to a primary, and not a secondary, employment of electricity. Thus we are brought to the question, from what source, other than mechanical action, can we hope to obtain a supply of electricity sufficiently cheap and abundant to enable it to take the place of heat as a motive energy? It is commonly said that we know so little of the nature ot electricity that it is impossible to set bounds to the means of obtaining it; but ignorance is at least as liable to mislead in the direction of exaggerated expectation as in that of incredulity. It may be freely admitted that the nature of electricity is much less understood than that of heat, but we know that the two are very nearly allied. The doctrine that heat consists of internal motion of molecules may be accepted with almost absolute certainty of its truth. The old idea of heat being a separate entity is no longer held except by those who prefer the fallacious evidence of their senses to the demonstrations of science. So also the old idea of electricity having a separate existence from tangible matter must be discarded, and we are justified in concluding that it is merely a strained or tensional condition of the molecules of matter. Although electricity is more prone to pass into heat than heat into electricity, yet we know that they are mutually convertible. In short I need scarcely remind you, that according to that magnificent generalization of modern times, so pregnant with great consequences, and for which we are indebted to many illustrious investigators, we now know that heat, electricity, and mechanical action, are all equivalent and transposable forms of energy, of which motion is the essence.

To take a cursory view of our available sources of energy, we have, firstly, the direct heating powers of the

sun's rays, which as yet we have not succeeded in applying to motive purposes. Secondly we have water power, wind power, and tidal power, all depending upon influences lying outside of our planet, And thirdly we have chemical attraction or affinity. Beyond these there is nothing worth naming. Of the radiant heat of the sun I shall have to speak hereafter, and bearing in mind that we are in search of electricity as a cause, and not an effect, of motive power we may pass over the dynamical agencies comprised under the second head, and direct our attention to chemical affinity as the sole remaining source of energy available for our purpose. At present we derive motive power, from chemical attraction through the medium of heat only, and the question is, can we with advantage draw upon the same source through the medium of electricity. The process by which we obtain our supply of heat from the exercise of affinity is that of combustion, in which the substances used consist, on the one hand, of those we call fuel, of which coal is the most important, and on the other, of oxygen, which we derive from the atmosphere. The oxygen has an immense advantage over every other available substance in being omnipresent and costless. The only money value involved is that of the fuel, and in using coal we employ the cheapest oxidizable substance to be found in nature. Moreover the weight of coal used in the combination is only about one-third of the weight of oxygen, so that we only pay upon one-fourth of the whole material consumed. Thus we have conditions of the most favorable description for the production of energy, in the form of heat, and if we could only use the affinities of the same substances with equal facility to evolve electric energy instead of heat energy, there would be nothing more to desire; but as yet there is no appearance of our being able to do this. According to our present practice we consume zinc, instead of coal, in the voltaic production of electricity, and not only is zinc thirty or forty times dearer than coal, but it requires to be used in about six-fold larger quantity in order to develop an equal amount of energy. Some people are bold enough to say that with our present imperfect knowledge of electricity we have no right to condemn all plentiful substances, other than coal, as impracticable substitutes for metallic zinc, but it is manifest that we cannot get energy from affinity, where affinity has already been satisfied. The numerous bodies which constitute the mass of our globe, and which we call earths, are bodies in this inert condition. They have already, by the union of the two elements composing them, evolved the energy due to combination, and that energy has ages ago been dissipated in space in the form of heat, never again to be available to us. As well might we try to make fire with ashes, as to use such bodies over again as sources of either heat or electricity. To make them fit for our purpose, we should first have to annul their state of combination, and this would require the expenditure of more energy upon them than we could derive from their recombination. Water, being oxidized hydrogen, must be placed in the same category as the earths. In short, the only abundant substances in nature possessing strong unsatisfied affinities are those of organic origin, and in the absence of coal, which is the accumulated product of a past vegetation, our supply of such substances would be insignificant. This being the case, until a means be found of making the combination of coal with oxygen directly available for the development of electric energy, as it now is of heat energy, there seems to be no probability of our obtaining electricity from chemical action at such a cost as to supplant heat as a motive agent.

But while still looking to heat as the fountain-head of our power, we may very possibly learn to transmute it, economically, into the more available form of electricity. One method of transformation we already possess, and we have every reason to believe there are others yet to be discovered. We know that when dissimilar metals are joined at opposite ends, and heated at one set of junctions while they are cooled at the other, part of the heat applied disappears in the process, and assumes the form of an electric current. Each couple of metals may be treated as the cell of a voltaic battery, and we may multiply them to any extent, and group them in series or in parallels, with the same results as are obtained by similar combinations of voltaic cells. The electricity so produced we term Thermo-electricity, and the apparatus by which the current is evolved is the thermo-electric battery. At present this apparatus is even more wasteful of heat than the steam-engine, but considering the very recent origin of this branch of electrical science, and our extremely imperfect knowledge of the actions involved, we may reasonably regard the present thermo-electric battery as the infant condition of a discovery, which, if it follow the rule of all previous discoveries in electricity, only requires time to develop into great practical importance. Now if we possessed an efficient apparatus of this description we could at once apply it to the steam-engine for the purpose of converting into electric energy the heat which now escapes with the rejected steam, and the gases from the fire. The vice of the steam-engine lies in its inability to utilize heat of comparatively low grade, but if we could use up the leavings of the steam-engine by a supplemental machine acting on thermo-electric principles, the present excessive waste would be avoided. We may even anticipate that in the distant future a thermo-electric engine may not only be used as an auxiliary, but in complete substitution of the steam-engine. Such an expectation certainly seems to be countenanced by what we may observe in animated nature. An animal is a living machine dependent upon food both for its formation and its action. That portion of the food which is not used for growth or structural repair, acts strictly as fuel in the production of heat. Part of that heat goes to the maintenance of the animal temperature, and the remainder gives rise to mechanical action. The only analogy between the steam engine and this living engine is that both are dependent upon the combustion of fuel, the combustion in the one case being extremely slow, and in the other very rapid. In the steam-engine the motion is produced by pressure, but in the animal machine it is effected by muscular contraction. The energy which causes that contraction, if not purely electrical, is so much of that nature that we can produce the same effect by electricity. The conductive system of the nerves is also in harmony with our conception of an electrical arrangement. In fact a description of the animal machine so closely coincides with that of an electro-dynamic machine actuated by thermo-electricity, that we may conceive them to be substantially the same thing. At all events, the animal process begins with combustion and ends with electrical action, or something so nearly allied to it as to differ only in kind. And now observe how superior the result is in nature's engine to what it is in ours. Nature only uses heat of low grade, such as we find wholly unavailable. We reject our steam as useless at a temperature that would cook the animal substance, while nature works with a heat so mild as not to hurt the most delicate tissue. And yet, notwithstanding the greater availability of high-grade temperature the quantity of work performed by the living engine relatively to the fuel consumed, puts the steam-engine to shame. How all this is done in the animal organization we do not yet understand, but the result points to the attainability of an efficient means of converting low-grade heat into electricity, and in striving after a method of accomplishing that object we shall do well to study nature, and profit by the excellence which is there displayed.

But it is not alone in connection with a better utilization of the heat of combustion that thermo-electricity bears so important an aspect, for it is only the want of an efficient apparatus for converting heat into electricity that prevents our using the direct heating action of the

sun's rays for motive power. In our climate, it is true, we shall never be able to depend upon sunshine for power, nor need we repine on that account so long as we have the preserved sunbeams which we possess in the condensed and portable form of coal, but in regions more favored with sun and less provided with coal the case would be different, The actual power of the sun's rays is enormous, being computed to be equal to melting a crust of ice 103 feet thick over the whole earth in a year. Within the tropics it would be a great deal more, but a large deduction would everywhere have to be made for absorption of heat by the atmosphere. Taking all things into account, however, we shall not be far from the truth in assuming the solar heat, in that part of the world, to be capable of melting annually, at the surface of the ground, a layer of ice 85 feet thick. Now let us see what this means in mechanical effect. To melt 1 lb. of ice requires 142.4 English units of heat, which, multiplied by 772, gives us 109,932 foot pounds as the mechanical equivalent of the heat consumed in melting a pound of ice. Hence we find that the solar heat, operating upon an area of one acre, in the tropics, and competent to melt a layer of ice 85 feet thick in a year, would, if fully utilized, exert the amazing power of 4000 horses acting for nearly nine hours every day. In dealing with the sun's energy we could afford to be wasteful. Waste of coal means waste of money and premature exhaustion of coal beds. But the sun's heat is poured upon the earth in endless profusion—endless at all events in a practical sense, for whatever anxiety we may feel as to the duration of coal, we need have none as to the duration of the sun. We have therefore only to consider whether we can divert to our use so much of the sun's motive energy as will repay the cost of the necessary apparatus, and whenever such an apparatus is forthcoming we may expect to bring into subjection a very considerable proportion of the 4000 invisible horses which science tells us are to be found within every acre of tropical ground.

But whatever may be the future of electricity as a prime mover, either in a dominant or subordinate relation to heat, it is certain to be largely used for mechanical purposes in a secondary capacity, that is to say, as the offspring instead of the parent of motive power. The most distinctive characteristic of electricity is that which we express by the word "current," and this gives it great value in cases where power is required in a transmissible form. The term may be objected to as implying a motion of translation analogous to the flow of a liquid through a pipe, whereas the passage of electricity through a conductor must be regarded as a wave-like action communicated from particle to particle. In the case of a fluid current through a pipe, the resistance to the flow increases as the square of the velocity, while in the case of an electric current the resistance through a given conductor is a constant proportion of the energy transmitted. So far therefore as resistance is concerned electricity has a great advantage over water for the transmission of power. The cost of the conductor will however be a grave consideration where the length is great, because its section must be increased in proportion to the length to keep the resistance the same. It must also be large enough in section to prevent heating, which not only represents loss but impairs conductivity. To work advantageously on this system, a high electromotive force must be used, and this will involve loss by imperfect insulation, increasing in amount with the length of the line. For these reasons there will be a limit to the distance to which electricity may be profitably conveyed, but within that limit there will be wide scope for its employment transmissively. Whenever the time arrives for utilizing the power of great waterfalls the transmission of power by electricity will become a system of vast importance. Even now small streams of water inconveniently situated for direct application may, by the adoption of this principle, be brought into useful operation.

For locomotive purposes also we find the dynamo-electric principle to be available, as instanced in the very interesting example presented in Siemens' electric railway, which has already attained that degree of success which generally foreshadows an important future. It forms a combined fixed engine and locomotive system of traction, the fixed engine being the generator of the power and the electric engine representing the locomotive.

Steam power may both be transmitted and distributed, by the intervention of electricity, but it will labor under great disadvantage when thus applied, until a thoroughly effective electric accumulator be provided, capable of giving out electric energy with almost unlimited rapidity. How far the secondary battery of M. Faure will fulfill the necessary conditions remains to be seen, and it is to be hoped that the discussions which may be expected to take place at this meeting of the British Association will enable a just estimate of its capabilities to be formed. The introduction of the Faure battery is at any rate a very important step in electrical progress. It will enable motors of small power, whatever their nature may be, to accomplish, by uninterrupted action, the effect of much larger machines acting for short periods, and by this means the value of very small streams of water will be greatly enhanced. This will be especially the case where the power of the stream is required for electric lighting, which, in summer, when the springs are low, will only be required during the brief hours of darkness, while in winter the longer nights will be met by a more abundant supply of water. Even the fitful power of wind, now so little used, will probably acquire new life when aided by a system which will not only collect, but equalize, the variable and uncertain power exerted by the air.

It would greatly add to the utility of the Faure battery if its weight and size could be considerably reduced, for in that case it might be applicable to many purposes of locomotion. We may easily conceive its becoming available in a lighter form for all sorts of carriages on common roads, thereby saving to a vast extent the labor of horses. Even the nobler animal that strides a bicycle, or the one of fainter courage that prefers the safer seat of a tricycle, may ere long be spared the labor of propulsion, and the time may not be distant when an electric horse, far more amenable to discipline than the living one, may be added to the bounteous gifts which science has bestowed on civilized man.

In conclusion I may observe that we can scarcely sufficiently admire the profound investigations which have revealed to us the strict dynamical relation of heat and electricity to outward mechanical motion. It would be a delicate task to apportion praise amongst those whose labors have contributed, in various degrees, to our present knowledge; but I shall do no injustice in saying that of those who have expounded the modern doctrine of energy, in special relation to mechanical practice, the names of Joule, Clausius, Rankine, and William Thomson, will always be conspicuous. But up to this time our knowledge of energy is almost confined to its inorganic aspect. Of its physiological action we remain in deep ignorance, and as we may expect to derive much valuable guidance from a knowledge of Nature's methods of dealing with energy in her wondrous mechanisms, it is to be hoped that future research will be directed to the elucidation of that branch of science which as yet has not even a name, but which I may provisionally term "Animal Energetics."

The dark violet fluor-spar of Wölsendorf contains some strongly odorous substance, the nature of which has not hitherto been satisfactorily explained. From recent experiments (described to the Berlin Chemical Society,) Herr Low concludes that the smell is due to presence of *free fluorine*, arising on elevation of temperature through dissociation of a small quantity of flouride, (probably ceric fluoride).

CORRESPONDENCE.

To the Editor of "SCIENCE."

It seems to me that Pres. Gaines' objection to the accepted theory of vision (see SCIENCE of Aug. 6, p. 370) may easily be answered.

It is universally agreed that vision is a sensation produced by ethereal undulations, and that these undulations are induced by molecular motion in a luminous body. Each point of a luminous body is a radiant point, that is, emits rays of light in every direction, and it is by some of these rays of ethereal undulations, either directly from the luminous point or refracted by, or reflected from, some non-luminous body, that all impressions of vision are made. Hence, all non-luminous objects are manifested to vision by reflected light, but reflected light is also *radiant* light; that is, reflected ethereal waves radiate from every point of non-luminous objects manifested to vision. These waves have fallen upon the reflecting surface (not necessarily a minimum surface or front) from various directions, many of them reflected from other non-luminous objects, and, the angle of reflexion being the same as the angle of incidence, they necessarily *radiate* from the object; and by their difference of intensity, that is, by the different capacities of contiguous surfaces to reflect rays in a particular direction, we receive different impressions from the different parts of the object, and hence assign to the object peculiarities corresponding to the peculiarities of the sensations produced. Hence, though I admit that we become cognizant of objects by radiant light, I contend that in all cases where the object is not self-luminous, the rays that impress us are reflected rays produced by some luminous body of which we learn nothing from these reflected rays.

J. E. HENDRICKS.

DES MOINES.

ANOTHER CONFIRMATION OF PREDICTION.

By PLINY EARLE CHASE, LL.D.

On the 4th of October, 1878, I presented a communication to the American Philosophical Society,* in which I showed that the position of Watson's first intra-Mercurial planet, as computed by Gaillot and Monchez, represented the third intra-Mercurial term of my harmonic series. At the last meeting of the British Association, Professor Balfour Stewart read a paper in which he gave indications of sun spot disturbances by a planet, revolving in 24.011 days, and consequently having a semi-axis major of .163. This confirmation, both of my own prediction,† and of the calculations of the French astronomers, is the more interesting, because the first confirmation of my series was contained in a communication which was made to the Royal Society by Messrs. De la Rue, Stewart and Loewy, forty-one days after I had announced the series to the Philosophical Society, and published it in the New York Tribune.‡ The accordances are as follows:

PREDICTION.		CONFIRMATION.	
1st interior harmonic term	.267.	De la Rue, S. & L.	.267
3d " " "	.165.	Gaillot & Monchez	.164
		Stewart - -	.163

*Proc. A. P. S., xviii., 34-6.
†Ib., xiii., 238.
‡Ib., p. 470.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 29, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER.	BAROMETER.					THERMOMETERS.												
	MEAN FOR THE DAY.	MAXIMUM.		MINIMUM.		MEAN.		MAXIMUM.				MINIMUM.				MAXI'M
	Reduced to Freezing.	Reduced to Freezing.	Time.	Reduced to Freezing.	Time.	Dry Bulb.	Wet Bulb.	Dry Bulb.	Time.	Wet Bulb.	Time.	Dry Bulb.	Time.	Wet Bulb.	Time.	In Sun.
Sunday, 23	29.989	30.104	0 a. m.	29.848	12 p. m.	60.6	56.3	69	4 p. m.	60	4 p. m.	51	7 a. m.	51	7 a. m.	125.
Monday, 24	29.640	29.848	0 a. m.	29.508	12 p. m.	57.3	56.6	61	0 a. m.	58	0 a. m.	54	12 p. m.	54	12 p. m.	69.
Tuesday, 25	29.415	29.508	0 a. m.	29.338	2 p. m.	57.0	54.0	64	4 p. m.	60	4 p. m.	52	5 a. m.	52	5 a. m.	108.
Wednesday, 26	29.810	29.982	12 p. m.	29.500	0 a. m.	45.3	40.7	55	0 a. m.	50	0 a. m.	38	12 p. m.	37	12 p. m.	110.
Thursday, 27	30.015	30.058	9 a. m.	29.982	0 a. m.	48.6	43.0	59	4 p. m.	49	4 p. m.	34	6 a. m.	34	6 a. m.	109.
Friday, 28	30.113	30.136	9 a. m.	30.012	0 a. m.	51.7	45.3	56	2 p. m.	48	2 p. m.	44	6 a. m.	41	8 a. m.	94.
Saturday, 29	30.020	30.112	0 a. m.	29.910	12 p. m.	59.3	57.3	62	12 p. m.	61	12 p. m.	54	0 a. m.	48	0 a. m.	64.

Mean for the week .... 29.857 inches.
Maximum for the week at 9 a. m., Oct. 28th .... 30.136 "
Minimum " at 2 p. m., Oct. 25th .... 29.338 "
Range .... .798 "

	Dry.		Wet.	
Mean for the week	54.2	degrees	50.4	degrees.
Maximum for the week, at 4 pm. 23d	69.	"	at 4 pm 23d, 60.	"
Minimum " " 6 am. 27th	34.	"	at 6 am 27th, 34.	"
Range " "	35.	"	26.	"

OCTOBER.	WIND.						HYGROMETER.						CLOUDS.			RAIN AND SNOW				OZONE.
	DIRECTION.			VELOCITY IN MILES.	FORCE IN LBS. PER SQR. FEET.		FORCE OF VAPOR.			RELATIVE HUMIDITY.			CLEAR, 0 OVERCAST. 10			DEPTH OF RAIN AND SNOW IN INCHES.				
	7 a. m.	2 p. m.	9 p. m.	Distance for the Day.	Max.	Time.	7 a. m.	2 p. m.	9 p. m.	7 a. m.	2 p. m.	9 p. m.	7 a. m.	2 p. m.	9 p. m.	Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	0 10
Sunday, 23	w. s. w.	s. w.	s. w.	225	3½	3 pm	.374	.380	.447	100	55	77	0	2 cu.	10	-----	----	-----	--	0
Monday, 24	w.	n. e.	e.	124	¾	10 pm	.482	.436	.436	100	93	93	10	10	10	4.50 am 10 pm	2 pm 11 pm	9.10 1.00	.19 .01	0 0
Tuesday, 25	e.	w. s. w.	w. n. w.	116	4¾	7.15 pm	.388	.443	.309	100	82	64	10	7 cu.	0	-----	-----	-----	--	0
Wednesday, 26	n. w.	n. w.	n.	338	9¼	10.15 am	.228	.162	.190	76	45	74	0	0	0	-----	-----	-----	--	0
Thursday, 27	w. n. w.	w.	w. s. w.	163	2½	4 pm	.204	.191	.231	100	41	55	0	0	0	-----	-----	-----	--	0
Friday, 28	n. e.	n. e.	e.	93	1¼	0.10 pm	.192	.230	.244	61	51	60	4 cir. cu.	0	10	-----	-----	-----	--	0
Saturday, 29	e.	s. s. e.	s. s. e.	220	7¼	12 m	.321	.505	.523	74	94	94	10	10	10	3.15 am	12 pm	20.45	.37	

Distance traveled during the week .... 1,279 miles.
Maximum force .... 9¼ lbs.

Total amount of water for the week .... .57 inch.
Duration of rain .... 1 day, 6 hours, 55 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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JOHN MICHELS, Editor.

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SATURDAY, NOVEMBER 12, 1881.

The distribution of honors at the French Electrical Exhibition is very gratifying to the pride of the American people, as the American exhibitors have relatively carried off a large share of the prizes.

Edison has maintained the prestige of his country, and asserted the integrity and value of his wonderful series of electrical inventions, by *alone* receiving a "diploma of honor" for the electric light. This high mark of distinction he shared in other departments with the United States Signal Office, the Smithsonian Institution, the United States Patent Office, and Messrs. Graham & Bell.

Gold medals were awarded to the Anglo-American and Brush Electric Light Companies, the United States Electric Lighting Company, Elisha Gray and Taintor. Silver medals to Bailey & Puskas, Conolly Brothers & MacTighe, Dolbear, Eccard, Electric Purifier Company, Hubbard Pond Indicator Company, Western Electric Manufacturing Company, Western Electric Light Company and the Electro-Dynamic Company. Bronze medals to Messrs. Chavat, Cumming and Dion, the Hoosac Tunnel Company, the Trinitro Glycerine Works, Partz, Photo-Relievo Company, Whitehouse, Mills & Williams.

That Mr. Edison, with the whole world competing, and with every system represented, should receive from such a critical committee this special recognition and honor, as the inventor of the most perfect system of electrical illumination, appears to decide this point in a decisive manner. The practical application of this system on a scale which will astonish the world, is near at hand. The immense dynamo machines designed for use to illuminate a district in New York City with Mr. Edison's perfected lamps have been placed in position, and the mass of details connected with placing wires and fittings are nearing completion. Soon the word will be given that all is ready, and Mr. Edison will probably enjoy a triumph to which all his previous successes will be insignificant.

Mr. Edison must experience some regret that he was unable to be present at Paris, and in person receive the congratulations which would have been showered upon him, but we understand that he was most worthily represented by Mr. Charles Batcheler and Mr. Otto Moses, whose courtesy and indefatigable exertions have been fully recognized in some of our Parisian exchanges.

An instrument was lately described in a French journal, which was invented for the purpose of detecting oleomargarine as against pure butter.

This instrument discriminated between the specific gravities of the two substances. Shortly after the announcement of the making of this instrument, a report was spread in the daily papers, that the slight difference of density between oleomargarine and butter, was insufficient for this purpose.

A correspondent writes as follows on this subject: "The report that no difference of density is of any use in distinguishing oleomargarine from butter, is very easily disposed of, as the density of oleomargarine is 0.915 and the density of butter is 0.925. One will float at 15 C. in alcohol $53\frac{3}{4}$ per cent., and the other in alcohol $59\frac{1}{4}$ per cent. I mean by floating that the butter or oleomargarine will neither rise nor sink, when placed in the alcohol. If placed in the middle it will neither go to the top nor bottom, except very slowly. Of course there are persons who cannot distinguish between 0.915 and 0.925 specific gravities, and who cannot make an observation at a fixed temperature, but it is unreasonable to expect that any process can be satisfactory to such persons."

SCIENTIFIC ASSOCIATIONS IN WASHINGTON.

The three societies at the metropolis, the Philosophical, the Anthropological, and the Biological, all reorganized in October under very favorable auspices. A short account of their proceedings is given below:

Philosophical Society of Washington.—Three papers were read, one on Geology, by G. K. Gilbert, which our correspondent did not hear; a communication on Fog-signals, by Prof. Johnson, of the Light-house Board, and a paper on the Best Methods of Calculating the Solar Parallax, by Professor Harkness, of the National Observatory. Mr. Johnson's remarks were an account of investigations made the last summer upon the refractions of sound, in pursuance of the experiments set on foot by Professor Henry. The inquiries were prosecuted mainly in Newport harbor and its vicinity. The facts set forth were of great interest to scientific men and of great practical value to the mariner. Professor Harkness, who is a very ready speaker, gave the Society an explanation of the various methods employed in calculating the distance of the sun and the planets, inclining to prefer the transit observations as yielding the best

results. Professor Harkness has great hopes of photography as an auxiliary in this direction.

THE ANTHROPOLOGICAL SOCIETY.—Four papers were read in October, all of them mythological and all of permanent value, to wit: the Buffalo Woman: an Omaha Myth, by the Rev. Owen Dorsey; Myths of the Wintuns, by Major J. W. Powell; the Stone God of the Putepemni, by the Rev. S. D. Hurman; and the Dangers of Symbolic Interpretations, by Col. Gerrick Mallery. It is impossible to give an abstract of a myth. We can only say that Major Powell years ago conceived the idea of studying myths by the Baconian method. He told the writer of this sketch, "there are books and books on mythology, but very few myths. I will collect a volume of well authenticated myths, from which mythologic philosophy can be deduced." The Major has himself gathered a great number, and Messrs. Dorsey and Hurman were for many years missionaries among the Dakotas, speaking their language with the greatest freedom. Our readers will be pained to hear that Major Powell has been confined for several weeks by an acute attack of iritis. Colonel Mallery's paper was a thoughtful treatment of the subject of symbolism, neatly considered in its threefold aspect of signs, emblems, and symbols. The North American Indians north of Mexico had not arrived at that psychologic stage wherein true symbolism manifests itself.

THE BIOLOGICAL SOCIETY OF WASHINGTON.—The opening meeting of the Biological occurred on the evening when the city was all excitement over the reception of our French and German guests. The session of Friday, October 28th, however, was one of considerable interest. Professor Lester F. Ward exhibited an example from the petrified forests of Wyoming, mimicking the paw of an animal, which elicited a discussion as to the formation of agates and other minerals of that character.

Mr. Henry Elliot's communication on the biology of the Sea-Otter was very instructive. Little is known of the habit of this animal, the stuffed specimens in the museums conveying a very poor notion of its form. It is supposed to breed on the great beds of kelp which float in the northern seas, having one pup at a birth. Its fur is a hundred times more valuable than all other fur products combined. The hunting is especially dangerous and requires great skill.

Professor Thomas Taylor exhibited and described a freezing microtome, in which the cooling effect of a current of water from salt and ice is used to produce the hardening. The extreme cheapness, simplicity, and practicability of this apparatus will enable the microscopist to dispense with the more costly and difficult methods hitherto used for obtaining thin sections of tissues and for examining the brain and other soft parts of the body in a rigid condition.

THE EVOLUTION OF FLYING ANIMALS.

By CHARLES MORRIS.

There are some questions in Biological science which it will be difficult, if not impossible, to settle by an appeal to facts, and in the investigation of which we are obliged to employ a degree of speculation. Thus we have abundant reason to believe that birds are direct derivatives from reptiles. We know, in fact, that these animals resemble each other in such essential particulars as to justify the grouping of them together in a single vertebrate section, the Sauropsidæ of Huxley. We can even trace, by aid of the palæontological record, some of the steps by which birds arose from their reptilian progenitors. And yet no definite hypothesis has been advanced as to how the scales of the reptile became the feathers of the bird, how the quadrupedal habit of the one became the bipedal habit of the other, or how the walking changed to the flying method of locomotion.

These questions we cannot now, and perhaps may never be able to, answer with the argument of facts. But if some probable mode by which such variations may have arisen can be suggested, the speculation will hardly be an empty one. All the great theories of science have simply the force of highly probable speculations, based on known facts; and lesser theories, if given the same basis, may prove equally desirable.

One of the most striking features in animal life is its tendency to spread outwards, functionally, in every possible direction, so as to occupy each field of nature in every advantageous manner. One-half of the animal world seeks to feed on the other half, while this second half seeks to escape being fed upon. This is one of the main elements of natural selection. Every change in organization that proves an advantage to the carnivorous animal in assailing his prey, is apt to be retained. Every change that aids his prey in escaping is likewise retained. Through this cause there have been contifiual variations, since every favorable change in the one class would prove injurious to the other class, unless met by an equal counter change.

In this long continued process of adaptation to circumstances, every advantage offered by water and land to their animal inhabitants, in overcoming their prey, or in escaping from their enemies, has been long since adopted, and an immense variety of animal forms has arisen in consequence. But the air also presents favorable conditions both for escape and pursuit, and the adaptation of animals to aerial flight is so obviously advantageous, that it must have arisen as soon as the developing organization of animal life, and the occurrence of the necessary terrestrial conditions, rendered it possible.

In considering the problem of how flight originated, it will be desirable to take up successively the three questions above given. First, how did scales become feathers? The three higher classes of vertebrate animals have each its peculiar dermal covering. The Reptile has its bony plates, or its scales, the Mammal its hairs, and the Bird its feathers. Scales, hairs, and feathers are alike in origin, and are but specialized forms of a similar epithelial outgrowth. Yet these three classes of animals seldom invade each other's province. No reptile has a hairy or feathery coating. If mammals and birds were evolved from reptilian progenitors, the change of scales into hairs and feathers forms one of the processes of this evolution, and should be explicable under the natural selection hypothesis.

Certainly reptiles never became feathered through the Lamarckian process. No effort to fly, however vigorous, could have converted the scale of the reptile into the feather of the bird. It would be useless for flight until it had become almost a perfect feather, and therefore there could be no moulding influence upon its intermediate stages. The rudimentary feather must have arisen under the pressure of some other influence, and its adaptation to flight must have been a secondary resultant.

If we ask, what is the rudimentary feather, we seem to find it in the hair. In the larger land birds, as the Ostrich, the feathers on some parts of the body are indistinguishable from hairs; and in the tails of flying-squirrels the hairs spread out in a manner that seems preliminary to a development into the feathery condition. We may begin by asking, then, through what process of natural selection did the scale develop into the hair?

In seeking to solve this problem we first ask, what advantage has the hair over the scale as a dermal covering? The only positive answer we can make to this is, that it has greater warmth. It enables the haired animals to endure degrees of cold which would be fatal to the scaled animals. This difference in covering has a marked effect on the lives of the two classes of animals. Through the wide possibilities of increase in length and thickness of their hairy coat, mammals can endure the

greatest extremes of winter temperature, while reptiles are strictly summer animals, those inhabiting the colder zones being forced to hibernate during the winter. Existing reptiles, then, have no need of a warmer covering than they possess. Their local ties and life habits render this sufficient to protect them from all the changes of temperature to which they are exposed during their period of activity. But if the possession of a hairy covering would have enabled the reptiles of the past to remain active throughout the whole year in cold climates, why was it not developed? The answer is that it would have been of no special advantage to them. They are otherwise unfitted for activity during the season of low temperature, and to adapt them to this condition, not only their outer covering needed to be modified, but their internal organization as well. This change in organization has taken place in many cases, and with it the development of a warmer covering than the reptilian coat. But the reptile, thus modified, has lost its reptilian character. It has, in the one case, with other less important changes, become a bird; in the other case, with other more important changes, it has become a mammal.

The change in internal organization referred to is that in the circulating system. The imperfect heart, the sack-like lung, and the half-ærated blood of the reptile have developed into the perfect heart, the unlike but widely-extended lungs, and the fully ærated blood of the mammal and the bird. The varying temperature of the reptile is exchanged for the unvarying temperature of his successors. The so-called cold-blooded reptile, with its insufficient oxygenating organs, is at a disadvantage as compared with the bird and the mammal, with their fully oxygenated blood.

To bodily activity is necessary an internal temperature sufficiently high to render the organic chemistry of the body active. In the temperature of the tropics, and the summer temperature of the extra tropical zones, all animals possess this temperature, and none are at a disadvantage in this particular. But the reptile depends directly on the solar heat for its temperature, the bird and the mammal do not. Thus when the temperature falls the internal temperature of the reptile similarly decreases, its organic chemical change declines in activity, it becomes sluggish in movement, unable to obtain food, and would perish but for the hibernating habit which is customary with it. But the bird and the mammal preserve the temperature essential to organic chemical activity. They continue, therefore, awake and energetic, and in a condition to obtain the necessary food-supply.

The reptile is essentially a tropical animal. Its organization unfits it for the extremes of extra tropical temperature, and it is active in the temperate and frigid zones only during the tropic heat of their summers, but conceals itself and continues torpid during the cold of their winters.

Birds and mammals are essentially adapted to a life in the colder zones. They must have originated in regions in which wintry cold, for some part of the year, replaced the summer heat. The reptilian circulation sufficed for the needs of animals bathed in a fixed degree of external heat, high enough to promote their bodily activity. But animals exposed to severe cold during any portion of the year must either hibernate during that period, or must gain an improved circulation. The heat which fails them without must be produced within, or their activity must cease.

This is what we must understand from the systems of circulation of the bird and the mammal. Their reptilian progenitors slowly gained more complex lungs with an increased ærating surface; the blood became more fully oxygenated; the arterial and venous blood became more completely separated in the chambers of the heart; and as a natural result the internal temperature increased. Such slight changes could not have been preserved and augmented unless of advantage. They would have been of no special advantage to the tropical animal. To the animal of the temperate zones they were decidedly advantageous, in enabling it to remain active during a greater portion of the year, and finally during the whole year, the internal stores of heat replacing the lost external stores when winter replaced summer.

But these internal stores must not only be produced, but must be retained. A heat-retaining covering is necessary to hinder the chilling effect of the wintry air. The reptilian scale is obviously not sufficient for this purpose. As the internal heat of the animal increased, and it was able to prolong its period of active life more and more into the cold season, some modification of the scale became necessary, so as to make it more efficient in retaining this internal heat. The scales may, from their points of origin, have grown out longitudinally, covering each other in successive layers, and thus forming a warmer and closer covering. Such a process of elongation, if accompanied by a narrowing of the individual points of origin, would, in time, convert the the scale into a hair. It is well known that they are capable of becoming so converted, by such an elongating outgrowth.

Thus the haired and feathered animals could not have arisen until the possibly general summer of early times was replaced by a double season of summer and winter in the extra tropical regions. But though thus of temperate origin, there was nothing to hinder their spreading both into the frigid and the tropic zones. Their improved circulation gave them an activity superior to that of the preceding reptilian rulers of the tropics, and they thus had an advantage in the life battle, which soon showed its effects. The giant reptiles disappeared and giant mammals took their place. Gradually the reptiles retreated before the march of the mammals. They sank to the ground, hid in holes, learned to creep, to squirm, to swim, while their mammalian successors proudly stalked over their conquered realm, the lords of the earth.

If this, through the advantage gained by adaptation to wintry cold, animals were evolved possessed of a perfect circulation, a fixed internal temperature, and a poorly conducting external covering of hair; and if these animals, through their improved powers, banished their reptilian predecessors, or forced them to retreat to the waters, the holes, and the dark recesses of the earth; it remains to consider the subsequent variations of these hair covered animals; or, at least, of the flying sections of these creatures.

The next question to be considered is that of the change from a quadrupedal to a bipedal habit of motion. There is only one true biped among the whole great class of mammals, namely, man. He is approached in this bipedal habit by the higher apes, and it is not difficult to understand how the specialization of limbs took place in the latter. It undoubtedly arose from the climbing habits of monkeys. The fore limbs became used as grasping organs, the hind limbs as supporting organs. As climbing monkeys increased in size, they must in many cases have moved by grasping upper branches with their hands, and supporting their feet on lower branches. This was an imperfect bipedal movement. Eventually some of them became too heavy to render a continual arboreal residence desirable. These came to spend the most of their lives upon the earth, as we find in the larger apes of the present day. But these apes are neither quadrupeds nor bipeds. The specialization of their limbs during a long arboreal residence has unfitted them for either mode of motion upon the ground, and they move along in an awkward and inefficient compromise between the two modes of motion. Evidently the method of progression of these animals is not a desirable one for a land residence. Natural selection must tend to make them full quadrupeds or full bipeds. Those of them which have recently changed their arboreal for a ground habitat, have not had time to change. Those which earlier descended to the earth have

evolved improved methods of progression. The most of them have returned to the quadrupedal condition, if we may conceive the four-footed baboons to have arisen in this manner. As to whether any of them have gained the perfected bipedal condition, it is perhaps best to make no assertion. Those who hold that man had an ape-like progenitor, must accept this view.

There are other mammals with partially bipedal habits. These compromise the jumping animals, the kangaroos, jerboas, etc. But in these cases there has been no specialization of the fore-limbs. They have simply become partly aborted. The bear also, through its plantigrade feet, and perhaps its climbing habit, has gained imperfect bipedal powers, and a grasping habit with its fore limbs. But there has been no specialization of these limbs. They continue true walking organs.

In the reptilian world other instances of bipedal habits present themselves, developed in still another manner.

The animals thus organized are all creatures of a vanished age—the huge Deinosaurian reptiles presented to us in the geological record. These creatures may have gained their specialization of form through the same cause, though not in the same manner, as the giraffe gained its special formation. Many of them lived by browsing on the foliage of trees. And these, instead of developing an elongated neck, like the giraffe, probably obtained their food by a partially climbing process. Their fore limbs clasped the tree trunk, while their weight rested on the hind limbs and the tail. In this manner they were able to reach the desired food.

A long continuance of such habits would produce, through selection, a specialization of the fore limbs. To become efficient organs for grasping tree trunks, they must have become inefficient walking organs. Through this specialization the fore limbs seem to have become small and comparatively weak, the hind limbs large and powerful. To look at the remains of these creatures now, as preserved for us in the rock strata, it seems as if a quadrupedal motion must have been very awkward and inefficient; while their habit of erecting themselves on their hind legs, may have rendered a bipedal motion easy and natural. Professor E. D. Cope says of them: "some have chiefly squatted, some leaped on their hind legs like the kangaroo, some stalked on erect legs like the great birds, with their small arms hanging uselessly by their sides." Yet when we consider the great size of these reptiles, which comprise the huge Iguanodon and Megalosaurus, the Hadrosaurus of our New Jersey marl, and other such gigantic creatures, we may well imagine that they presented an appearance widely different from that of any existing creatures. To see animals thirty feet in height and huge in proportion, to whom our elephant would be a mere pigmy, stalking about erect on their hind legs, would certainly be an astonishing spectacle. Yet such a view was very probably presented by that bizarre world of the past which time has swept away.

These Deinosaurian reptiles, with their peculiarities of structure, their hollow bones, and their three-toed feet, presented certain strong affinities to the great land birds of modern times. So close, indeed, that some have conjectured that these large wingless birds, such as the Ostrich, are direct descendants of the Deinosaurs. In this claim there are no powers of flight to be explained, yet the possession of feathers by the ostrich seems a fatal obstacle to the hypothesis. Feathers are a highly specialized form of dermal covering. They are specially adapted to purposes of flight, and we can imagine for them no other use which the less specialized hairs or scales would not have subserved. We are therefore disposed to conclude that any animals possessed of feathers must have gained them through powers of flight in themselves or their ancestors; and that the resemblances in organization above mentioned arose from similarity in modes of progression, and not from hereditary connection.

How, then, was the further step in the process taken? The primitive hairy covering being gained, how did hairs develop into feathers, how were the imperfect bipeds among land animals succeeded by the perfect bipeds among flying animals, and how did motion upon the earth develop into motion through the air? It certainly did not arise as a result of leaping habits. We cannot imagine the spring of a kangaroo as so advantageously aided by an accidental conformation of the fore limbs, as to produce a natural selection of this conformation. If these leaping animals habitually sought to assist their flight by a motion of the fore limbs, then any membraneous expansion or special thickness of hairy covering would be advantageous. But none of those now existing have such a habit, and without it their leap could never become a flight.

(*To be continued.*)

THE NEW COMPRESSED AIR LOCOMOTIVE.

On the 13th ultimo a trial of a new engine built by the Baldwin Works, Philadelphia, took place on the Second Avenue Railroad, the result of the trial being on the whole satisfactory. Compressed air as a motive power for railway engines has been repeatedly tried already in this country and in Europe. At Paris and Nantes the Mekarski Air Engine has at different times been used with more or less success, at Glasgow. Mr. Scott Moncrieff has labored perseveringly to demonstrate the superiority of compressed air over steam for locomotive purposes, while in June last year Col. Beaumont produced (in London) an engine which was thought at the time to have eclipsed its predecessors in point of efficiency and small working cost. The "success," however, of these engines has been so very undecided, and the advantages they presented in point of cleanliness, and absence of smoke and noise, have been so counterbalanced by the cost of compressing and storing the air, that as yet we have heard of no line of railroad or tramway being *successfully* worked by compressed air.

Comparing the data obtainable from these engines with the result of the late trial, we find a decided superiority in the efficiency of the American engine which possesses several new and important features, and is the result of long experience and study of the subject by the inventor and patentee, Mr. Thos. Hardie, the Pneumatic Company's Chief Engineer.

A short description of the engine and its trial may not be uninteresting to our readers. In length and weight it is as nearly as possible the same as an average Elevated Railroad engine, the part usually reserved for the boiler being in this case occupied by the receivers for containing the air, four in number and of unequal lengths, having an aggregate capacity of 460 cubic feet, in which air is stored at a pressure of 600 lbs. per square inch. Inside the cab is a small boiler (the consumption of coal in which is nominal) through which air from the receivers is passed before being allowed to enter the cylinders. An automatic throttle valve on the supply pipe of this boiler regulates the pressure at which the cold air enters the boiling water. The air being thus heated expands and the pressure is of course considerably augmented, and in this hot, moist condition it passes into the cylinders, having a far larger percentage of efficiency than if it were allowed to do so in a cold, dry condition. There is thus by this means a great saving in the quantity of air consumed. The system of drawing air from the reservoir at a low pressure and expanding it by heat until it attains a working pressure of from 100 to 130 lbs. per square inch is, we believe, entirely novel, and in this respect the engine differs altogether from Col. Beaumont's machine, in which air was admitted to the cylinders at its initial reservoir pressure—1000 lbs., and then quickly cut off.

In a former engine built by the Pneumatic Company

the air, instead of being heated by a small boiler, was made to pass through a tank which was supplied at intervals with boiling water and recharged as soon as the water cooled. The present arrangement is the result of experience derived from its predecessor.

The valve gear is simple and is fitted with a variable expansion valve under the control of the engineer, by which the cut-off can be varied from 1-10th to 5-8ths of the stroke. The link is worked by "crossed" eccentric rods, the effect of this being to prevent *any* opening of the parts when the reversing lever stands in the middle notch. By this arrangement the cylinders are, when necessary, converted into vacuum pumps and are utilized to operate the vacuum brakes attached to the cars. It has been found that when using the air expansively while running, *i. e.*, with a quick "cut-off," the expension is sometimes so rapid that towards the end of the stroke the pressure in the cylinders is less than the external atmosphere; to obviate the loss of power which would be caused by the vacuum thus created, valves are placed in the exhaust passages, which prevent any vacuum being formed. Another feature in the engine is the existence of a suction and delivery valve at each end of both cylinders, which render it possible when going down hill, or approaching a station, to convert the cylinders themselves into "compressors," by which the pressure in the reservoirs can be increased, thus utilizing the waste energy which is usually given off in friction against the brakes. This arrangement is so successful that no other brakes are required on the engine. There are several minor points in the construction of the machine which it is not necessary to mention here; we may, however, say in conclusion that the engine has been carefully studied in every detail.

At the trial, the engine started from 128th street with a pressure in the reservoirs of 580 lbs. per inch, and travelled as far as 42nd street, a distance of 4½ miles or thereabouts, stopping at every station, and loaded with three cars containing about 50 people. At 42nd street some switching was done, and the engine then returned to the starting place, reaching 128th street with a remaining pressure of 115 lbs.

These figures show that the train would have run from Harlem to South Ferry, the entire route of the Elevated Road. But in making any practical calculation, it must be remembered that four cars are often used instead of three, and that these four cars would often be loaded with 600 persons. This probably implies an additional weight of about thirty tons to that placed behind the Pheumatic Engine during the recent experiment.

The company must be congratulated on building a most successful engine.

UNIVERSAL ENERGY OF LIGHT.*

By Pliny Earle Chase, LL. D.,

Professor of Philosophy in Haverford College.

Force is generally regarded as a function of mass and velocity. The greatest known velocities which can be produced by central forces are wave velocities. The greatest known wave-velocity which appears to be universally diffused is the velocity of light.

Let $v\lambda$ = velocity of light; v_0=circular-orbital velocity at sun's surface $=\sqrt{g_0 r_0}$; v_3=Earth's mean orbital velocity; v_r = velocity of Sun's equatorial rotation; u_3= potential velocity of water at 0°C. $=\sqrt{2g \times 100 \times 1389.6}$ ft.; u_4 = potential velocity of water at its maximum density; u_5=potential velocity of water-evaporation = $\sqrt{2g \times 536.37 \times 1389.6}$ ft.; m_0, m_3, m_5, m_6=masses of Sun, Earth, Jupiter, Saturn; h_0=Earth's semi-axis major; h_2=height of mean oscillatory projection due to the combining energy of H_2O; t_a=time of acquiring circular-orbital velocity at Laplace's limit of synchronous rotation and revolution = time of rotation $\div 2\pi$; t_n = time of acquiring "nascent" or dissociative velocity at nucleal surface = ½ time of rotation $= \pi t_a$; χ =Weber's electrochemical unit; μ=electromagnetic unit; ρ_0=total magnetic force; ρ_3=terrestrial magnetic force; t_0=present value of t_n at Sun's surface; g_0=gravitating acceleration at sun's surface.

The simplicity of the relations of the universal velocity $v\lambda$ to other physical velocities, is shown in the following equations:

$$1.\quad \frac{v\lambda}{u_3} = \frac{h_o}{h_2} = \frac{m_o}{m_3} = \frac{t_n^2}{t_a} \cdot \sqrt{\frac{\rho_0}{\rho_3}}$$

$$2.\quad \frac{v\lambda}{v_0} = \frac{v^o}{u_4} \cdot \sqrt{2} = \frac{t_n}{t_a} \cdot \frac{v_0}{v_r}$$

$$3.\quad \frac{v\lambda}{g_0} = t_0$$

$$4.\quad \frac{v\lambda}{v_3} = \sqrt{\frac{m_0 m_5}{m_3}} = \frac{m_o t_a}{m_3 t_n} \cdot \sqrt{\frac{\mu}{\chi}}$$

$$5.\quad \frac{v_5}{u_5} = \frac{3^4 m_0}{2 m_6} = \frac{5 \times 3^3 m_0}{m_5}$$

The velocity of solar atmospheric rotation, at the secular mean centre of gravity of the solar system, is also equivalent to u_5.

The law of conservation of areas, in an expanding or contracting nucleus, requires that g_0 should vary inversely as t_0. Equation 3 should, therefore, hold good for all stages of solar existence, past, present and future.

The values which satisfy the above equations are: m_0=328470 m_3; h_0=92476500 miles; $v\lambda$ = 185760 miles; v_3=18.412 miles; u_3=2986 ft.; u_5=6916.2 ft.

The following table shows the accordance between theoretical and observed values:

	Theoretical.	Observed.
Boiling point of water........	99°.18	100°
Combining heat of H_2O......	69319	67616 to 69584*
ρ_6........................	140.65	140 lb. pr. sq. in.
Maximum density of water....	4°.19	3°.33 to 4°.85
v_3........................	18.31	18.41
Latent heat of steam..........	536°.374	536°.385 †
$\chi \div \mu$........................	107.38	106.67

The velocity of light is also a factor of electromotive energy. Weber and Kohlrausch demonstrated this fact by measuring quantity of electricity; Thomson and Maxwell, by measuring electromotive force; Ayrton and Perry, by measuring electrostatic capacity.

Perhaps the most interesting of the above indications is the past, present and future equivalence of Sun's "nascent" velocity to the velocity of light; the sum of the cyclical reactions of solar superficial gravitation against the actions of external gravitation, during each half-rotation, being *equivalent to the velocity of light.*

* Read before the American Association for the Advancement of Science, August, 1881.

The Metal Actinium, by J. L. Phipson. — The author stated that he had been able to separate a new element from the pigment zinc-white. The oxide of the new element is said to be slightly soluble in caustic soda, and is soluble in ammonia and ammoniacal salts. Its color is uninfluenced by exposure to light. The sulphide of actinium is described as a pale yellow canary-colored substance; it is insoluble in ammonium sulphide, is soluble in acetic acid, and becomes darker on exposure to the air.—*British Association*, 1881.

* The mean of six estimates, cited by Naumann, is 68886.

† This is the mean of four estimates, viz.: Favre and Silbermann, 535°.77; Andrews, 535°.90; Regnault, 536°.67; Tyndall, 537°.20.

THE ELECTRIC EXPOSITION.

THE ELECTRIC LIGHT.

THE GENERATORS.

The Palace of Industry offers to the world a unique collection of apparatus for producing the electric light.

The problem seems to be solved, if we can judge by the multiplicity of the solutions proposed; we shall see in the sequel that it is not yet completely solved, but this same multiplicity sets out well in relief the incomparable elasticity of electricity applied to light, and shows that it is possible from this day to introduce the electric light in all applications; by giving to it, in each particular case, the special qualities which assure its superiority over other systems, under the limitation of two conditions which we shall treat separately: economy and the distribution of electricity.

We shall rapidly examine the processes of generating electricity for the special purpose of light; a following article will be devoted to lights, regulators, and incandescent lamps.

Three methods are known of generating electricity in quantity sufficient for the electric light: hydro-electric piles, thermo-electric piles, and electro-dynamic machines.

In this Exposition there is no thermo-electric pile applied to light. Some years ago we had hoped that M. Clamond would have continued his work in thermo-electricity, but he has, unfortunately, done nothing, and we can only express our regrets in this respect.

The piles intended for the electric light are represented at the Exposition but by two types: the pile of M. Cloris Baudet and that of M. Tommasi.

The pile of M. Cloris Baudet is a pile with bichromate of potash; with five elements, of which, according to the inventor, only one a day needs to be replaced, the pile can sustain a voltaic arc, with carbons of three millimetres, whose power is about 15 Carcel burners.

The pile of M. Tommasi is a Bunsen pile. The improvements which have been applied to it do not appear fortunate to us, and we do not yet know of an application where it serves in a practical manner for domestic usage, as the prospectus pompously announces it. It is only necessary to approach for a moment the exposition of M. Tommasi, on the ground floor, in order to *feel*—in the proper and in the figurative sense—that the vapor of liberated hypoazotic acid makes the pile absolutely inapplicable to the usage for which it was primitively intended.

The instalment of this extensive apparatus and the manipulation which it requires, are, on the other hand, out of proportion with the result obtained.

Leaving aside these two separate cases, and the electric accumulators, to which we shall return, we can say that the electric light of the Palace is exclusively obtained from mechanical generators of electricity.

The motors which drive the electric machines demand a special study. They are of two kinds: steam and gas. The Exposition contains several interesting types of motors especially intended to drive the electro-dynamic machines; we will cite among others the Brotherhood motor and the Dolgoronki rotative system motor. In these systems of motors of great rapidity, the driving shaft of the electric machine forms the prolongation of that of the motor; thus all intermediate transmission is done away with, but simplicity is purchased, it must be admitted, by a greater expenditure of steam.

The largest part of the motive force is produced by fixed, half-fixed, or movable machines varying from five to 150 horse-power. We do not say that the latter are most economic, because they consume, with equal power, much less of carbon, and because they have also a more regular motion—an essential condition for a good electric light.

We will notice more particularly two types of these powerful machines: one, exhibited by MM. Carels, is an expansion-engine, in a single cylinder; the other, exhibited by MM. Weyher and Richemond, belongs to the *compound* type, that is to say, with a compound cylinder; the expansion is made successively in the two cylinders. Figure 7 represents this motor driving the electric generators with alternate currents of Gramme and Lambotte-Lachaussée. The advantage of expansion-engines, either with one cylinder or with two conjugate cylinders, is great, for as soon as 100 horse-power is reached, less than a kilogramme of carbon is consumed each hour for every horse-power.

A large number of gas motors are also used to produce motive force. Most of them belong to the Otto type; they vary from 1 to 50 horse-power. The gas motors are practical enough, and also, up to a certain point, economical, when they serve to produce a light for a few hours each day, and in an intermittent manner.

For the same quantity of gas consumed, we can obtain 10 or 15 times more light by passing through the medium of the motor, the electric generator and the lamp, than by directly burning the gas in the ordinary burners, all in producing 100 or 150 times less heat in the light.

It is by an 8 horse-power gas motor that M. Jaspar drives the three Gramme machines which feed the three regulators placed in hall XV; a 50 horse-power gas motor also serves to light a part of the Palace.

We now come to the machines. We can first divide them, according to the generally admitted classification, into magneto-electric machines, of which the inductors are magnets, and into dynamo-electric machines, of which the inductors are electro-magnets.

The Exposition contains only two kinds of magneto-electric machines, the old type of Alliance and the machine of M. de Méritens. These machines are applied to beacon lights, and they also feed several Berjot regulators. Without wishing to condemn the electro-magnetic machines, it seems to be established, even by the Exposition, that their industrial reign has terminated. It must not be concluded by this that the electro-magnetic machines are worthless, but only that they are not industrial, in the practical sense of the word; that is to say, the power being equal, they are heavier, more expensive, and more encumbering than the electro-dynamic machines which are almost universally employed to-day.

In light-houses, where the question of capital engaged plays but a secondary role, the preference has been given to magneto-electric machines which, in consequence of the masses put in motion, give a greater relative regularity than electro-dynamic machines.

Magneto-electric machines, applied to light, are all with alternative currents.

Dynamo-electric machines are divided into two classes, according as they furnish alternative or continuous currents.

Machines with continuous currents.—The machines with continuous currents are suited to illumination by the voltaic arc and by incandescence. When they supply a single light they are mounted as represented in figure 1:

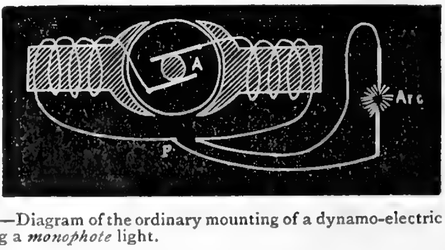


FIG. 1.—Diagram of the ordinary mounting of a dynamo-electric machine supplying a *monophote* light.

(A) an *inducted* Gramme ring, or Siemens bobbin turning between the two poles of an inductor II′, sustained

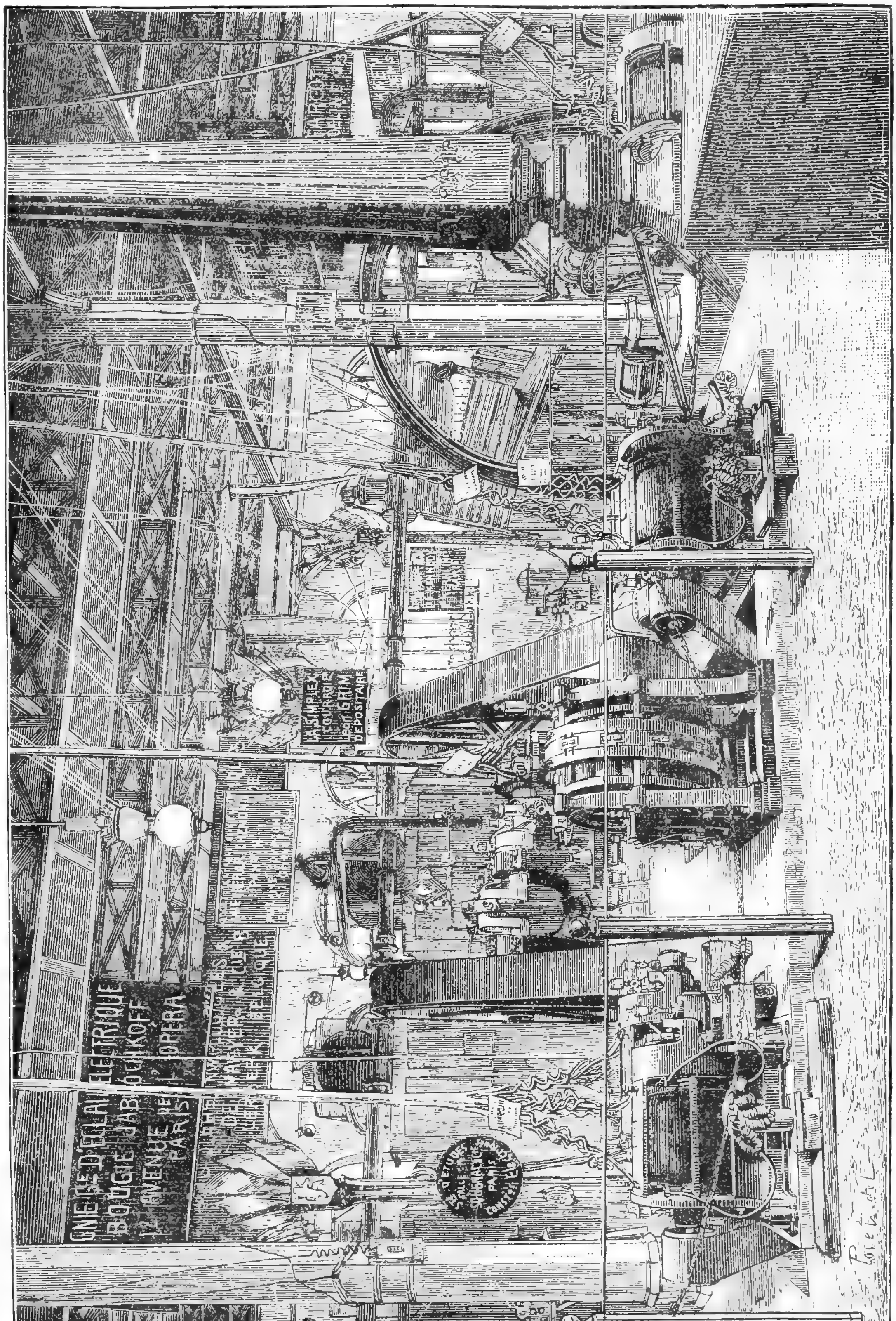


Fig. 7.—The Electric Generators of the Palace of Industry. International Exposition of Electricity. (After a photograph.)

by the current from the bobbin, which also traverses the voltaic arc.

This is the mounting adopted to-day in most of the *monophote* regulators.

On examining this system a little closer, we see that it presents a serious inconvenience. When the arc is lengthened, the intensity of the current diminishes, for two reasons, first, in consequence of the increase of resistance of the current; second, because this enfeeblement corresponds to an enfeeblement in the same way of the power of the inductors, and, as a result, of the electro-motive force of the machine, since this electro-motive force is itself a function of the power of the inductors. If the arc is made shorter, the reverse phenomenon results. This is a poor condition of regulation, since the increase of power of the machine corresponds to a shortening of the arc, and inversely, the diminution of the electro-motive force, corresponds to the lengthening of the arc. The production of the machine is, then in a contrary direction to the needs of the arc, and it is certainly one of the great reasons for which this mounting demands, in order to work well, sufficiently sensible regulators. They avoid this inconvenience by several methods.

The first consists of arranging the inductors *by derivation;* this arrangement, conceived by Wheatstone in 1866, has not yet received many practical applications. M. Siemens, of London, is studying it at the present time and we shall find, by and by, an application of it in Edison's machine.

The second method, universally employed in the machines with alternative currents and which is commencing to spread in somewhat important applications where the lights have continuous currents, consists of charging the inductors of a series of machines by a special machine. Diagram 2 represents this arrangement. The arcs 1, 2, 3, 4 are bound to the brooms of the inducted bobbins A_1, A_2, A_3, &c., of the respective machines.

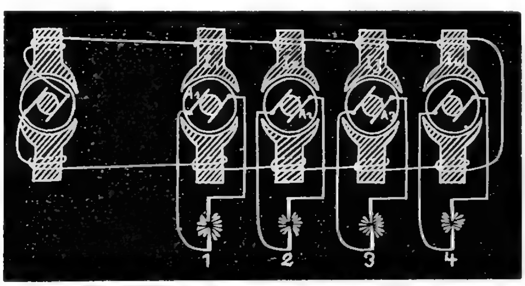


FIG. 2.—MACHINES charged by a special machine. Four machines with continuous currents supplying four regulators with a voltaic arc. The inductors are supplied by a separate generator.

By this means a constant magnetic field is assured, whose power depends only on the velocity of the generator; as a result, the electro-motive force is then absolutely independent of the variations of resistance of the voltaic arc which it sustains. Thus is found the advantage of the magneto-electric machines whose magnetic field is constant, but we gain the additional advantage of having the most powerful machines, and of being able to vary the production of these machines by regulating at will the rapidity of the generator. There is in the French section a series of machines, arranged according to this principle.

Such are the arrangements employed with the *monophote* apparatus.

When a single machine is to supply several lights the arrangements change, and the lamps can be grouped in different ways.

When they are all branched over two general conductors starting from the limits of the machine, the lights are said to be established in *derivation*, in *multiple* or in *quantity* (fig. 3). When the lights are arranged, one following the other, on one and the same conductor, they are said to be mounted in *tension*, in *series*, or in *circuit* (fig. 4).

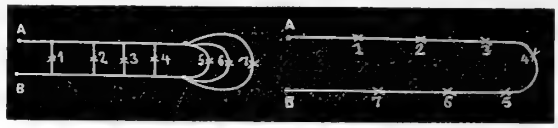


FIG. 3.—Mounting in derivation, in multiple arc, or in quantity.

The mounting in multiple arc requires volume, that in circuit requires especially pressure or tension. The one or the other is applied according to the case.

Sometimes even several *derivations* are established, each carrying two, three, ten, etc., lamps in *circuit*. It is the case, for example, of the lamps of the Swan system of incandescence, fed by Brush machines.

The reasons of these multiple combinations are easy to comprehend. If the electric source of the machine we are arranging has more electro-motive force than that exacted by a single light, it would be an advantage to group several lights on the same circuit; when, on the contrary, the volume of current which the machine can produce is greater than that which is necessitated by a single light, we arrange them in *quantity* or in *derivation*. The Edison and Maxim systems of incandescence are monted in quantity over the source. They differ only, leaving on one side for a moment the lamp itself, in the manner of regulating the current.

In the Maxim system, the mounting of which is represented in figure 5, a separate generator supplies a series of machines, whose brooms are set between them in quantity, that is to say, by poles of the same name. All the lamps are branched over the conductors in derivation. The regulating obtains, by charging *automatically* the setting of the brooms of the generator, which reacts on the power of the current of the generator, and, consequently, on that of the inductor.

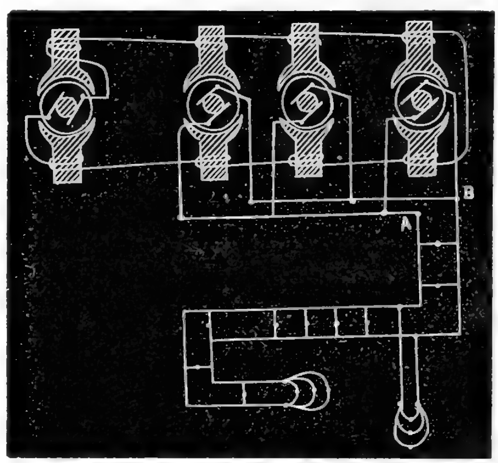


FIG. 5.—Mounting of the Maxim machines.

In the Edison system, the lamps are also mounted in derivation, but the inductors II[1] (fig. 6) are placed on a derived circuit led to the brooms of the machine in B and B[1]. The power of the inductors is regulated, and consequently that of the machine, by manœuvring by hand a rheostat which serves to increase or diminish the resistance of the generating current, and consequently the electro-motive force of the machine. It is the Wheatstone mounting.

Machines with Alternative Currents.—The employment of alternative currents steps in with electric candles, because the two carbons must be equally consumed. Certain regulators also act with the alternative currents. The equal consuming of the carbon limits the displacement of the luminous point, which is often an advantage. All

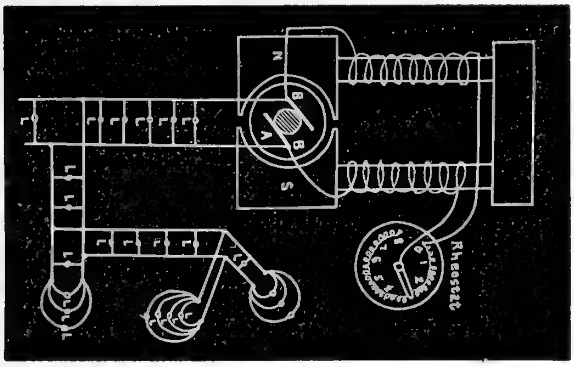


FIG. 6.—Mounting of the Edison machine.

the lights with alternate currents produce a peculiar humming owing to the nature of the currents which traverse them; this humming is often sufficient to forbid their use in places where it is necessary to have comparative silence.

The ensemble of a system of lighting by dynamo-electric machines, with alternative currents always includes two distinct machines: a machine with continuous currents or *generator*, and a machine with alternate currents, or *distributor*. This distributor consists of a variable number of circuits. Figure 8, simplified to show the

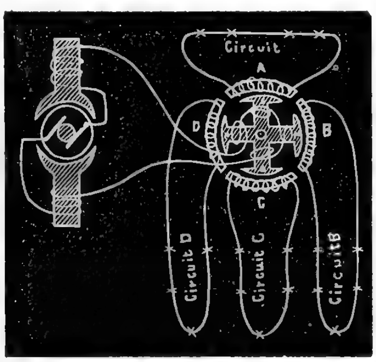


FIG. 8.—Mounting of a machine with alternate currents for candles.

principle, represents the mounting of a Gramme machine with alternative currents, supplying twenty Jablochkoff candles, arranged on four circuits of five candles each. The *movable* inductor bears eight poles, the successive ones with contrary names, in place of four. The generator can be of any system, whatever; it is only necessary to have a continuous current.

The power is regulated by the reciprocal velocities of the generator and the distributor. Sometimes the two machines mounted on the same axis turn with the same velocity, forming in reality but one. These machines are *self-generators*. In this case, we can no longer regulate the generator by its velocity, since this velocity is conjointly acting with the distributor; the regulating is then effected by the resistances introduced in the generating circuit. We have supposed the inducted bobbins *fixed* and the inductors *movable*. It is the case with the Lontin, Gramme, and Lambotte-Lachaussée machines. At other times, as in the Wilde and Siemens machines with alternate currents, the inducted bobbin is movable and the inductors fixed, but nothing is changed for this in the general principle. We see from these several examples that the art of the engineer allied with the science of the experimentalist, offers some resources to convert mechanical energy into electric energy and then to distribute it to the lights which utilize it.

MICROSCOPISTS.

The first meeting of the State Microscopical Society of Illinois, for the present season was held at the rooms of the Society, in the Academy of Sciences, Friday evening October 14, the President, Dr. Lester Curtis in the chair.

After the transaction of routine business, Mr. Stuart described the microscopical structure of some vegetable drugs. The subject is not suitable for abstraction, and requires illustrations to be useful.

His paper was followed by one by Dr. Curtis, describing a new stand made for him by Bulloch. This stand presented some novel features, among the most striking was a mechanical stage of extreme thinness, admitting light at an angle of 160°. The movements were effected by a double pinion above the stage, an arrangement pronounced by those familiar with the operation of the contrivance, as exceedingly useful and convenient.

The stand excited considerable interest, as did also a right angled camera lucida of German manufacture which was adapted to it, the superiority of which over the ordinary form was so marked as to be unmistakable on trying it, even under the disadvantages of a crowded room and constant jar. After a discussion of the papers, the meeting adjourned.

E. B. STUART.
Secretary pro tem.

PERMANGANATE OF POTASH USED AS AN ANTIDOTE TO THE POISON OF SERPENTS.

Very interesting experiments have been made in Brazil, by M. de Lacerda, which have established the fact that permanganate of potash is one of the most energetic antidotes to the venom of snakes. M. de Lacerda has addressed a memorial of his important works to the Academy of Sciences (meeting of the 12th of September, 1881).

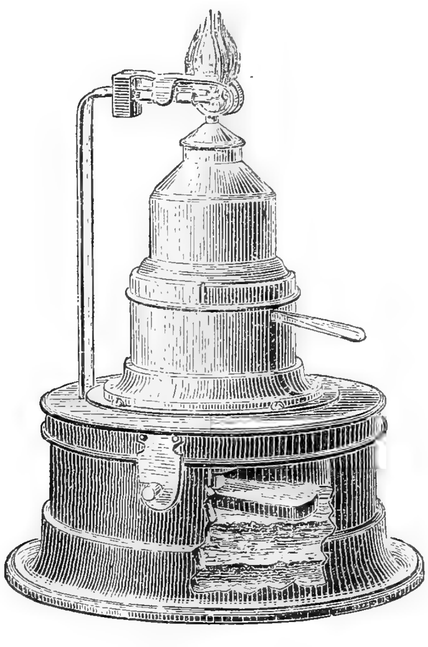
The result of these researches is really astonishing; thus in a series of experiments, frequently renewed, of injecting the active venom of *boshrops*, diluted with distilled water, in the cellular tissues, or the veins of dogs, M. de Lacerda found that the permanganate of potash was able *to stop completely the manifestation of local injuries from the venom*. Yet the same poison, which had served for these experiments, being injected without antidote into other dogs, always produced great local tumefactions, with loss of substance and destruction of tissue.

These very remarkable results have been stated on various occasions, not only by the Emperor of Brazil, who assisted at these experiments, but also by physicians, professors of faculties, and members of the diplomatic corps.

MEANS OF DETECTING THE SOPHISTICATIONS OF OLIVE OIL WITH OTHER OILS.—The oils employed at Marseille for the adulteration of olive oil are the oils of colza, sesame, cotton, and earth-nuts, Colza oil is detected by means of the sulphur which it contains; 10 grms. of the sample are saponified in a glass capsule with an alcoholic solution of caustic alkali free from sulphides. The mixture is stirred with a silver spoon, and if this is blackened, colza, or at least some cruciferous, oil is present. For the detection of the oil of sesame a little sugar is added to hydrochloric acid at 30° (Baume?) which is then mixed with an equal bulk of the oil in question. The mixture is well shaken up, and the least traces of oil of sesame are indicated by a red coloration. For the detection of cotton-seed oil there is added to the sample an equal volume of nitric acid at 40°. On stirring the mixture takes a coffee color. The detection of oil of earth-nuts is less simple. The sample is saponified with an alcoholic solution of potash, the soap separated as completely as possible, heated to expel the alcohol, and treated with enough hydrochloric acid to neutralize the alkali. The supernatant fatty acid—arachidic acid—is collected and dissolved in boiling alcohol, from which it separates in a characteristic white nacreous form.

ELECTRIC LIGHTER OF M. DESRUELLES.

This is a small apparatus, simple and practical, which will certainly be very highly appreciated by smokers and, in general, by all persons who are often in need of fire or light. It is one of the most direct applications of the drying of piles of all the systems by the process of M. Desruelles. This process consists of introducing in the piles, in the place of liquid, a kind of amianthus sponge that is afterwards filled with acid or some suitable solution. We thus gain by having a pile *dry* to some degree, which can be removed, displaced, or reversed without the liquid pouring out; this has its advantage for movable machines, such as portable lamps, piles for bells on board of ships, railroads, etc. The introduction of this inert substance diminishes the volume of the liquid; without saying that the electromotive force of the pile is not at all affected, its interior resistance is increased. This is of no importance in the case which we are now considering. The lamp consists of a small round box of wood, in which the pile is placed; over this box is placed a small lamp with oil; a platinum spiral in juxtaposition to the wick serves to produce the light.



The pile is an element to the bichromate of potash, in which the liquid is replaced by a kind of amianthus saturated with a bichromatic solution similar to that of the pile jar.

The zinc is hung from a small lever which it is only necessary to touch lightly in order to bring the zinc in contact with the sponge; the circuit is then formed, the zinc is attacked, and the current produced traverses the spiral, which reddens and inflames the oil. The pile once charged will serve for several hundred lightings. When the spiral no longer becomes red hot, the sponge must be replaced—a very simple operation. When the small lever is not pressed upon, the zinc is raised and kept thus from the action of the liquid which the sponge of amianthus absorbs. M. Desruelles constructed on the same principle a lighter to gas burners, in which the pile is placed at the extremity of an arm which is long or short, according to the height of the burner. This small domestic apparatus can be seen at the Electrical Exposition, where its practical working is shown.

INTERNATIONAL CONGRESS OF ELECTRICIANS.—Professor G. F. Barker, in a letter to the *American Journal of Science* says:

The exhibition as a whole has been a decided success. It has brought together an immense mass of highly interesting material. There are in all something over 1500 exhibitors, of which one half are French, 155 Belgian, 115 English, 114 German, 81 Italian, 72 American, 39 Austrian, 32 Russian, 21 Swedish, 13 Swiss, 17 Spanish, 13 Norwegian, 11 Dutch, 5 Danish, and 2 Japanese. Of decided novelties, there are more in the United States section than in any other. Edison has made a wonderful exhibition of his inventions, and his rooms are thronged continually. The principle discovered by him that an electric current varies friction, the so-called motograph principle, together with the applications of it practically, are beautifully illustrated. The principle of the varying resistance of bodies which imperfectly conduct, whenthey are subjected to pressure, a principle which he was the first to investigate and to apply, is exhibited in a large series of instruments, one set of which traces the progress of development of the carbon telephone. The system of incandescent lighting which he has perfected is shown in all its details, from the unique dynamo machine of low resistance and high electromotive force, the street conductors with their connections, safety-catches, expansion-caps, etc., the ingenious meter and the house conductors with their incombustible covering, to the fixtures with double conductors and safety catches, and lastly to the incandescent lamp itself. Dolbear exhibits a new electro-static telephone which performs admirably and which consists simply of two thin metal plates, connected to the secondary wire by an induction coil. They are oppositely charged by the coil and so attract each other. Gray's harmonic multiple telegraph is in successful operation and Bell's original photophone is also exhibited. The most original thing exhibited in the French section is the secondary battery: Planté exhibits several forms of it, Faure shows the improvement which he made by covering the plates with minium, and lastly Meritens is working a still newer form, in which only lead plates are used, but a large number of them are put in a small space. In the historical line the collection in the exhibition is unrivaled. The pile of Volta, the electroscopes of Galvani, the thermopiles of Nobili and Melloni, the electro-magnetic induction ring of Faraday, the first magneto-machine of Pixii, the rheostats and telegraphs of Wheatstone, the telegraphs of Sœmmering, of Steinheil and of Gauss and Weber, the continuous current-machine of Pacimotti, the electro-thermic and electro-motor apparatus of Becquerel, the electro-capillary apparatus of Lippmann; all these and many more are here collected. And as for arc lights, the exhibition at night is like day. The Brush machine and light are in great favor. A large lamp of this sort just put up has carbons two inches in diameter, and is claimed to give a light of 80,000 candles.

BOOKS RECEIVED.

A TREATISE ON THE METHOD OF GOVERNMENT SURVEY, with complete Mathematical, Astronomical and Practical Instructions. By SHOBAL V. CLEVENGER. Second Edition, revised. D. Van Nostrand, 23 Murray street, New York.

This excellent treatise will be found of the greatest value to all engaged in government land surveying, and appears to surpass all its predecessors in its completeness and adaptability for practical work. Dr. Clevenger is one of our most esteemed contributors, and our readers are aware of the thorough nature of all literary productions which proceed from his pen. The present treatise on government land survey is exhaustive of the subject, and has been accepted by the highest authorities as an authoritive manual.

SATELLITES OF MARS.

DATA FOR EPHEMERIDES OF THE SATELLITES OF MARS IN THE OPPOSITION OF 1881.

By Professor Asaph Hall.

Greenw. M. Noon. Date.	Log f	F		Log g	G		Phobos u_1	Deimos u_2	Aberr.
		°	′		°	′	″	°	m
1881 Nov. 16.0	9.97946	296	3.7	9.50991	318	56.7	308.50	234.01	— 5.9
18.0	9.97948	296	2.8	9.50901	318	41.0	46.18	84.34	5.8
20.0	9.97942	296	5.3	9.50858	318	22.0	143.86	294 67	5.7
22.0	9.97928	296	11.2	9.50865	318	0.1	241.54	144 99	5.7
24.0	9.97907	296	20.4	9.50923	317	34.9	339.22	355.32	5.6
26.0	9.97877	296	33.0	9.51037	317	7.2	76.90	205.65	5.5
28.0	9.97839	296	49.1	9.51205	316	36.8	174.58	55.97	5.4
30.0	9.97792	297	8.6	9 51431	316	4.3	272.26	266 30	5.4
Dec. 2.0	9.97738	297	31.4	9.51715	315	30.0	10.95	116.63	5.3
4.0	9.97674	297	57.6	9 52055	314	54.3	107.63	326.95	5.3
6.0	9.97603	298	27.1	9.52456	314	17.8	205.31	177.28	5.2
8.0	9 97523	298	59.7	9 52910	313	41.0	302.99	27.61	5.2
10.0	9.97435	299	35.4	9.53418	313	4.3	40.67	237.93	5.1
12.0	9 97340	300	14.0	9.53976	312	28.5	138.35	88.26	5.1
14.0	9.97236	300	55.3	9.54578	311	54.0	236.03	298.58	5.0
16 0	9.97125	301	39.0	9.55218	311	21.3	333.71	148 91	5.0
18.0	9.97008	302	24.8	9.55890	310	51 0	71.40	359.24	5.0
20.0	9.96886	303	12.4	9 56587	310	23.2	169.08	209.56	5.0
22.0	9.96759	304	1.5	9.57298	309	58.4	266.76	59.89	5.0
24.0	9 96629	304	51.6	9.58016	309	36.7	4.44	270.21	5.0
26.0	9.96496	305	42.2	9.58733	309	18.1	102.12	120.54	5.0
28.0	9.96363	306	33.0	9.59441	309	2.9	199 80	330.87	5.0
30.0	9.96230	307	23.4	9.60131	308	50.6	297 48	181.19	5.1
1882 Jan. 1.0	9.96099	308	13.2	9.60800	308	41.3	35.16	31.52	5.1
3.0	9.95971	309	1.8	9.61440	308	34.6	132.84	241.84	5.1
5.0	9.95847	309	48.8	9.62047	308	30.5	230.52	92.17	5.2
7.0	9.95728	310	34.0	9.62618	308	28.5	328.21	302 50	5.2
9 0	9 95614	311	16.9	9.63149	308	28 5	65.89	152.83	5.3
11.0	9.95508	311	57.3	9.63639	308	30.0	163 57	3.15	5.4
13.0	9.95410	312	34.8	9.64084	308	32.6	261.25	213 47	— 5.5

The angle of position and the distance of the satellite, p and s, will be computed by the formulæ

$$s \sin p = \frac{a}{\rho} f \sin (F + u)$$

$$s \cos p = \frac{a}{\rho} \,.\, g \sin (G + u),$$

where ρ is geocentric distance of Mars. The values of a, and of μ, the mean distances and the mean daily motions of the satellites are as follows:

Phobos.	Deimos.
$a = 12''9531$	$a = 32''3541$
$\mu = 1128°8405$	$\mu = 285°1632$

The quantity u for each satellite is given for the corresponding dates in the columns u_1 and u_2. For elongations the value of u is given by the equation

$$\text{tang } 2u = - \frac{f^2 \sin 2F + g^2 \sin 2G}{f^2 \cos 2F + g^2 \cos 2G}.$$

Thus for Dec. 20, $u = 325°83$ at the elongation, and in the case of Deimos $s = 53\ 7$. Near the conjunctions this satellite passes within $2''5$ of the centre of the planet, and the apparent ellipse will be very eccentric. Calling the brightness of the satellites unity on October 1, 1877, the brightness of the next opposition will be as follows:

1881 Nov. 16 brightness = 0.303
Dec. 14 " = 0.399
1882 Jan. 13 " = 0.330

The brightness of the satellites on November 16 will be a little greater than when they were last observed with the 15-inch refractor of the Harvard College Observatory.

On account of the greater distances of the planet from the Earth and Sun, these satellites will be faint next December, but as the planet will be in declination + 26°, they will be within the reach of several large telescopes, and it is possible that a good series of observations may be obtained. The elongation will occur in the angles of position 68° and 248° nearly, and the satellites should be looked for carefully at such times.

After the next opposition I hope to unite the observations of 1877, 1879 and 1881 in a new determination of the orbtis.

U. S. Naval Observatory, Washington, *June* 22, 1881.

The absorption of ultra-violet rays by certain media is being investigated by M. De Chardonnet. One method adopted is to direct a beam through a liquid in a trough with parallel glass or quartz sides, to Poitevin's photochromic paper (which indicates by change of tint, the presence of actinic rays). In a second method, a solar beam from a heliostat is sent through a slit, an objective of quartz and Iceland spar, and a prism of the spar, to a photographic plate or fluorescent screen; then a trough half filled with liquid is put before the slit. The author finds that the liquid circulating in plants, or impregnating roots and fruits have all an avidity for chemical rays. Fluorescence does not seem to be directly related to intensity of actinic absorption; thus decoction of radish absorbs less than decoction of potatoes, yet the former is without the property, while the latter is not. White wine is weakly fluorescent; red wine does not fluoresce. Of the few animal liquids examined, blood is found a powerful absorbent; but the aqueous humour of a calf's eye, and the albumen of eggs, have no action on chemical rays. Distilled water, alcohol, sulphuric ether, collodion, and solution of cane sugar are also inactive. Gelatine intercepts all the chemical rays, and it is sensibly fluorescent.

A PAPER on the "Electrical Resistance and the Coefficient of Expansion of Incandescent Platinum," by E. L. Nichols, Ph.D., was read at the Cincinnati Meeting of the American Association for the Advancement of Science, August, 1881, fully reported in *Amer. Jour. Science*, November. In his discussion of the subject, the author after showing the discrepancies in the formulæ of resistances as obtained by Siemens, Benoit, Matthiesen, and other physicists, draws the following conclusions: —

1st. The formulæ in question are based for the most part upon unwarrantable suppositions, such as the constancy of the specific heat of copper and of platinum; the constancy of the coefficient of expansion of the latter metal, and upon the accuracy of certain very doubtful values for the boiling points of zinc, cadmium, etc.

2d. That, aside from the inaccuracy of those data, the varying resistance of different specimens of platinum renders any formula for the calculation of temperature of that metal from its electric resistance applicable only to the identical wire for which the law of change of resistance with the temperature has been determined.

3d. That from the data at command we are not in position to calculate the temperature of an incandescent platinum wire from its change of resistance, nor from its length, nor indeed in any other manner, further than to express the temperature in terms of the length or the resistance of the wire.

4th. That, owing to the great variations shown by different specimens of platinum as regards its resistance, the determination of the expansion of the wire is to be preferred, whenever practicable, to the measurement of its conductivity.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of "SCIENCE."

Dr. Rogers seems again to misunderstand. It was not his quotation from Faraday that, was objected to, but the use apparently made of it to support his strange "questioning of the dogma that 'gravity acts in versely as the square of the distance,' on the ground that if that force is weakened by the earth's being removed to aphelion, it could not again bring back the body to perihelion." Any attempt to sustain that position by the authority of Faraday must certainly be a failure. Your correspondent seems not to distinguish between the definition of the force of gravitation, to which Faraday pertinently objected, and the law of gravitating action to which I particularly referred, and concerning which Faraday says, in the sentence immediately preceding that quoted by your correspondent, "It will not be imagined for a moment that I am opposed to what may be called the law of gravitating action, that is, the law by which all the known effects of gravity are governed:"—the very "dogma" your correspondent assumed to question!

GEO. B. MERRIMAN.

November 2, 1881.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING NOV. 5, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER AND NOVEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 30	29.893	29.910	0 a. m.	29.826	12 p. m.	66.7	64.0	70	12 m.	65	12 m.	62	0 a. m.	61	0 a. m.	125.
Monday, 31	29.750	29.826	0 a. m.	29.702	3 p. m.	67.0	64.7	69	2 p. m.	66	2 p. m.	62	12 p. m.	62	12 p. m.	74.
Tuesday, 1	29.846	29.918	12 p. m.	29.750	0 a. m.	58.3	56.6	62	0 a. m.	62	0 a. m.	55	12 p. m.	54	12 p. m.	64.
Wednesday, 2	29.949	29.992	9 a. m.	29.798	12 p. m.	55.6	55.3	57	1 p. m.	57	9 p. m.	53	7 a. m.	53	7 a. m.	66.
Thursday, 3	29.591	29.798	0 a. m.	29.446	12 p. m.	56.6	56.3	61	11 a. m.	60	11 a. m.	47	12 p. m.	47	12 p. m.	71.
Friday, 4	29.586	29.850	12 p. m.	29.446	0 a. m.	41.0	39.0	47	0 a. m.	47	0 a. m.	38	12 p. m.	37	12 p. m.	103.
Saturday, 5	29.990	30.062	9 a. m.	29.850	0 a. m.	48.3	45.6	56	4 p. m.	52	4 p. m.	36	5 a. m.	36	5 a. m.	104.

Mean for the week .... 29.800 inches.
Maximum for the week at 9 a. m., Nov. 5th .... 30.062 "
Minimum " at 12 p. m., Nov. 3d .... 29.446 "
Range .... .616 "

	Dry.		Wet.
Mean for the week	56.2 degrees		54.5 degrees.
Maximum for the week	at 12 m. 30th 70. "	at 2 pm 31st,	66. "
Minimum "	" 5 am. 5th 36. "	at 5 am 5th,	36. "
Range "	" 34. "		30. "

OCTOBER AND NOVEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 30	w. n.w.	s. w.	s. s. w.	106	3	9.10pm	.512	.564	.577	94	79	84	9 cu.	8 cu.	10	0 am	4.30 am	4.30	.09	--
																4.45pm	12 pm	7.15	.02	0
Monday, 31	s. w.	s. w.	e. n. e.	147	8	10.15 am	.577	.599	.562	84	84	94	10	10	10	0 am	4.30 am	4.30	.17	--
																9 am	12 pm	15.00	.14	0
Tuesday, 1	n. e.	n. e.	n. e.	215	5¾	7.50 am	.487	.422	.407	94	87	87	10	9 cu.	6 cu.	0 am	8 am	8.00	.07	5
Wednesday, 2	n. n. e.	e. n. e.	e. s. e.	150	1¾	10.00 am	.403	.436	.466	100	93	100	9 cu.	10	10	-----	----	-----	--	0
Thursday, 3	s. e.	w.	n. w.	104	6¼	9.15pm	.466	.487	.403	100	94	100	10	9 cu.	10	3 am	8 am	5.00	.05	--
																8.30 am	12 pm	3.30	.18	10
Friday, 4	w. n.w.	w. n.w.	w.	374	22¾	6.40 pm	.248	.195	.194	100	67	81	8 cu.	9 cu.	0	0 am	6.30 am	6.30	.29	0
Saturday, 5	w.	s.	s. s. w.	230	7	10.30pm	.220	.282	.335	100	67	80	3cir.cu.s	3 cir.	0	-----	-----	-----	--	0

Distance traveled during the week .... 1,326 miles.
Maximum force .... 22¾ lbs.

Total amount of water for the week .... 1.01 inch.
Duration of rain .... 2 days, 6 hours, 15 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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SATURDAY, NOVEMBER 19, 1881.

EDISON'S METHOD OF PRESERVING ORGANIC SUBSTANCES.

In a recent number of this JOURNAL,* we published a report by Mr. Otto Hehner, an official analyist of London, England, who had endeavored to trace the cause of the gastric disturbances, which were traceable in persons who had consumed preserved articles of food put up in tin cases. This poisoning had been attributed by some to traces of lead dissolved from the solder with which the tins were closed, but Mr. Hehner, although admitting the occasional presence of lead in such cases, found on a careful analysis of both vegetable and animal foods thus prepared in tin cases, that the trouble was caused by tin.

With one exception he found that the whole of the samples contained more or less tin, many to such an extent that abundant reactions could be obtained from two or three grammes of the vegetable substances; whilst of the animal foods one of the soups contained thirty-five milligrammes, one of the condensed milks eight milligrammes, and oysters forty-five milligrammes of tin to the pound.

In reply to the question whether tin when thus taken up in the system was injurious or not, he states that as forensic literature does not furnish a positive and satisfactory reply, he endeavored to settle the question by making a few experiments.

These experiments which will be found on page 507 of this JOURNAL, produced results which caused him to draw the following conclusions: "it plainly follows that while stannic compounds are not injurious in the doses given, tin in the stannous condition, is a virulent irritant poison."

*" SCIENCE," No. 69, October 22, 1881.

These facts induced Mr. Hehner to demand some other and improved method of packing preserved food other than by the use of tin cases.

A remedy appears to hand at a most opportune moment. In the Patent office reports for October 18, last, we find that Mr. Edison has invented a method of preserving articles of food in glass vessels from which the air has been exhausted and a high vacuum produced. The glass vessel is then hermetically closed by sealing off the channel to the air pump, the envelope produced being essentially a homogenous piece of glass. This invention appears to meet the difficulty experienced in the use of tin cans and promises great results in offering a method of preserving fruits and other organic substances in which their original purity and freshness is maintained to a great degree, and the introduction of mineral poisons rendered an impossibility. The specification, as usual, is very brief and we hope to present our readers with a more detailed description of this interesting invention, on a future occasion.

ALCOHOLIC TRANCE.

At a meeting of the New York Medico-Legal Society, held at the Hall of the Academy of Medicine, November 2, Dr. Crothers read a paper on "Alcoholic Trance." The main point of the paper consisted in an attempt to establish the existence of a trance-like condition in inebriates. In this condition they were supposed to commit all sorts of ridiculous, or injurious, or even criminal actions, without a subsequent recollection of what they had done. Dr. Crothers related cases, the like of certain of which no other physician has yet seen or reported, and the like of which it may be quite safe to say no other physician is likely to record in the future. One was that of an engineer who ran a Mississippi steamer an entire trip without knowing it; another of a gentleman who regularly woke out of his "trance" at a railroad station, and was compelled to ask his fellow passengers where he was; a third, a house-painter, who would regularly climb to the top of a house, paint a whole story correctly, come down and "wake up." Other cases were still more complicated, and evidently called into action the *risorius* muscles of the Doctor's audience. Among the less remarkable instances was one of a hack-driver who became a confirmed drunkard, and several times went to States Prison, finally dying there, after being convicted of stealing horses; and of a solicitor, who had fits of jealousy and suspicion concerning his wife, and made a number of wills in a trance-like state.

Dr. Spitzka stated that he would like to ask the reader of the paper two questions. As far as he could gather, the reports were all obtained from third parties. His first question was whether Dr. Crothers had ever himself seen patients in this alcoholic "trance?"

Dr. Crothers replied that he had.

Dr. Spitzka reiterated that in that case the attendant phenomena had not been described by the Doctor in a convincing manner. His second question was, whether the hack-driver referred to had exhibited any inequality or anomaly of the pupils, the facial folds or tremor of the tongue and hands?

Dr. Crothers replied that he had manifested none of these symptoms, after some hesitation.

Dr. Beard took the floor. His remarks were not of such a nature as to permit the reporter to follow him, but

he read an extract from the *Journal of Mental Science*, which he claimed showed the awakening interest manifested by Europeans in "unconscious states." The Doctor then wandering off to the fall of a Swiss mountain and to Astronomy, was called to order, and subsided.

Dr. Spitzka, without desiring to introduce personalities into the discussion, remarked that it was a pity the preceding speaker had not turned back a few pages in the *Journal of Mental Science* and read the extract relating to the collapse of Dr. Beard's demonstration in England, and how Dr. Beard had failed to come forward with a paper he had announced before a scientific body. As to the paper read that evening, he regretted to say that instead of science being behind in its views on the question of alcoholism, it was the paper which was far from being up to the science of the day. He would call the attention of the reader to Magnan's work, in which he would find such of his cases as had the strongest semblance to reality, carefully described under the heads of alcoholic stupor and alcoholic epilepsy. As to the hack-driver's case, that was an evident example of a well-established and well-known form of disease, namely—alcoholic paralytic dementia. He was surprised to find such a common manifestation of alcoholism as tremor reported absent by Dr. Crothers. He was still further surprised to find such ordinary everyday and characteristic symptoms of chronic alcoholism as delusions of marital infidelity, morbid suspicion, inconsistencies of behavior, stupor and amnesia erected into trance-like states. Nowhere in the paper did the author give any evidence that he made that distinction between Dipsomania, Chronic Alcoholism and Acute Alcoholic Delirium, which was the A B C of our knowledge of the subject. The speaker concluded by regretting that the first time in years that so important a matter was brought before the Society, it was brought forward in so imperfect a form, and coupled with a term "trance," which in the past history of the Society had certainly acquired no good odor.

Dr. Girdner endorsed the preceding speaker's remarks, and gave an analysis of the ordinary effects of alcohol on the mind, which he referred to dynamic interferences. He concluded by objecting to the acceptation of such views as Dr. Crothers advanced until they could be better substantiated, as their acceptation would involve some remarkable medico-legal consequences. He did not believe that alcoholism, aside from its effect in producing chronic insanity, should constitute an excuse for crime. He thought that a crime committed in a drunken excess should be punished like any other crime, because the person, by his own agency, put himself in a proper condition to commit such crime.

Mr. Eller, of the New York Bar, stated that the view last announced by the preceding speaker was not a sound one in law; it was certainly not the one entertained by lawyers. He alluded to the great injustice done by police justices in sending persons to the workhouse on the complaint of any two (possibly) conspiring persons, that such person was a "habitual drunkard." He thought that term required definition.

Dr. Crothers, in closing the discussion, among other remarks of a general character, stated that our knowledge of alcoholism was not at all perfect, and that his views were an addition to science, notwithstanding what had been alleged that evening.

M. PICKET has examined seven varieties of steel (chiefly from a Sheffield and a Vienna house) with regard to magnetic power *Arch. des Sciences*, August 15). This power he finds to depend on the presence of carbon in the iron, and the aggregation of these substances. One of the two steels giving the best results had ⅞th per cent of carbon. Samples with 1½ and 1⅛th per cent were inferior.

NEW YORK ACADEMY OF SCIENCES.

October 24, 1881.

SECTION OF PHYSICS.

Vice-president, Dr. B. N. Martin, in the Chair.

Thirty-one persons present.

Mr. W. Le Conte Stevens read a paper, of which the following is an abstract.

WHEATSTONE AND BREWSTER'S THEORY OF BINOCULAR PERSPECTIVE.

For some time after the publication of Sir Charles Wheatstone's essay (1) in 1838, on the Physiology of Vision, this subject was studied with much zeal by Sir David Brewster, whose name is permanently associated with the lenticular stereoscope, an instrument now familiar in every household. Although the theories advanced by these two physicists to account for the illusion of binocular relief have since been shown insufficient, their mode of accounting for the estimate of distance as perceived in the stereoscope has been quite generally accepted. In 1844, Brewster published an essay (2) "On the Knowledge of Distance given by Binocular Vision," in which he elaborated and abundantly illustrated the idea that the apparent distance of an object is determined by the intersection of visual lines. The stereoscope had already been explained as an instrument by which rays of light from two slightly dissimilar pictures were made to enter the eyes, as if coming from a single object into which they are combined in front, and on each point of which the visual lines could be made to meet. Thus, in Fig. 1

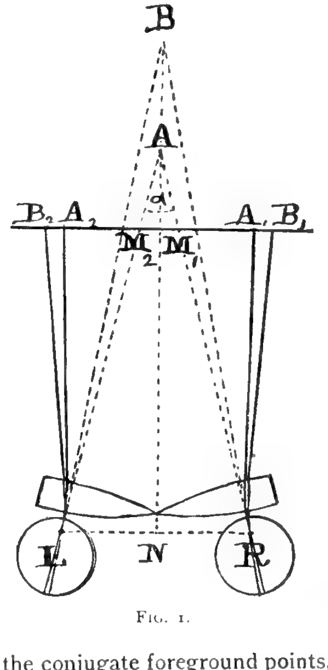

FIG. 1.

if rays from the conjugate foreground points, A_1 and A_2, be deviated by the semi-lenses, they appear to have come from A. In like manner, the background appears at B. If i = interocular distance RL., and a = optic angle, then for the distance of A we have

$$D = \tfrac{1}{2}\ i\ cot\ \tfrac{1}{2}\ a$$

From this formula it is obvious that D ceases to have any positive finite value when the visual lines cease to converge.

If the semi-lenses be taken away, and A_1 and A_2 be

(1) Phil. Transactions, 1838, Part II. Reprinted in Phil. Magazine, s. 4, vol., III., April, 1852.

(2) Edinburgh Transactions, vol. XV., Part III., p. 360.

removed to M_1 and M_2 respectively, while the convergence of visual lines remains unchanged, the images still appear at A and B. Wheatstone seems to have been the first to show experimentally that the illusion of apparent solidity can be obtained in this manner from a pair of projections representing the same object from slightly different points of view. If the eyes be properly trained, the visual lines may be directed to points whose distance is greater or less than that of the objects regarded at the same moment, and Brewster described many striking illusions thus obtained without the aid of the stereoscope. The principle applied by him, as described in the paper to which reference has been made, may be briefly given, and his results can be easily tested by anyone who is accustomed to analyzing his own visual sensations. Upon a uniform horizontal surface (Fig. 2) let two lines, A C and B C, be drawn, forming a small angle, β, with its vertex toward the observer. Let the eyes, R and L, be placed above this. If they be directed to the point, C, this appears in its true position. If the right eye be directed to B and the left to A, the axes meet at P; this point Brewster calls the binocular centre; and since the retinal images of B and A correspond, the visual effect is that of the union of these two external points at the binocular centre. Sweeping the glance toward C, the two lines appear united in the air, and P C is the apparent position of the combination, intermediate in direction between two monocular images, which may be disregarded or hidden from view with screens. If the convergence of visual lines be now diminished, the binocular image is lost until the right eye becomes directed to A and the left to B. The two points appear united at P′, and the line P′C now appears in the air on the further side of the surface. If the convergence be increased till P is again the binocular centre, and the face be lowered and withdrawn till the eyes are at R″ and L″, then C P″ becomes the position of the variable external image. And if lowered until R″L″ coincides with the surface, C P″ vanishes at the moment of becoming coincident with the prolongation of G C, the median of the triangle A C B.

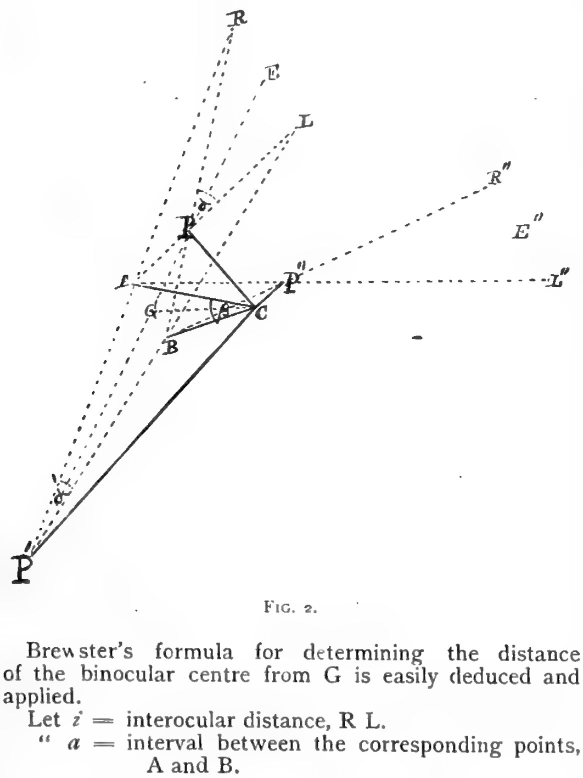

FIG. 2.

Brewster's formula for determining the distance of the binocular centre from G is easily deduced and applied.

Let i = interocular distance, R L.

" a = interval between the corresponding points, A and B.

Let b = distance, G E, between card and observer.

" x = distance G P, or G P′, which is positive when measured toward the observer, negative in the direction opposite. Then, observing the usual rule of signs, we have, by Geometry,

$$x = \pm \frac{a\,b}{i \pm a}$$

Applying this formula, Brewster constructed a table of distances for the binocular centre. For negative values it is seen that x becomes infinite when the visual lines become parallel; and, if they be slightly divergent, the binocular centre is far in the rear of the observer. Either of these conditions would make binocular vision impossible if the theory be correct. In testing the experiment with trained eyes, it is found quite possible to secure binocular fusion of the images of A and B when the interval between these points equals or slightly exceeds the interocular distance. It is also found that fusion of the images of the whole line at any given instant is impossible, especially when the angle β is large, or the lines are viewed very obliquely, as from R″ and L″. If the images of A and B fall on corresponding retinal points, the resulting sensation is binocular fusion, whether the visual lines be convergent, parallel or divergent; and the images of any two points nearer or farther apart cannot fall on corresponding retinal points at the same moment with those of A and B, though small differences are easily neglected. Whatever may be the importance therefore of optic convergence, as a factor ordinarily in determining the binocular judgment of distance, it has no such exclusive and measurable value as that attributed in Brewster's experiments; and the apparent distance of objects viewed through the stereoscope is obviously not determined by intersection of visual lines, if conditions are such as to render these parallel or divergent. The visual effects of optic divergence can be more conveniently studied by using stereographs than by the method already described, and a modification of Wheatstone's reflecting stereoscope affords the best means of measuring variations of the optic angle. As the lenticular stereoscope, however, is now almost universally employed, it is important that this instrument, as found in the market, be examined first.

By diminishing the natural convergence of visual lines, the stereoscopic effect of binocular relief can be quite easily obtained, while gazing upon a stereograph, without any instrument, when the interval between corresponding points of the two pictures does not exceed that between the observer's optic centres. This distance does not often differ very much from 64 mm., which may be taken as an average value. In Fig. 3 the distance between the two central dots is 50 mm. If the reader will fix his gaze upon a point ten feet off, just visible below the edge of the page, and then suddenly raise the visual lines to the figure without changing their convergence, he will see three circles instead of two; the central one moreover will appear as the base of a cone whose vertex is pointed toward him, but capped with a small circle. A little attention then will reveal the fact that when the dots are seen distinctly and singly, the small circle is double and slightly indistinct, and *vice versa*.

On stereographs, however, the interval between corresponding points is always greater than 50 mm. As the result of measurement made upon the foreground intervals of 166 cards, European and American, taken at random, the mean value I have found to be 72.9 mm., the maximum being 95 mm. If binocular combination is secured without the stereoscope, therefore, optic divergence is nearly always necessary. To ascertain the extent to which this is counteracted by the semi-lenses of our best stereoscopes, 30 pairs of these were kindly loaned me by Mr. H. T. Anthony, of New York. With very slight variation, their focal length was found to be 18.3 cm., and their deviating power not sufficient to prevent the neces-

sity of optic divergence, when the pictures are binocularly regarded through them, if the stereographic interval exceed 80 mm. As this limit is not unfrequently exceeded, optic divergence is often practiced unconsciously in using the stereoscope. Every oculist is familiar with the mode of using prisms to test the power of the muscles of the eyeballs, for both convergence and divergence of visual lines, and knows that 4° or 5° of divergence is not uncommon. Helmholtz (3) refers to the use of stereographs for the same purpose.

But familiar as is the production of optic divergence by artificial means, little or nothing seems to have been written in regard to the modification which the possibility of it imposes upon the theory of binocular perspective held by both Wheatstone and Brewster, accepted by most writers on vision since their time, and abundantly reproduced in our text books on Physics.* Of these I have not been able to find one that gives any account of the stereoscope except on the hypothesis that the visual lines are made to converge by the use of this instrument. On the uncertainty attached to the judgment of absolute distance from convergence of visual lines alone, Helmholtz (4) has written more fully than any one else. It is unfortunate that no English translation of his masterly work on Physiological Optics has ever been published. Although he gives no analysis of the visual phenomena produced in binocular fusion by optic divergence, his discussion of the judgment of distance would certainly tend to cast some doubt upon the explanation of vision through the stereoscope, as found in our text-books. And yet Helmholtz himself employs Brewster's theory in his mathematical discussion (8) of stereoscopic projection. This discussion, on the data assumed, is a model of elegance; but it contains no provision for divergence of visual lines. It is strictly applicable to the conditions involved in taking photographs with the binocular camera, and to the projection of images viewed in the stereoscope when the convergence of visual lines is identical with that of the camera axes, but not otherwise. Instead of human eyes we may assume a pair of camera lenses, an interocular distance apart, and a pair of sensitized plates behind them. Helmholtz's formulas enable us to determine the stereoscopic displacements in the images projected. If proofs from the negatives thus obtained be inverted and placed in front of a pair of eyes in such manner that the visual lines passing through corresponding photograph points shall bear to each other the exact relation that existed between the secondary camera axes that terminated in them, these two points will appear as one, and nearly at the distance of the real point in space to which the camera axes were converged. The effect is much the same as if the eyes, with normal convergence of visual lines, had been substituted for the cameras. But if the proofs be too near together or too far apart, increase of convergence makes the whole picture seem nearer, while divergence makes it farther. The relation between the different parts having been fixed at the time the picture was taken, increased convergence makes the distance from background to foreground seem less, divergence makes it greater. No one can have failed to notice the gross exaggeration of perspective often seen in the stereoscope, when the pictures are so far apart as to make the visual lines parallel or divergent, while the angle between the camera axes, when they were taken, was relatively large. But in no case do these conditions cause variations of such magnitude as Brewster's theory of binocular perspective would demand. This is easily illustrated with Wheatstone's reflecting sterescope. (5) Suppose the stereograph to represent a concave surface with the opening toward the observer, and that the arms of the instrument are properly adjusted. If they are pushed back, so as to make the visual lines divergent, the cavity apparently recedes and deepens; if pulled forward, so as to make them strongly convergent, it seems to approach and grow shallow. The apparent diameter of the image enlarges in the first case and diminishes in the second. Wheatstone notices this last variation in the account which he gave of his invention and its applications, in 1852, in the Bakerian lecture before the Royal Society (6); but, strange to say, the variation which is produced in apparent distance and depth under the same conditions seems to have escaped his notice, and the possibility of using his instrument to test the peculiarities of binocular vision with divergence of visual lines, seems not to have occurred to him. For the refracting stereoscope, however, like Brewster, he constructs a table of apparent distances corresponding to various optic angles, and applicable in using the binocular camera for the purpose of taking slightly dissimilar pic tures of the same object. He adds, (7) "when the optic axes are parallel, in strictness there should be no difference between the pictures presented to each eye, and in this case there should be no binocular relief; but I find that an excellent effect is produced, when the axes are nearly parallel, by pictures taken at an inclination of 7° or 8°, and even a difference of 16° or 17° has no decidedly bad effect. There is a peculiarity in such images worthy of remark; although the optic axes are parallel, or nearly so, the image does not appear to be referred to the distance we should, from this circumstance, suppose it to be, but it is perceived to be much nearer." This would not have seemed anomalous to Wheatstone, had he supposed binocular vision possible with divergence of visual lines, and entered into an analysis of the resulting visual phenomena. This analysis will be given in a future paper.

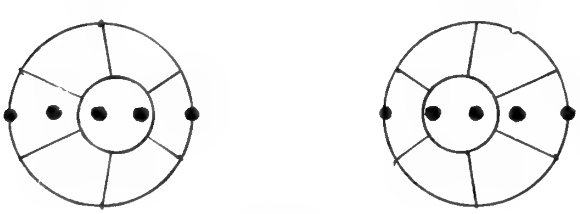

FIG. 3.

THE WATERS OF PARIS.

IN one of the previous numbers, *La Nature* gives an account of the work of an English observer, Mr. J. Hogg, on the waters of London. But since 1850, Mr. Hassall[a], at the request of the inhabitants of London, examined the degree of purity of the potable waters of that city, and more recently, Professor Farlow, of Boston, make an analogous work at the request of the citizens of that city.[b] M. A. Gérardin[c], however, has studied this question with a certain authority, by observing the cryptogamic vegetation in small streams of water which receive the waste products from the factories and manufactories on their banks. M. Gérardin observed that such industry favored the development of certain particular species which were

(3) Helmholtz, Optique Physiologique, pp. 616 and 827.

(4) Ditto, pp. 823, 828.

(5) For description see Phil. Mag., s. 4, vol., III., June, 1852, p. 506.

(6) Phil. Mag. s. 4, vol. III., p. 504.

(7) Ditto, p. 514.

(8) Opt. Phys., p. 842.

* Nov. 15th. Since the above was put in type, I have received from Prof. C. F. Himes, of Carlisle, Pa., an article written by him in 1862, in which he mentions his successful attainment of binocular vision by optic divergence, and criticises Brewster's theory of distance in relation to the stereoscope. Though his observation was independent, as my own was also, I find that he was preceded by a German, Burckhardt, in 1860 or 1861. I have already referred to Helmholtz in this connection (*Am. Journal of Science*, Nov. 1881, p. 361) and therefore have claimed no priority in discovering the possibility of this unusual, but still voluntary, employment of the eyes. It is the more remarkable that in our text-books the assumption should be so universal, that convergence of visual lines is a necessity in binocular vision for the determination of the apparent point of sight.

replaced by others in places where a different industry was in operation.

It is in this same order of ideas that the Administrator of Bridges and Roads of the Seine and Oise sent us lately a difficult question to answer. The question was to disinfect a river on the environs of Beaumont, which, during the heat of summer, was poisoned by a cryptogam of the genus Leptothrix. This vegetable is an aquatic and filaceous fungi which thrives and grows on the refuse of factories. The only answer to make was the well-known axiom: *sublata causa tollitur effectus.*

M. Neuville, the author of the memoir of which we are treating, is astonished that a work similar to that which he has undertaken, has not yet been done for the waters of Paris, whose sources are different, and which, consequently, do not present an equal degree of purity. Placing himself in a purely utilitarian point of view, he seeks to enlighten the administrator and the public on one of the indispensable sources of nourishment, that is to say the water consumed each day, by making use of his knowledge of cryptogams, and chiefly those of the family of Diatoms, for the study of which he had a predilection.

The complete purity of water is not always on account of the greater or less number of organic matters that it contains; it must be that these matters have a purifying character in themselves, and this condition obtains only when they are living vegetable growths, containing chlorophylle or green matter, having the property of freeing the oxygen which is dissolved in the water, on the one hand, and of absorbing, on the other, the carbonic acid gas which makes the water unfit to drink. Aquatic plants, then, are useful to the water which gives them shelter, yet with the condition that their bodies do not, by their accumulation, counterbalance the salutary effects.

Another cause very often acts to give water unhealthy properties. The lime salts which it holds in solution, and sometimes in great quantity, cause troublesome and sometimes serious diseases. The carbonate, but above all the sulphate, of calcium, makes the waters selenitic, as it is said, and then they become unfit for the cooking of vegetables, do not dissolve soap, and are indigestible. It is possible, however, for water to dissolve soap and nevertheless to cook vegetables; this occurs when in place of sulphate of calcium it contains magnesium.[d]

The waters of the source are often sought after for their limpidity and freshness, which does not always imply that they are really potable; it is even probable that if the waters of our rivers were taken at their source they would not have the qualities which they acquire after a journey of several miles. But when these rivers pass through an important manufacturing city, the injections, the impurities of all sorts which they receive will rapidly change their qualities; stocked, so to speak, in the passage by the city, they are yet able to improve themselves somewhat after a new journey in the country, which permits them to deposit the foreign matters which contaminate them. There are these elementary considerations which should always insist that the waters which supply a city should be taken above and not below the city.

Borrowing from the interesting works of M. Miquel, the savant of the Observatory of Montsouris, whatever they contain of use to his work, M. Neuville, among other things, reproduces the following table of comparison for the use of the reader, and shows what organisms the waters from the source and of different qualities can contain, observed with considerable magnifying power:

PROPORTION OF LOW ORGANISMS CONTAINED IN DIFFERENT WATERS.

	Microbs by hund. cube.
Water of condensation	0.2
Rain water	35.0
Water of the Vanne	62.0
" " Seine, in Paris	1200.0
Sewer water	20000.0

It is indispensable that analyses of this nature should be made rapidly, because in a short time the microbes grow in a great degree, and will no longer give exact figures.

The method which M. Neuville has followed does not consist of chemical analyses of the waters of Paris, which had been done long before by well-known scientists; it is rather a kind of statistics of the foreign matters contained in each of them, and which the use of the microscope reveals.

The waters submitted to his examination are those of the Marne, taken at Saint-Maur and Charenton; of the Seine, at the Port-à-l'Anglais, at the bridge of Austerlitz, taken at Chaillot, Auteuil, and at Saint-Ouen; the waters of the canal of Ourcq, of the Vanne, of the Dhuis, of Arcueil, of the sources of the Nord of Paris, of the artesian wells at Grenelle and Passy, finally that of a well of the left bank of the Seine.

A constant quantity of five litres of water was taken at the middle of the above-mentioned streams, or at the inlet of the waters of the sources; then after a settling of 12 hours, by means of an appropriate siphon, he decanted until it was reduced to 300 grammes. The contents were turned into a graduated gauge and after rest, by the use of a small pipe be gently raised the liquid in order to leave but a deposit of two to three centimetre cubes. It is this deposit which is directly submitted to microscopical observation, after having been put in cellular preparations before being preserved. Observation was made on a determined number of preparations, all sketched in a light room, and the sum of the latter serves to form one of the plates which accompany this article, which contains seventeen. A diagram, or schematic table, indicates the result of chemical analyses and of microscopical observations made for the Seine in its journey through Paris.

The water of the Marne taken at Saint-Maur, is relatively rather pure; yet organic matters are found in great abundance in it; but they are, above all, living matters, and more purifying than corrupting (Fig. 1.) They are a part of the Desmids in the green matter and belong to the genus *Pediastrum* (No. 9) or *Raphidium* (No. 10); or else of filaceous algæ (No. 11) of the genus *Ulothrix*. On the other hand, and much more abundant are the Diatoms with silicious carapace, and, for the most part, gifted with movements of translation very curious to observe. Such are the genera *Sirurella* (No. 1), *Nitzschia* (No. 2 and 3), *Cymatopleura* (No. 4), *Cynedra* (No. 5), *Diatoma* (No. 6), *Pleurosigma* (No. 7), etc., or even Infusoria (No. 13), which make prey of several of these small growths; but, above all, the organic remains. Finally the mineral matters which, in the water of the Marne river, are always more or less muddy, form the small chrystalline groups in the preparation (No. 15). This water is then very good; but on approaching its confluence, that is to say, at Charenton, not only the Diatoms are increased in number, but the Infusoria abound in it as well as detritus, which probably is due to the inhabitants of the Marne at this place.

In the water of the Seine taken at the Port-à-l'Anglais (fig. 2), a very great proportion of organisms is found. This point of the river, intended at one time to furnish Paris with a very potable water, has lost its value on account of the factories and manufactories which are built on it above the city, and the length of the small streams of water which it receives in these places. Desmids are found in it the genus *Closterium* (No. I), *Scenedesmus*

[a] A microscopic exam. of water . . . of London, 1850.

[b] Remarks of some Algæ found in the water . . . of Boston, 1877.

[c] Rapport sur l'altération, la corruption et l'assainissement des rivières, 1873.

[d] Translated from *La Nature*.

(No. 2), *Raphidium* (No. 9), *Tetraspora* (No. 10), and other Algae as *Chlamydococcus* (No. 11) or *Ulothrix*. Infusoria equally (*Chætonotus* [No. 20], *Brachytous* [No. 23], *Euglena* [No. 12]) and several species of Diatoms already named or a little different. But that which is striking is the already notable quantity of organic remains, fragments of plants (No. 24), mycelium of mushrooms (No. 18), and an eel (No. 17), moving about in the middle of all this multitude, and, in fig. 2, crossing a spicule of Spongille (No. 15).

The water taken at the bridge of Austerlitz is more charged with detritus; the purifying algæ disappear and diatoms are rare. It is infusoria and their bodies which predominate, then finally there are remains of tissues of linen or cotton, of vegetables in decomposition, etc., which the waters of Bercy, of Rapée and Bièvre brought.

If now the water of the pump of Chaillot is examined (fig. 3), the maximum contamination of the Seine is attained. (If this is not, however, the water taken at Saint-Ouen; but at this place the stream has received, as is known, the sewer collector at Asnières, which is a serious cause of infection of the water for a long distance.) The quantity of carbonic acid has already increased, and the oxygen grown less.

In the Chaillot water (fig. 3) there are scarcely any more algæ, here and there remains of conferva (*Cladophora* [No. 14] or some rare specimens of more reduced species (*Pandorina* [No. 1]), *Chlamydococcus* [No. 5], *Sirurella* [No. 2], *Stauroneis* [No. 3], *Epithemia* [No. 4]), yet most often there is only the silicious carapace, void of endochrome, of these last diatoms. On the other hand, the deposit of detritus is dominant in it: muscular fibres, (No. 10), vegetable cellules (No. 11), then myceliums of inferior fungi, and lastly eels (No. 9). Yet there is still more: some microscopic crustaceans appear (*Daphnia pulex* and others [Nos. 7 and 8]), the whole associated with earthy or indeterminable remains.

M. Neuville disputed the opinion of M. Frankland who refuses to admit that rivers purify themselves. He maintains, which seems to be perfectly demonstrated, that after a tranquil journey without receiving other impurities, a river purifies itself, little by little, by deposition, either on the bottom of the bed, or on the banks, of the substances which it held in solution. The harmful matters held in solution are eliminated and they disappear; the carbonic acid becomes exhausted little by little and the oxygen, on the other hand, returns in proportion.

During the war of 1870–71, the forced stopping on the rivers which habitually received the injections of factories and of industries polluting these streams, had changed their conditions. Thus the Bièvre, tainted from Arcueil to its arrival in the Seine at Paris, had again become limpid, and fish had made a home in it, coming from the parts above Arcueil, although however the bottom of the bed of the Bièvre was at this moment covered by a thick bed of mud nauseating at ordinary times.

The water of the Seine at Saint-Ouen became again more impure than any other part; but, as has been said, this was due to the sewer collector of Asnières. The carbonic acid which, at the bridge of Austerlitz, was represented by 16.2 by hund. cube, reached here the proportion of 65, that is to say the maximum for the waters of Paris. Living creatures here are very rare; but the remains of stuffs: linen, yarn, cotton, animal and vegetable fibres abound here. It is no longer a deposit, it is a real soup. The chlorides, the sulphates, and the sal ammoniacs, as well as sulphuretted hydrogen, are largely represented here. Finally further on, where the Seine passes by St. Denis and receives from it some quantity of refuse, vegetation is no longer found in it, and the bottom of the stream is nothing but a blackish and contaminated mud, as M. Gérardin observed it.

The canal waters of Ourcq have been for a long tim the principal source of the supply of Paris. They ar selenitic; but yet M. Neuville does not consider them the worst waters used in the city. A great number of algæ live in them, this being in their favor, and their relative tranquillity is also one of the causes of this notable growth of vegetation. The numerous boats which sail on these waters and the warehouses bordering this canal, where unloading is done, must prove sources of pollution of these waters? Their hydrometric degree is 30° to 31° while the mean of the waters of the Seine is between 17° and 20°, but this does not constitute in the eyes of M. Neuville a plausible reason of inferiority for the waters of Ourcq. It is their stagnation which ought to be the dominant cause of their depreciation.

Figure 4 represents the composition, in organisms, of the waters of the Vanne. It is, according to microscopical observations as well as according to the official reports, the best of the waters of Paris. M. Belgrand says: "comparison was not possible between the excellent waters of the Vanne and those of the Seine and the Ourcq, which, warm in summer, cold in winter, agitated and not transparent in all seasons, are besides tainted more and more by the residue from industrial and human refuse. After a journey of 176 kilometres by means of closed inlets, the waters of the Vanne arrive at the reservoirs of Montsouris and can furnish 100,000 metres cubes of water in 24 hours. Limpid, fresh, exempt from organic matters, it unites all perfections, says M. Neuville, it is water that I wish to see distributed in all Paris."

Several algæ of good quality, such as *Ulothrix* (No. 7), *Melosisa* (No. 2), *Meridion* (No. 1), *Navicula* No. 3), and *Synedra*, are the respectable inhabitants of these waters; here and there some vegetable growths and earthy remains, and finally some crystals of carbonate or sulphate of calcium (No. 11).

The waters of the Dhuis (fig. 5) only arrive at the reservoirs of Ménilmontant, associated with those of the Surmelin. At their point of junction, those of the Dhuis appear rather pure but nevertheless they are agitated and have need of rest. It is a remarkable thing that the microscope reveals no algæ in them; on the other hand, organisms of another order are met with, some filaments of Mucorinées (No. 1), mycéliums (No. 2), and some earthy or organic remains (Nos. 4, 5, 6). According to all appearances, these waters are not sufficiently oxygenated, and a journey a little prolonged in the open air and in the light will make them perfect.

Rivalling with the waters of the Vanne for purity and freshness, the reputation of the waters of Arcueil has been known for a long time (fig. 6); they were so named because they flow through the valley of the Bièvre over an aqueduct at Arcueil. But in truth, they have come from the village of Rungis since the Roman epoch, for these are the waters which supplied the baths called baths of Julius Cæsar or thermæ of Cluny. To the Roman aqueduct, of which several traces remain, succeeded that which can now be seen, built by Desbrosse in 1624, which has to conduct the waters of Rungis to the palace of Luxembourg and in the Saint-Jacques quarter. Advantage is taken of this monument, solidly constructed, to build above it the new aqueduct for the waters of the Vanne, which are conducted to the basin of Montsouris. M. Neuville says "with difficulty can be seen in them, several Vorticelles and Oscillaries (Nos. 1, 5, fig, 6), then several diatoms (*Gomphonema* [No. 3], *Nitzschia* [No. 2]) and lastly some salts of calcium, chrystallized and precipitated from their solution by the loss of a portion of carbonic acid. It is water of good quality, which can be improved by means of successive cascades, and it is unfortunate that we have not more of it.

The sources of the Nord of Paris offer but little interest. Given up to the consumption of the inhabitants of Paris from the end of the twelfth century, they were for a long time the only waters which supplied the fountains of the capital. These, with the waters of Arcueil are the most ancient known. They are furnished by

FIG. 1.—Water of the Marne at St. Mauer.

FIG. 2.—Water of the Seine at Port-à l' Anglais.

FIG. 3.—Water of the Seine at Chaillot.

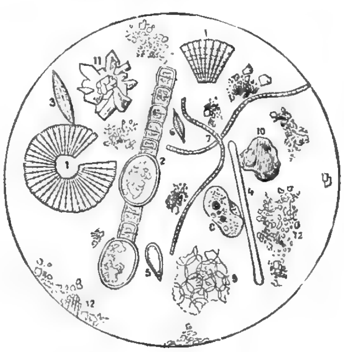

FIG. 4.—Water of the Vanne.

FIG. 5.—Water of the Dhuis.

FIG. 6.—Water of Arcueil.

FIG. 7.—Water of the Well of Grenelle.

FIG. 8.—Water of the Artesian Well of Passy.

THE WATER SUPPLY OF PARIS.

Saint-Gervais and the heights of Belleville. Their quality leaves much to desire.

"Essentially selenitic, their hydrometric degree reaches enormous proportions: 100 degrees to 150 degrees according as the waters of Saint Gervais and of Belleville are more or less mingled." They contain very little organic matters, some rare algæ and many calcareous crystals.

The well of Grenelle, become celebrated, was commenced in 1833 and only finished in 1841. It was on the occasion of its boring that the relations of the elevation of temperature to the depth of the soil observed by Arago and Walferdin, were stated; that is to say, with every 32 metres the temperature rises one degree.

The water of the well of Grenelle (fig. 7) lacks oxygen which it can gain in the basins of the Pantheon where it is conducted. This water is limpid, indicates at its outlet about 28 degrees, and is very nearly pure from all organism, since it only contains some traces of mycélium of mushrooms (No. 2) and, here and there, several Diatoms (No. 1) probably drawn from the tubes through which it flows. The traces of sulphuretted hydrogen which it contains, impairs a little the quality of this water, but not enough to make it unfit for consumption.

The water of the well of Passy, perforated from 1854 to 1861, can be compared with that of Grenelle (fig. 8). The results which the piercing of this well caused are known; the sale of the water of Grenelle was made considerably less. The carbonate of lime, as well as the bicarbonate of potash, are abundant in both. All things equal, moreover, M. Neuville gives his preference for the waters of Passy rather than for those of Grenelle. Several algæ are found in them (*Calothrix* [No. 8], *Rhizoclonium* [No. 5], *Cosmarium* [No. 1]. Glœocystis [No. 9]); several encysted infusoria (No. 2), and a few unimportant organic remains.

Lastly, the waters of ordinary wells are potable only when they are not found in a great city. The varied infiltrations, the connection with the soil, and the industries which can contaminate them, are considerations which must be taken into account in deciding the quality of the waters of wells, and which can render them unfit for consumption. They contain at Paris much of the nitrate and sulphate of lime, and they are also very much charged with organic matters in a state of decomposition; but of algæ there is no trace and there are, here and there, several microscopic crustaceans. It is above all in the neighborhood of cemeteries that the waters of wells should not be used except for gross purposes. This advice is upheld by the studies of M. Belgrand, who has observed that, in the environs of Père-Lachaise and of the cemetery of Montparnasse, the waters of wells were stocked, above all during the heat of summer.

Basing his results on microscopical analyses, M. Neuville arranges the waters of Paris in the following order to indicate their degree of purity: 1, waters of the Vanne; 2, of the Marne at Saint-Maur; 3, of the Marne at Charenton; 4, of the Seine at Port-à-l'-Anglais; 5, of the canal of Ourcq; 6, of Arcueil; 7, of the sources of the Nord; 8, of the wells of Passy; 9, of Grenelle; 10, of the Dhuis; 11, of the Seine at the bridge of Austerlitz; 12, of a well on the left bank; 13, of the Seine at Saint-Ouen; 14, at Auteuil; 15, at Chaillot.

TIDAL POWER AT BRISTOL.—At a recent meeting of the Town Council of Bristol a motion was brought forward, but not adopted, that "instructions be given to the sanitary authority to cause inquiries to be made into the tidal power of the Avon with a view to its being utilized for working electric lights for the city, the storage of motive power and other purposes, and that scientific aid be employed for the purpose."

THE EVOLUTION OF FLYING ANIMALS.

BY CHARLES MORRIS.

Continued from page 536.

Yet there is one instance of a leaping animal in which a partial flight has been gained in this manner. We allude to the flying-fish. Whatever first induced this creature to spring from the water through the impulse of its swimming motion—whether the pursuit of enemies, or some other cause—at any rate its fore limbs were already developed into wing-like organs, through their use as fins. The flying-fish does not really fly. But an increased spread of its supporting fins, which act as parachutes, would enable it to make longer leaps, and natural selection has undoubtedly produced this extension of the fins.

Land animals present us with several instances of this parachute motion. And significantly it never arises in earth-leaping, but always in tree-leaping animals. Among mammals we find three instances of such a habit, in widely separated families, embracing the Flying Squirrel, the Flying Phalanger, and the Flying Lemur. Among reptiles there is one instance, the Flying Dragon. The three mammalian genera mentioned include a number of species, and an imperfect flight is gained in the same manner in every case. During their so-called flight the limbs are extended almost at right angles to the body, and the skin of the sides has been developed until it is expanded into a broad membrane between these limbs, which, in the case of the Flying Lemur, extends from the nape of the neck to the tail. In their bold leaps from the branches of trees these creatures are partly supported by their membraneous wings, so that they descend slowly and easily. Some of them can even slightly vary the direction of their motion, so as to pursue insects.

The flying reptile, the little Draco Volans, gains its support in a somewhat different manner. In this case the extended membrane is supported, not upon the limbs but upon the false ribs, which grow out horizontally from their vertebral connection to a considerable distance, giving the animal a wing-like expansion of its sides.

We may readily conjecture the method in which such an organization was gained. The smaller tree dwelling animals are exposed to attacks from foes, the same as all other animals. Or, if carnivorous, they need to pursue their prey. In both these cases the power to make long leaps from branch to branch, or from tree to tree, is so obvious an advantage, that it is not surprising that many animals have become very bold and skillful in this particular. Many of these animals have also the habit of crouching on the branches of trees for concealment, their legs being extended side-wise, and their bodies flattened. This position of the legs in rest would most probably be retained during the leaping motion in which they are not employed. If, then, in any case, the width of the body should be increased, as by a chance extra expansion of the skin of the sides, supported by the outstretched limbs, the animal would be borne up by the air, and could make a longer leap. Such a conformation would aid it in flight or pursuit, and natural selection must operate to retain any such special advantage of form. It certainly seems very probable that the supporting membrane of these creatures was thus developed, and that the Flying Dragon gained its rib expansion through a similar process.

These cases may seem of little importance in an investigation of the origin of the flying power in birds; yet they are, in reality, of considerable importance. They point significantly to the most probable method of flight development, namely, as the result of an original leaping habit, from branch to branch or from tree to tree. Although the above cases are instances of parachute motion only, not of true flight, yet we have strong reason to believe that the earliest flying animals gained their power of flight as a direct extension of the above method.

In the incessant battle for existence among animals the possession of powers of flight are, in certain directions, so specially advantageous, that any accident of conformation, aiding animals in any degree to support themselves in the air, seems very likely to be preserved and to be added to. At present, however, when the field of air is so fully occupied, and there are so many vigorous carnivorous birds constantly seeking for poorly defended food animals, the tendency to develop powers of flight is checked. The aerial motion of the leaping animals mentioned is not such as to greatly expose them to danger from this source. But were their powers of flight increased, so that they could support themselves longer in the air, and move out from the immediate shelter of trees, they would be exposed to attacks from carnivorous birds, and such an imperfect flight must prove a special disadvantage. Their weak flight would expose them to a danger from which they could escape neither by rapidity of motion, nor by a return to the shelter of the forest. Thus the existence of these strong carnivorous birds operates as a decided check on the development of any other form of flight, and it is very unlikely that the leaping creatures mentioned will ever develop a more efficient flight.

There is another illustration of the same principle. The true flying mammals, the bats, perhaps developed their powers of flight at the same geological age as birds did. Thus there was no special hindrance to their evolution. It might seem curious to some, however, that they have failed to gain the same extensive habitat as birds; that they are exclusively crepuscular or nocturnal in their habits, while the birds are almost exclusively diurnal. And yet this difference must have arisen in consequence of a long-continued conflict between bats and birds, in which birds conquered. If we consider that the flying organs of birds are more efficient than those of bats, the whole question is answered. The birds drove the bats back into a field which they did not care to occupy. The fight was for the possession of the air during the day. The birds conquered. The bats were forced to content themselves with nocturnal flight and the imperfect food-supplies which the night yields, while the birds proudly held the dominion of day. It was a case somewhat similar to that which we have already considered, of the supremacy of mammals over reptiles; and the disappearance of the flying Pterosaurs of the far past may possibly have been due, both to the superiority of flying powers and to the more efficient vascular system of birds over flying reptiles.

But there was a period in which the haired animal had not yet succeeded the scaled animal, in which hairs had not been specialized into feathers, and in which the broad fields of air were free to the first occupant, and contained no active carnivoræ ready to check the development of flight in its incipient stages. Were such the case now, our Flying Squirrels, etc., might gain more efficient powers in this respect, and evolve true flight. We may safely come to this conclusion from the fact that certain lizards gained powers of flight in the early days of bird evolution, and apparently by a direct continuation of the process which we perceive in the Flying Squirrels. The expanded skin between the legs was merely a thin, naked membrane in these lizards. It apparently spread wider and wider, until it extended to the extremities of the toes. But this condition would only produce a more efficient parachute motion. For flight to arise a movement of the fore limbs must be employed, and this may have originated in the effort to keep the body horizontal and prevent its anterior extremity from sinking. Such a movement, with the possession of such a membrane, would yield an imperfect flight. But for the flight to become perfect, in such an animal, the membrane must become still more extended, and the movement of the fore limbs more efficient. This extension was gained by the gradual outgrowth of the fourth finger, to whose tip the membrane had already extended. By the preservation of favorable variations in this direction, it is very likely that the immensely extended flying finger of the Pterodactyle, the bird-like conformation of its skeleton, and its probably more efficient lung action, were gradually gained; and that it thus developed from an original tree-leaping into a flying reptile.

The advantages of an aerial residence, at that early period when the atmosphere had not yet been preempted by vertebrate inhabitants, were so many and great, that the selective principle must have proved of particular efficiency in this direction. Even a long leap from a solid support infested by many enemies, through an aerial field devoid of enemies, was highly advantageous, and the development of flying powers may have been preceded by the appearance of many animals possessed of a parachute motion. Such animals would leave no geological record, as their motion did not necessarily modify the osseous framework of the body. There are comparatively few of them now, since the advantage to be gained by such powers is greatly reduced. But it is very obvious that any extension of this power, assimilating it to true flight, must have proved far more advantageous. An animal capable of supporting itself in the air, at this early period, was exceptionally free from danger, while its chances to obtain food were greatly increased. The better flyers were, of course, the more secure, and the original partial flight must have rapidly developed into the most perfect flight of which such a membranous wing extension was capable. Not until these flying reptiles began to prey on each other was there any check to their evolution and diversity of formation. The remains of some twenty different species have already been discovered, and this is indicative of hundreds, perhaps thousands, of diverse species of these strangely flying Pterosaurs.

A somewhat similar instance of early and perhaps general possession of the atmospheric field is afforded in the case of insects. Some peculiarity of formation, perhaps the remnant of an original dorsal gill, gave them a structural organ capable of partly aiding them in leaping and of being modified into a membranous wing. But as in vertebrates so in insects, the field of air became in time so fully occupied, and the claimants for its food supply so numerous, that the less vigorous flyers or those adapted to specially terrestrial food, descended to the earth again, and in time lost the power, with the loss of the desire or need, of flight.

Some such fate seems to have overtaken the Pterosaurs. For the possession of the field of air was sharply contested by two other classes of animals, both of which had already stepped beyond the reptilian rank, and become hair-covered and warm-blooded, and one had gained the still more advanced condition of the mammalian organization. The contest between birds, bats, and Pterosaurs, was not alone a battle of flying carnivoræ, but a struggle for common supplies of food, in which the most vigorous flyer was most likely to win. As a probable consequence of this contest the birds have gained the diurnal possession of the air, the bats have become adapted to nocturnal food supplies, and the Pterosaurs have disappeared. They were probably vanquished in the fight, and gradually resumed terrestrial habits, or died out.

So far as we are aware the evolution of flying mammals was much later than that of birds. But the geological record is so incomplete that bats may have existed at a much earlier date than their discovered remains seem to show. They are found in Eocene deposits, and the bird remains yet found in earlier deposits are very few in number of instances or species. In considering the development of bat flight we are in the same line with those already examined. It is a development of the skin into a flying membrane. But in this case, in addition to the extension of this membrane between the fore and

hind limbs, it is also extended between the fingers of the fore limbs. Possibly in the early flying efforts of bats the fingers were extended in such a manner as to render any accidental growth of skin between them an advantage. Differences in the locomotive habits, and in the foot development, of the progenitors of flying reptiles and flying mammals, may account for this greater extension of the flying membrane in the case of the latter. Naturally such an advantage was seized upon and improved by natural selection. The membrane extended to the tips of the fingers, the fingers themselves grew much longer, and in time the fore limb lost all its powers as a walking organ, but became developed into an efficient flying organ, Bats, by this specialization of their fore limbs, ceased to be quadrupeds, though they never became true bipeds. Sleep and flight constitute the measure of their existence.

Thus the development of all flying vertebrates other than birds pursued the same general course. It began with the extension of the skin of the sides, so as to serve for parachute motion; and ended with an extension of the fingers of the fore limbs, an extension of the membrane to the tips of these elongated fingers, and a flying motion of these limbs. In necessary connection with these were concomitant internal changes, the whole anatomy of the animal becoming adapted to its flying habits.

But in the development of bird flight quite a different mode of evolution appears. Flight is here attained not by a special adaptation of the skin, but of the dermal covering. This covering was probably not the hair in its full modern sense. It was a primitive derivation from the reptilian scale, which secondarily became the avian feather and the mammalian hair. The feather of the bird agrees with the scale of the reptile in being developed in little hillocks upon the skin. The hair of the mammal developed in closed follicles within the skin. There thus has been a specialization in both, with the production of a change in the terminal character of the feather and in the dermal origin of the hair.

The progenitors of birds were either land or tree dwelling reptiles; most probably the latter. We have seen the extreme improbability that any leaping motion from the earth developed into a flight. We have also seen how natural it is for animals to leap from the limbs of trees, and that, in several modern instances, a degree of aerial support has been thus developed. But in such a leaping motion it is highly probable that some animals would make other efforts for support in the air besides the horizontal extension of their limbs. The swimming motion is a very natural one. It is naturally adopted by all land animals which fall into the water, and the webbed feet of swimming birds have been produced by it in the same manner as the webbed fingers of flying bats. Let us now consider some early animal, so far advanced beyond the reptilian ranks as to have become warm blooded, and covered with the primitive form of hair, arboreal in its habits, and accustomed to make long leaps from limb to limb. It is by no means improbable that some such animals would seek to swim in the air. A rapid motion of the fore limbs could not but aid in keeping the body horizontal, and if these limbs were covered with thick hair this must aid in breaking the fall of the animal. Any such habit could have but one result. A thicker hairy covering of the fore limbs, and even of the whole body, would prove an advantage to the animal, and these thickly matted hairs would tend to spread laterally, precisely as we find in the tails of Flying Squirrels and Phalangers. A still further advantage would be gained were these hairs rough instead of smooth on their edges, so as to cling together, and prevent the air from passing between them.

Such a swimming motion, performed by the fore limbs principally,—the hind limbs being the leaping organs,—and aided by the lateral outgrowth, and the "felting" of rough edged hairs, would, from its inception, be more than a parachute motion. It would be incipient flight from the first, a swimming in the air. The essential advantages gained by longer and longer leaps must tend to preserve any favorable conditions of the hair, and we can readily conceive the rough edges of the hairs extending into interlocking feathery expansions. In fact it is not difficult to imagine the slow evolution of true feathers in this manner, since every incipient approach to the feather must prove advantageous to the animal in its aerial motion.

But every better adapted movement of the fore limbs must prove similarly advantageous. Not only any variation from hairs towards feathers would be advantageous, but also any variation from a swimming towards a flying movement of the fore limbs. Of course the process must have been a slow one. It was necessarily slow also in the case of the bat and the Pterodactyl. But in all these cases every increment of variation from leaping towards flying was advantageous, so that there was no hindrance to a continual evolution towards full powers of flight.

But the development of the fore limbs into feathered wings unfitted them more and more for walking organs. The slowly developing bird must have trusted more and more to its hind limbs for support. Its arboreal habit developed the toes of these limbs into grasping organs. The original quadruped in time became a true biped, with a foot specially adapted to grasp the rounded surfaces of the limbs of trees, and so changed in position as to fall under the centre of gravity of the body. Hairs became feathers, the bones of the fore limbs aborted in part and became wing bones, and the original tree leaping reptile became a flying bird.

We may close with a very brief further consideration. In the first place it is highly probable that only quite small animals first gained this flying habit. Considerable weight would hinder its development. But after it was once gained there would be no special hindrance to increase in size in the newly evolved species. Yet a very great increase in size would so greatly increase the muscular effort necessary to flight that the larger birds would most likely spend a considerable portion of their time upon the earth. And in many cases the increased weight which is apt to arise from diminution of muscular exercise might render a resumption of the flying habit impossible. Such birds would lose their aerial powers, and become true land bipeds. We may ascribe the land residence, and the aborted wings, of the Ostrich, Cassowary, &c., to some such secondary process of evolution. On the other hand, many virtually land birds have become so by an adaptation to food in the obtaining of which flight was no advantage. Organs not used soon lose their muscular vigor, their size decreases, and gradual abortion takes place, unless adaptation to some new function gives them a special development in this new direction, and checks their tendency to disappear.

II.

STONE IMPLEMENTS IN THE DRIFT.*

By Watson C. Holbrook.

Many stone implements have been found deeply buried in the clay and gravel of Whiteside county, Illinois. Mindful of the many sources of error, and fully conscious of the many grave and serious questions involved, I have endeavored to examine with care and attention every one of the finds. The first is a black chest spear head about five inches long found incased in a block of granular stalagmite. This specimen was found in a light-blue clay. Above this clay was an alluvial deposit about five feet thick. Some pre-historic man must have left his spear head in a cave or hid it in a fissure of rock. Layer after layer of stalagmite was found. The spear's head

* A. A. A. S., Cincinnati, 1881.

was first covered by this incrustation of limestone, but in the course of time was completely buried in the thin, ribbon-like layers of this stalagmite. Then the floor of the cave was broken up, and the detached piece containing this specimen was carried here by water, or ice, or both, and here it has remained imbedded in this blue clay till all of the alluvium has been deposited. Several arrow-points have been found buried seven or eight feet below the surface of the earth. I have carefully examined two of these finds. They were buried in a tough, compact clay. They were found by workmen while cutting into the hillside and grading the public roads. A small arrow point was found by a friend of mine while digging a well. It was twenty-four feet below the surface of the earth. It is a well-made and beautiful arrow-point, and my friend will not part with his valuable specimen.

FRENCH ACADEMY OF SCIENCES.

August 8, 1881.

MINERALOGY.—M. Klein presents a communication on different solutions of very great density, which can be advantageously utilized in laboratories to separate pulverulent mineral particles from foreign bodies. The salts employed by M. Klein are the tungstoborates of cadmium, nickel and cobalt. The density of the solutions of the last two salts is 3. 4; yet M. Klein prefers to them the solution of tungstoborate of cadmium, whose density is only 3. 2, but which is quite transparent, while the others are very colored. The tungstoborate of cadmium can, besides, be obtained in crystals; it melts at a temperature of 75°, and becomes a transparent liquid, whose density is 3. 6.

PHYSICS.—M. Ancelin described a method of heating intended to replace foot-warmers of water. His system is based on the fact that every body which passes from a liquid to a solid state gives off its latent heat of fusion.

M. Ancelin encloses some acetate of soda in a metallic vase, which is then heated to a temperature of about 80°. Then left to itself, the apparatus cools little by little to about 59°; the acetate of soda then commences to solidify, and gives off its latent heat. While the solidification continues, the vase remains at the same temperature. Boilers heated in this way will remain hot four times as long as by the use of water, about twenty to twenty-two hours.

EXTRACTION OF SULPHUR.—M. Dubreuil, who has devised a new method for extracting the sulphur of Sicily, announces that he has found in the mother waters of the salt marshes of Palermo, charged with chloride of magnesium and boiling at 120°, a suitable substance to separate from the sulphur the earthy bodies which accompany it.

FOR the unities of electric measures there are adopted the fundamental unities—centimetre, gramme, second, and this system is briefly designated by the letters C. G. S. The practical units, the *ohm* and the *volt*, will retain their present definitions; the ohm is a resistance equal to 10^8 absolute unities (C. G. S.), and the volt is an electromotive force equal to 10^9 absolute unities (C. G. S.). The practical unit of resistance (ohm) will be represented by a column of mercury of 1 square mm. in section at the temperature of 0° C. An international commission will be charged with ascertaining for practice, by means of new experiments, the height of this column of mercury representing the ohm. The name *ampère* will be given to the current produced by the electromotor force of 1 volt in a circuit whose resistanse is 1 ohm. *Coulomb* is the quantity of electricity defined by the condition that in the current of an ampère the section of the conductor is traversed by a coulomb per second. *Farad* is the capacity defined by the condition that a coulomb in a condenser, whose capacity is a farad, establishes a difference of potential of a volt between the armatures.

COMET (*g*) 1881, SWIFT.

At eleven o'clock last evening, Director Lewis Swift, of Warner Observatory, discovered the seventh comet of the year in the Constellation of Cassiopeia in a line between Polaris and the great cluster in Perseus, a trifle nearer Polaris. It is nearly round, faint, has a slight central condensation, but no tail is yet visible. Its right ascension is one hour and fifty minutes, (1 h. 50 m). Declination north seventy-one (71) degrees, and its motion slow westward. Estimated diameter, about four minutes. As the comet of 1812 is anticipated from this quarter, it may be the great Pons Comet. This makes the sixth comet discovered in this country since May 1st, Swift getting the two hundred dollar Warner prize twice. The fifteen hundred dollars given in comet prizes during the past twelve months by Mr. Warner has evidently given an extraordinary impetus to astronomical study in this country. Director Swift, of the Warner Observatory, will visit Egypt, by the generosity of the founder of the Observatory, in December, 1882, to observe the total eclipse of the sun and verify his celebrated discovery of an intra-mercurial planet in 1878, which has been so much disputed by astronomers. C. S. WHITTLERE,

Sec'y. Roch. Astro. Society.

WARNER OBSERVATORY, ROCHESTER, N. Y., *November* 17, 1881.

COPYING INK FOR READILY TRANSCRIBING LETTERS WITHOUT A PRESS.

A paper on this subject by Professor Attfield, F.R.S., &c., was read at the last annual Pharmaceutical Conference at York, England. The author stated that for the past thirteen years all letters, reports, &c., that he had written had been transcribed into an ordinary thin-paper copying-book with no more effort than was employed in using apiece of blotting-paper. It had only been necessary to place the page of writing, note size, letter size, or even foolscap, in the letter-book, and use a leaf of the letter-book just as one would use a leaf of blotting-paper. The superfluous ink that would go into blotting-paper went on to the leaf of the letter-book, and, showing through the thin paper as usual, gave, on the other side of the leaf, a perfect transcript of the letter. Any excess of ink on the page, either of the letter or of the copying paper, was removed by placing a sheet of blotting-paper between them and running one's hand firmly over the whole in the ordinary manner.

This ready transcription was accomplished, as would be anticipated, by using ink which dried slowly. Indeed, obviously, the ink must dry sufficiently slowly for the characters at the top of a page of writing to remain wet when the last line was written, while it must dry sufficiently fast to preclude any chance of the copied page being smeared while subsequent pages were being covered. The drying must also be sufficiently rapid to prevent the characters "setting off," as printers term it, from one page on to another after folding.

The author then alluded to some difficulties attending the employment of the ink which had prevented its becoming an article of wholesale trade, but, he said, any chemist and druggist could make it and sell it, giving directions for use to customers. He himself had used it from year's end to year's end without any trouble whatever. It would be particularly useful to professional men and private persons.

The principle of the method of preparation consisted in dissolving a moderately powerful hygroscopic substance in any ordinary ink. After experimenting on all such substances known to him, he gave the preference to glycerin. Reduce, by evaporation, ten volumes of ink to six; then add four volumes of glycerin. Or manufacture some ink of nearly double strength and add to any quantity of it nearly an equal volume of glycerin.

SECONDARY BATTERIES.—J. Rousse.—In order to accumulate electricity for the production of light or motive power, the author has arranged secondary batteries, which differ from those of M. G. Plant. At the negative pole he uses a sheet of palladium, which, during the electrolysis, absorbs more than 900 times its volume of hydrogen. At the positive pole he uses a sheet of lead. The electrolysed liquid is sulphuric acid at 1-10th. This element is very powerful, even when of small dimensions. Another secondary element which has also given good results, is formed at the negative pole of a slender plate of sheet-iron. This plate absorbs more than 200 times its volume of hydrogen when electrolysed in a solution of ammonium sulphate. The positive pole is formed of a plate of lead, pure or covered with a stuatum of litharge, or pure oixide, or all these substances mixed. These metallic plates are immersed in a solution containing 50 per cent of ammonium sulphate. Another arrangement is at the negative pole, sheet-iron; at the positive pole a cylinder of ferro-manganese. The electrolysed liquid contains 40 per cent ammonium sulphate.

CONSTITUTION OF THE MILKY WAY.—When the milky way is regarded with an indifferent eye, it seems that its brightness is the same in all parts. But it is quite otherwise when the relative luminous intensity of its different portions is measured. It is then found that the milky way is composed of a series of luminous plates separated from each other by darker portions. Thirty-three of these nodules have been counted, the centre of which is more brilliant than the borders, and it is stated that they are arranged nearly mathematically along a great circle of the celestial sphere.

AN EXPLANATION.

To the Editor of "SCIENCE."

DEAR SIR,—In giving the specific rotatory power in my article "Amylose" in SCIENCE of Oct. 1st this year, I used the expression (a) to designate the specified rotatory power for the *teinte de passage* since that is the usual ray employed. On the other hand I used (a)j to designate the same property for the yellow ray, meaning by the yellow ray the monochromatic sodium flame.

Since, however, it is the usual custom to designate the "rose-purple" transition tint by (a)j as if it were a yellow ray and the sodium ray by (a) D, I desire to make this explanation of the symbols used.

Respectfully,

H. W. WILEY.

LAFAYETTE, IND., *Nov.* 5, 1881.

OBSERVATIONS AND RESEARCHES ON BLOOD-STAINS.—D. Vitaci—Attention has been recently called to a reaction discovered by Schœbein—the blue coloration produced by a mixture of oil of turpentine and alcoholic tincture of the resin of guiacum, on the additionof a little blood or a very dilute solution of hæmoglobin. It is said that this reaction is preferable to any other, not excepting that founded on the formation of crystals of hæmine and on spectroscopic observation, and that none of the substances capable of simulating blood-spots give the same opaque blue color. The author, however, shows that all substances capable of acting as direct or indirect oxidising agents are capable of producing the same reaction.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING NOV. 12, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

NOVEMBER.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 6	30.151	30.302	12 p. m.	29.988	0 a. m.	52.3	48.3	60	3 p. m.	52	3 p. m.	46	12 p. m.	43	12 p. m.	112.
Monday, 7	30.315	30.400	9 a. m.	30.252	12 p. m.	47.6	46.0	51	5 p. m.	50	12 p. m.	42	7 a. m.	42	7 a. m.	56.
Tuesday, 8	30.145	30.252	0 a. m.	30.100	12 p. m.	58.0	57.6	62	4 p. m.	61	4 p. m.	50	0 a. m.	49	0 a. m.	68.
Wednesday, 9	30.008	30.112	12 p. m.	29.910	4 p. m.	62.3	60.0	68	3 p. m.	65	3 p. m.	54	12 p. m.	49	12 p. m.	80.
Thursday, 10	30.245	30.296	12 p. m.	30.112	0 a. m	46.7	43.3	51	2 p. m.	45	2 p. m.	43	12 p. m.	40	12 p. m.	110.
Friday, 11	30.319	30.394	9 a. m.	30.222	12 p. m.	42.3	39.0	46	3 p. m.	41	3 p. m.	39	8 a. m.	37	10 a. m.	111.
Saturday, 12	29.801	30.222	0 a. m.	29.548	12 p. m.	51.3	50.0	60	8 p. m.	59	8 p. m.	42	2 a. m.	38	2 a. m.	62.

Mean for the week .... 30.140 inches.
Maximum for the week at 9 a. m., Nov. 7th .... 30.400 "
Minimum " at 12 p. m., Nov. 12th .... 29.548 "
Range .... .852 "

	Dry.		Wet.
Mean for the week	51.5 degrees		49.1 degrees
Maximum for the week, at 3 pm. 9th	68. "	at 3 pm 9th,	65. "
Minimum " " 8 am. 11th	39. "	at 10 am 11th,	37. "
Range " "	29. "		28. "

NOVEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 6	w.	n. w.	n. e.	188	5	1.00 am	.310	.269	.283	92	54	78	0	0	0	-----	----	-----	--	3
Monday, 7	n. e.	e. n. e.	e.	155	$2\frac{1}{2}$	5.00 pm	.267	.283	.321	100	78	86	8 cu.	9 cu.	10	7 pm	12 pm	5.00	.04	0
Tuesday, 8	n. e.	s. e.	s. s. e.	107	$1\frac{1}{2}$	0.15 am	.418	.505	.487	100	94	94	10	10	10	4 am 3 pm	8 am 5 pm	4.00 2.00	.10 .10	10
Wednesday, 9	s. s. e.	w. s. w.	n. w.	154	$4\frac{3}{4}$	10.00 pm	.487	.577	.409	94	84	82	10	9 cu.	8 cu.	4 am 4 pm	11 am 6 pm	7.00 2.00	.13 .02	6
Thursday, 10	w. n. w.	n. w.	n. w.	273	$7\frac{3}{4}$	1.30 am	.275	.220	.218	92	59	75	1 cir.	7 cir. cu.	4 cir. cu.	-----	-----	-----	--	0
Friday, 11	n. n. w.	n. w.	e.	177	$3\frac{1}{4}$	0.00 am	.216	.173	.195	90	60	68	3 cir. cu.	1 cir. s.	5 cu.	-----	-----	-----	--	2
Saturday, 12	e. s. e.	s. e.	w. s. w.	134	$6\frac{1}{4}$	6.00 pm	.231	.348	.487	83	93	94	10	10	10	$10\frac{1}{2}$ am	11 pm	12.30	.65	1

Distance traveled during the week .... 1,188 miles.
Maximum force .... 7¾ lbs.

Total amount of water for the week .... 1.04 inch.
Duration of rain .... 1 day, 8 hours, 30 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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SATURDAY, NOVEMBER 26, 1881.

THE SATELLITES OF MARS.

The outer satellite of Mars was seen here on Nov. 15th, and by an observation of Nov. 20th its position was

WASHINGTON, M. T.

		h.	*m.*	*p.*	*s.*
1881.	Nov. 20,	13	15,	71°.7,	45″.6.

This satellite is therefore near the predicted place. An hour later Phobos seemed to be visible, also near the computed position, but the sky had become a little thick and I could not be certain of seeing this satellite.

The planet will continue to approach the earth until December 21, and the satellites will become brighter. It is possible, therefore, that they may be observed for nearly two months during the present opposition.

A. Hall.

Washington, D. C., *Nov.* 22, 1881.

THEORY OF THE MOON'S MOTION.*

About a year ago the Vice-President of the Physical Section of our chief scientific association remarked, in his farewell address: "there are many subjects in astronomy that need investigation, but in most cases the labor required is very great, and the completion of the work would occupy a long time. * * * The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come." If persistent, painstaking, and conscientious effort have aught to do with such a matter, we must add to the list of distinguished lunar theorists, including Plana, Damoiseau, Hansen, and Delaunay, the name of Stockwell. We cannot say that his researches have yet met with that notice to which they are perhaps rightly entitled. Mr. Stockwell has published a number of monographs on many points connected with the lunar theory during the last six or seven years; and his works show great familiarity with, and expertness in, the involved computations of this sort of astronomical research.

If we may judge from the appearance of the pamphlet before us, Mr. Stockwell has now quite terminated his lunar investigations, and intends to complete the publication of his finished theory of the moon's motion at some early date. In his Introduction he has sketched the early historic development of the question with that explicitness which we should expect rather to have seen in some thorough elementary text-book; strangely, he devotes twice as much space to the ante-Newtonian aspect of the problem as to the most remarkable developments of the mathematical theory which have occurred since his time. He makes no mention of Damoiseau, who takes high rank not only among pure lunar theorists, but among the constructors of tables of the moon. His tables are well known to have been the first ever constructed from pure theory.

Though the age of the great lunar investigators is now gone, there are some very surprising results of Mr. Stockwell's "new method of analysis" to which the attention of the few theorists now working at the moon's motion might well be directed. He instances several comparisons of the values of his co-efficients with those obtained by Delaunay in his very refined development; in one case he obtains, by a rapidly-converging series of four terms, a result identically the same with that of Delaunay's series of seven terms; and remarks, "the four terms of my development are more accurate than the seven terms of Delaunay's, since the seventh term of the latter series is thirty times greater than the fourth term of the former." There is nothing new in the fact that the sum of a very small number of terms should come out equal to a very large series, but if theorists can be brought to acknowledge the essential accuracy of the "new method," Mr. Stockwell must no doubt be credited with effecting an enormous advance in mathematical astronomy. Mr. Stockwell has shown satisfactorily to himself the correctness and value of his method, and the facility of its application—he must now address himself to the equally difficult task of making others see it in the same light.

It seems a wholesale assertion on the part of Mr. Stockwell that there are "several terms of considerable magnitude in the theories of La Place, Plana, Pontécoulant and Delaunay, which are not functions of the disturbing force;" and we should, at first blush, be inclined to place much confidence in his demonstration that the general integral assumes the indeterminate form in special cases which occur in those theories. It is certainly a most important oversight, and leads us to believe that the lunar theorists who followed La Place would have done much better to have built up theories of their own with entire independence of what anyone else had done. It is a remarkable fact if this discovery has been left for Mr. Stockwell to make. He concludes: "if the computations of the present work are correct, astronomers have carried their approximations to terms of the *fifth*,

* John N. Stockwell, Ph. D. (Introductory.)

sixth, and *seventh* orders of magnitude, before those of the *third* and *fourth* had been correctly computed. This seems to be a sufficient reason for the nearly stationary condition of the lunar theory during the past three-quarters of a century, notwithstanding the great efforts which have been made to perfect its solution. Its advancement has been blocked by the obstacles thrown in its path by analysis itself; and we may therefore reasonably hope for substantial improvement in the theory and tables when they are no longer embarrassed with equations which have no existence in nature."

We may remark that there are two ways in which the correctness of Mr. Stockwell's conclusions may be tested: first, a mathematical expert competent to pronounce upon his theoretic processes should go over his work with the most searching criticism in every detail; and second, his theory should be compared with observations. But this latter would be a task of such immensity that no astronomer unassisted would hope for its completion.

NEW YORK ACADEMY OF SCIENCES.

Oct. 31, 1881.

The President, Dr. J. S. Newberry, in the Chair.

Twenty persons present.

The following paper was read by Mr. John H. Furman:

"The Geology of the Copper Region of Northern Texas and the Indian Territory."

The well-marked cretaceous beds of Parker County, Texas, extend for 30 miles north of west from Weatherford, on the road to Graham. They consist of strata of shelly limestone, sandstone and shaly clay, the latter grayish or reddish in color. An occasional thin seam of soft coal is found; and the water is strongly impregnated with lime. A stratum of sandstone stretches for thirty miles N. W. from Fort Worth. In this rock springs are found containing sodic carbonate, similar to the waters of the artesian wells of Fort Worth, Tarrant County, at a depth of about 270 feet. Towards Graham, the country assumes a semi-mountainous appearance, and, for twenty-five miles or more, sandstone ridges alternate with prairies, the hills being covered with scrub oak. Some of the ridges attain an elevation of two or three hundred feet above the prairies. The strata are horizontal, and large portions of the original surface have been carried away by erosion. The upper stratum is in many places a conglomerate, made up of small pebbles. In this region the seams of coal met with are generally soft, and the only workable bed known is one about three feet thick, yielding a fair quality of bituminous coal, which crops out and has been traced for several miles near the Clear Fork of the Brazos river in Young County. This supposed coal region has a general N. E. and S. W. direction.

Approaching Graham the prairies begin to resemble the plains; and the ridges, capped with sandstone, show bases of mottled reddish-colored shales, or clay; salt springs and salt streams are found, indicating the border of the great alkaline region. From Graham to Fort Griffin in Shackleford County, thence north in Throckmorton County, the country rises. Every few miles a steppe is mounted, the face of the escarpments showing horizontal thin limestone strata. The same features continue, and then the country slopes towards the Brazos river.

Turning westward through Haskell County, the surface lowers again towards the Brazos, the river coursing south to north, and a plain is crossed, the ground differing from any observed. The soil is mixed and covered with gravel, in many places several feet deep. The pebbles vary in size from half an inch to an inch and a half in diameter, and consist of feldspar quartz, porphyry, and basalt. On the western side of Haskell County the copper bed is reached not far from the Brazos river; and west of the copper a great belt of gypsum hills, several miles in width, extends northward, parallel with the copper bed, into the Indian Territory. Gypsum occurs there in most of its forms, including selenite which has been locally mistaken for mica.

On reaching a scene of attempted mining operations in search of supposed veins of copper, a very short examination convinced me that no vein would ever be discovered. Denudation has laid the bed bare, sweeping away the larger portion uncovered and leaving only patches; but these were sufficient to give a clear conception of the mode of occurrence. The copper-bearing stratum is an ashy-colored clay shale, more or less tinged with green, the upper portion showing the deep green carbonate of copper, usually two or three inches thick. Overlying this stratum is a cap-rock of gypsiferous sandstone, about three feet thick, with a layer ⅛ to ¼ inch thick, impregnated with carbonate of copper, as though it had soaked it up from below. Underneath the gray or green bed an intensely red clay shale is generally found. Nuggets of copper are scattered over the surface of the red bed, with pieces of cuprified wood and nuggets of iron pyrites. In the wood the original structure in many instances is perfectly preserved, also appearing cuprified in all stages of decay, as though it had become half rotten before the petrifaction was effected. The overlying sandstone frequently contains biscuit-like concretions of gypsum. Juniper trees abound and also cover the gypsum hills, the perfectly preserved cuprified wood, with its knots and bark, showing a fac-simile of that growth. I found in the gray bed fragments of wood partially unaltered, as though it had just commenced to absorb copper; also large pieces of coal, three or four inches or more in diameter, the cracks of the same piece being filled with crystalline carbonate of copper, or with white gypsum, thus appearing veined with copper and gypsum. In parts of the bed remaining the resemblance to piles of ashes and charcoal is strikingly deceptive; in one shaft, sunk to a depth of about thirty feet, the horizontal position of the strata was confirmed, the shaft passing through the cupriferous gray bed, and then through a succession of layers of red shale and soft red sandstone, in which not a trace of copper was found. The gray stratum extends seventy-five feet or more under a point of the gypsum hill. In a tunnel traversing this stratum I noticed occasionally pebbles belonging to the gravel drift. This copper formation has a general north and south course, usually less than fifty yards in width, and was traced for a distance of eight or ten miles to the southern boundary of Haskell County.

At one point the gray bed lies between beds of sandstone; the red bed does not appear, and the underlying sandstone strata are almost white, laminated, and very hard. The bed is more than two miles distant from the gypsum hills; the gravel drift is noticeable and even abundant. Observing the nuggets of copper ore and the drift pebbles lying about in places on the red bed, the idea forced itself upon me that there might be a remote connection between the two. However, the nuggets of ore are evidently concretions, and no pebbles occur in the gray bed. The gypsum range extends several miles across, with a western declivity similar to that on the eastern side. A plain, a little over one hundred feet below, reaches beyond to the foot of the great Llano Estacado. On these hills and on this western plain the gravel drift is wanting.

The copper bed was traced five miles further to the north; also in Knox county, not far from the Wichita river, and forty miles or more north of the southern portion of Haskell county, besides learning its supposed occurrence north of the Wichita river. The copper band

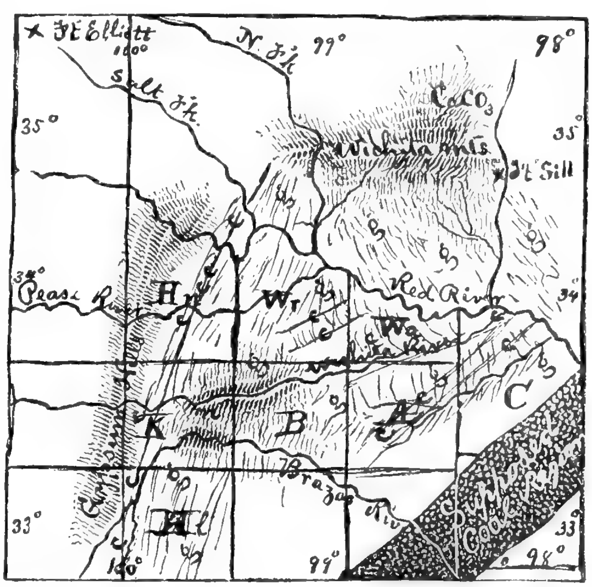
here lies between the sandstone and gray bed, with the red beds beneath. Eastward, between the Brazos and Wichita rivers, the gravel drift is abundant, with many stones of greater diameter. At the "Narrows," between the Wichita and Brazos rivers, the width is only sufficient to admit the passage of a single wagon. Continued caving in of the bluffs of the two rivers has widened an immense eroded area, rendering a large surface valueless, and while the channels of the rivers are several miles apart, their junction is only a question of time. In the copper region of the little Wichita river, near the centre of Archer county, the ore occurs under the same general conditions, with a different course, N. E. and S. W. and copper nuggets, coal and cuprified wood are found.

Embedded in the overlying sandstone, in some instances several feet above the gray bed, the sandstone frequently attains a thickness of more than fifteen feet. The cuprified wood is altogether different from that of Haskell county, and resembles the wood of the mesquite tree, which I found scattered about. The gravel drift here is identical in character with that of the region further west, and pebbles occur in the gray copper-bearing bed beneath the sandstone. The extension of the gravel drift of Haskell county, beyond the Brazos river system, its absence west of the gypsum hills, the larger size of the pebbles in Knox county, bordering the Wichita river, and the occurrence of the drift only in the vicinity of the copper-bearing lines mentioned, and in Archer county, suggested to me a possible relationship of some kind between the two, perhaps their origination in the same region.

Between the Wichita and Pease Rivers I crossed several copper-bearing beds, having a general northeast and southwest direction. In Wilbarger County the gravel drift is in great quantity, and boulders from three to seven inches in diameter occur. In places, and having a northeast and southwest bearing, heavy deposits or lines of gravel and boulders attract attention, appearing as though a great flow towards the southeast had met obstructions along its course, the great incline of this region being directed toward the southeast. Beyond Pease River the gravel drift lessens, but the large boulders are occasionally seen as far west as the gypsum hills. Not far north from the centre of Hardeman County I again found the Haskell County copper bed, the accompanying sandstones being thin and much mixed with gypsum. The copper bed reaches higher than the surrounding country, except the gypsum hills to the west. From this high locality of the copper, known as Prairie-dog Mounds, the country inclines on one side northward to a creek emptying into Red River, and on the other side southward to the Pease River.

South of these mounds, where only here and there patches of the bed are preserved in the midst of a general erosion, I found the largest mass of copper ore thus far discovered, consisting of an aggregation of cuprified wood, resembling the trunk of a tree, more than one foot in diameter. Beyond Red River the bed continues to the vicinity of the Salt Fork of Red River, distant but little over 20 miles from the Wichita Mountains of the Indian Territory. The bed probably continues nearly to the western end of these mountains, and here must be found the true centre of elevation and the origin of the gravel drift. The Haskell County copper bed was also traced south to the Wichita River, thus establishing its continuity from the southern portion of Haskell County, through Knox and Hardeman Counties, into the Indian Territory, a length of more than 100 miles. Subsequently, the northern end of the bed was found a short distance from the western end of the Wichita Mountains, on the south side of the range. The copper formations of Archer and Wichita Counties continue through Clay County to the Red River boundary of the Indian Territory. The gravel drift does not extend to the north of the Wichita Mountains, but a limestone district occurs about 20 miles in width, that reaches probably as far out to the north, from the Wichita Range, the course of the latter being east and west. This limestone area may be called mountainous, is much disturbed and tilted, and is similar in appearance to the metalliferous limestone formation of Mexico. The Wichita Mountains are mainly made up of porphyries, trachytes and basalt, and appear to be two parallel ranges with transverse ranges and small valleys between. About 12 miles west of Fort Sill an extensive body of hornblende slate makes its appearance between the two main ranges. The drift from the mountains extends to the south and southeast. It is found as far west as the Haskell County copper bed, and as far east as the Archer County copper bed is known. The river channels of that section of the country have been formed since this drift period. The development of the Wichita Mountains seems to have marked the close of a period of uplift and simultaneous erosion.

These mountains have the same general appearance as the Rocky Mountains, which pass through the western portion of Texas and the State of Coahuila, Mexico; and it has been a matter of much interest to observe that similar drifts of local origin are frequently met in the latter regions. The Wichita Mountains appear to be identical in origin with the Rocky Mountains, and constitute the most eastern spur of that system. In Northern Mexico short ranges are encountered, striking east and west, and of these the Wichita Mountains appear to be a reproduction. The Wichita Mountains will be found to contain mineral deposits, possibly of some value; veins of copper ores do exist 40 miles west of Fort Sill, near Otter Creek, in the mountains; but I am convinced that the copper bed or stratum of Northern Texas will prove of no commercial importance.



SCALE—52 MILES TO 1 INCH.

A.	Archer County.
B.	Baylor County.
C.	Clay County.
Hl.	Haskell County.
Hn.	Hardeman County.
Wa.	Wichita County.
Wr.	Wilbarger County.
c. c. c.	Copper Bed.
g. g. g.	Gravel Drift.
n.	Narrows.

Prof. Newberry remarked that the communication of Mr. Furman was of great interest, since no accurate description had before been given of the geological

structure of the region where the copper occurs in northern Texas and the Indian Territory. He had received specimens from that region long ago and recognized their similarity to the copper ores of New Mexico, where in the upper portion of the Triassic formation copper forming concretions and replacing wood occur in many localities, and have been more or less mined for. In one locality near Abiquini very extensive galleries have been cut in the sandstone in search of copper which there replaces branches and trunks of trees and forms concretions which are irregularly scattered through the rock. Here the work was done by the early Spanish explorers perhaps 200 years ago, and the remains of the furnaces in which the copper was smelted are still to be seen at the mouth of the mine. Still further west, in southern Utah, the same formation carries copper and considerable silver, at Silver Reef enough to pay well for mining, but in no locality yet known are the deposits of copper ore sufficiently concentrated and continuous to make mining for that material profitable, so it would doubtless be found in Texas and the Indian Territory. The copper was deposited with the Triassic rocks from a shallow sea in which an unusual quantity of copper was held in solution. This impregnated the sediments found at the bottom replacing wood and forming as nodules about some nucleus. The aggregate quantity of copper in this formation was enormous, but, except where by the erosion of the beds it accumulated at the surface and could be picked up without any expense in mining, it would hardly pay to attempt to obtain it by ordinary mining processes.

The wood replaced by copper Dr. Newberry said was undoubtedly all coniferous, and different from any now living. The beds which contained the cuprified wood also contained much that was silicified. Of this he had examined many specimens under the microscope and had found the peculiar dotted cells which are characteristic of the coniferæ, and these grouped in such a way as to prove the trees to have belonged to the Araucarian group of conifers. So far as yet known the angiosperms, or higher order of plants, did not make their appearance on the earth's surface until after the copper bearing rocks of the southwest had been deposited.

THE AMERICAN CHEMICAL SOCIETY.

The November meeting of this Society was held on Friday evening, November 4th, with Vice-President Leeds in the Chair.

The following gentlemen were duly elected members: Dr. C. W. Volney, Dr. Witthaus, Messrs. C. E. Munsell, W. W. Share, J. D. O'Connor, and Day. The first paper of the evening was "On some New Salts of Thymole Sulpho-acid, and some new facts concerning the same," (a second paper) by Mr. J. H. Stebbins, Jr., S. B. The sodium salt having the formula

$$C_6H_2\,(CH_3)\,(C_3H_7)\,(NaSO_3)\,O\,Na+2\tfrac{1}{2}H_2O$$

was described, and also the free sulphur salt had its characteristics enumerated.

Mr. Stebbins followed with a second paper "On the Combination of Diazo Compounds with Thymole Sulpho-Acid."

In this he described the experiments which he performed in his work, the results of which were given in the first paper. Both were technical and not of any popular interest.

The third paper was by Dr. C. W. Volney, and was entitled, "The Constitution of the Explosive Derivatives of Glycerine."

In this communication the author tried to prove that the nitro-glycerine was composed by the substitution of the nitrogen trioxide (NO_3) instead of the nitrous oxide NO_2, making the formula $C_3H_5\,(NO_3)_3$ instead of $C_3H_5\,(NO_2)\,O_3$, and secondly, he showed how it was possible to substitute chlorine for the nitrogen trioxide and so produce a new explosive compound.

This paper provoked much discussion on account of the theoretical arrangement of the atoms necessary to sustain Dr. Volney's statement.

Subsequently the Committee on Nominations reported that the following ticket was recommended to the Society for their votes at the December meeting.

Corresponding Secretary.— P. Casamajor.

Recording Secretary.—J. H. Stebbins, Jr.

Treasurer.—M. Alsberg.

Librarian.—Geo. A. Prochazka.

Curators.—A. J. Rossi, Wm. Rupp, A. A. Fesquet.

Committee on Publications.—Arno Behr, A. R. Ledoux, H. Endemann.

Committee on Nominations.—A. H. Elliott, O. H. Krause, J. P. Battershall, J. B. F. Herrishoff, T. O'C. Sloane.

Board of Directors.—P. Casamajor, J. H, Stebbins, Jr., H. Morton, C. F. Chandler, M. Alsberg, E. R. Squibb, W. H. Nichols, W. H. Habershaw, E. Waller, A. H. Gallatin, Geo, A. Prochazka.

ON THE NATURE OF THE DIPHTHERITIC CONTAGIUM.

By Dr. H. C. Wood.

The lecturer began by stating that the researches which formed the basis of the present address had been made under the auspices, and, indeed, at the suggestion, of the National Board of Health, by Dr. Henry F. Formad and himself, who were jointly responsible for the facts and inductions and jointly deserving of whatever reprobation or approbation might be due. The full text of the work is now in the hands of the National Board, and will be shortly published by them as an appendix to their annual report, and the lecturer desired that criticism be withheld until this was done, as the memoir will contain much that cannot be spoken of in the present lecture.

In the spring of 1880 work was begun by inoculating rabbits with diphtheritic membrane taken from the throats of patients at Philadelphia. An account of the labors of the following summer has been already published, but it seems necessary to epitomize them here. It was found that only in a very few cases was anything like diphtheria produced in the rabbit by inoculating with the membrane. The inoculations were practised by putting pieces of the material sometimes under the skin, sometimes deep in the muscles. Many rabbits died after some weeks, not of diphtheria, but of tuberculosis. In a series of experiments it was shown that this tuberculosis was an indirect and not a direct result of the inoculation, and that any apparent relation between the two diseases is only apparent, not real. Next, the tracheas of a series of rabbits were opened and false membrane inserted. It was found that under these circumstances a severe trachitis was frequently produced, and was attended by an abundant formation of pseudo-membrane. Careful studies made of the false membrane of diphtheria and of this false membrane showed that the two were identical, both containing in abundance fibrin fibres, corpuscular elements, and various forms of micrococci. To determine whether other inflammations of the trachea than that caused by diphtheria or its membrane are accompanied by the formation of false membrane, a number of experiments were made, and it was demonstrated that the production of false membrane has nothing specific in it, but that any trachitis of sufficient severity is accompanied by this product. Careful studies also showed that this false membrane does not differ in its constitution from that of true diphtheria, except it be that the micrococci are not so abundant in it. We always found some micrococci, and in some of these traumatic pseudo-mem-

*An address made before the Academy of Natural Sciences.

branes they were almost as numerous as in the diphtheritic exudation.

Last spring we resumed our investigations. Having heard that there was a very severe epidemic in Ludington, Mich., Dr. Formad was dispatched to examine cases and collect material. He found a small town situated upon the shore of Lake Michigan, in the centre of the lumber region, with inhabitants mostly engaged in the lumber trade and in managing very numerous large sawmills. The town was all built upon high ground except the Third Ward. This occupied a low swamp which had been filled in largely with sawdust. The soil was so moist that a hole dug in it would fill at once with water, and but few houses had any attempts at cellars. It was in this district that the disease had prevailed. Almost all the children had had it, and one-third of them were said to have died. Dr. Formad examined a large number of cases, obtained a supply of diphtheritic membrane, and brought home pieces of the internal organs of a child upon whom he had made an autopsy. In every case the blood was found more or less full of micrococci, some free, others in zoogl œa masses, others in the white blood-corpuscles. The organs brought home also all contained micrococci, which were especially abundant in the kidneys, where they formed numerous thrombi, choking up and distending the blood-vessels. In the summer of 1880 we examined the blood of several cases of endemic Philadelphia diphtheria, and in no case found any new elements in it. But during the present summer we have found micrococci in the blood of Philadelphia diphtheritic patients, showing the differences in the disease are simply in degree, not in kind.

Experiments were now made with the Ludington material upon animals. Inoculations were practised under the skin, deep in the muscles, and in the trachea. In all cases the results were similar. A grayish exudation appeared at the seat of inoculation, along with much local inflammation, the animal sickened, and in the course of a few days death occurred. The local symptoms increased and widened. In some cases the false membrane spread from where the poison had been put in the trachea up to the mouth. The blood examined during life or after death was found to contain micrococci precisely similar to those found in the Ludington cases, and in a few instances micrococci were found in abundance in the internal organs. Studies made upon the blood of these animals, as well as upon the Ludington cases, show that the micrococci first attack the white blood-corpuscles, in which they move with a vibratile motion. Under their influence the corpuscles alter their appearances, losing their granulations. They finally become full of the micrococci, which now are quiescent, and increase until the corpuscle bursts and the contents escape as an irregular, transparent mass full of micrococci, and form the so-called zoogl œa masses. In the diphtheritic membrane the micrococci exist frequently in balls, and it is plain that these collections are merely leucocytes full of the plant. The bone-marrow of the animals were found full of leucocytes and cells containing micrococci.

The question now arose, is the disease produced by diphtheritic inoculation in the rabbit diphtheria? We concluded that it is, because the poison producing it is the same, the symptoms manifested during life are the same, and the post-mortem lesions are identical. The contagious character of the disease is retained, as we succeeded in passing it from rabbit to rabbit.

Our next series of experiments were directed to determining whether the micrococci are or are not the cause of the affection. The experiments of Curtis and Satterthwaite, of New York, have shown that the infectious character of diphtheria depends upon its solid particles; for when they filtered an infusion of the membrane it became less and less toxic in proportion as the filtration was more and more perfect; and when the infusion was filtered through clay, the filtrate was harmless.

The urine of patients suffering from malignant diphtheria is full of micrococci, and may contain no other solid material. Following the experiments of Letzerich, we filtered this urine and then dried the filter-paper. Upon experiment we found this even more deadly in its effects than is the membrane. The symptoms and lesions following in the rabbit inoculation with such paper are precisely those which would have ensued had a piece of diphtheritic kidney or membrane been employed. This experiment shows that the solid particles of the membrane, which are the essential poison of malignant diphtheria, are the micrococci, which must be either the poison itself or the carriers or producers of the poison.

Leaving for a while this point, I will next direct your attention to our culture-experiments. These were performed in the manner commenced by Klein and that recommended by Sternberg. The first method seems to us the best for the purpose of studying the development of the micrococcus itself; the second the best for the obtaining of it in quantity for experimentation.

We cultivated micrococci from the surface of ordinary sore throats, from furred tongue, from cases of mild diphtheria as we commonly see it in Philadelphia and from Ludington cases. We found, in the first place, that there were no differences to be detected in the general or special appearance of the various micrococci, and no constant differences in size. We found that they all formed similar shapes in the culture apparatus; they had this difference, however,—whilst the Ludington micrococci grew most rapidly and eagerly generation after generation up to the tenth, those from Philadelphia diphtheria ceased their growth in the fourth or fifth generation, whilst those taken from furred tongue never got beyond the third transplantation. Various culture-fluids were used, but the results were identical. We conclude, therefore, that as no difference is detectable between the micrococci found in ordinary sore throat and those of diphtheria, save only in their reproductive activity, they are the same organisms in different states. As the result of some hundreds of cultures, we believe that the vitality of the micrococci under artificial culture is in direct proportion to the contagious powers of the membrane from which they have been taken. We have made many inoculations with cultivated micrococci and have succeeded in producing diphtheria with the second generation, but never with any later product. This success, taken in conjunction with the urine experiments already spoken of, seems to us sufficient to establish the fact that the micrococci are the *fons et origo mali* of diphtheria. The experiments of Pasteur and others have proven that it is possible for an inert organism to be changed into one possessed of most virulent activity, or *vice versa*, and we believe that we can offer direct proof that the micrococci of the mouth are really identical in species with the micrococci of diphtheria, and do not merely seem to be so. We exposed the Ludington membrane for some weeks to the air in a dried condition. There was no putridity or other change detectable in it; but, whereas formerly it had been most virulent, now it was inert, and its micrococci not only looked like those taken from an ordinary angina, but acted like them. They were not dead, they had still power of multiplication, but they no longer grew in the culture-fluid beyond the third or fourth generation. Certainly they were specifically the same as they had been, and certainly therefore the power of rapid growth in culture-fluids and in the body of the rabbit is not a specific character of the diphtheria micrococcus.

As is well known, Pasteur attributes the change from an active to an inert organism to the influence of the oxygen of the air upon the organism. Whether this be true or not of the diphtheria micrococcus is uncertain, but the effects of exposure of the dried membrane seem to point in such direction.

With the facts that are known in regard to the clinical history of diphtheria and those which we have determined in our research, it is easy to make out a theory of the disease which reconciles all existing differences of opinion and seems to be true.

A child gets a catarrhal angina or trachitis. Under the stimulation of the inflammation products the inert micrococci in the mouth begin to grow; and, if the conditions be favorable, the sluggish plant may be finally transformed into an active organism, and a self-generated diphtheria results. It may be, however, that by appropriate treatment such a case is arrested before it fairly passes the bounds of an ordinary sore throat. Every practitioner knows that such diversity does exist. Again, conditions outside of the body favoring the passage of inert into active micrococci may exist, and the air at last become well loaded with organisms, which, alighting upon the tender throats of children, may begin to grow and themselves produce violent angina, trachitis, and finally fatal diphtheria.

In the first instance we have endemic diphtheria as we see it in Philadelphia; in the second, the malignant epidemic form of the disease as it existed in Ludington. It is also apparent that in the endemic cases the plant whose activity has been developed within the patient may escape with the breath, and a second case of diphtheria be produced by contagion. It is also plain that as the plant gradually in such a case passes from the mild to the active state, there must be degrees of activity in the contagium, one case being more apt to give the disease than is another; also that the malignant diphtheria must be more contagious than the mild endemic cases. We think there is scarcely a practitioner who will not agree that clinical experience is in accord with these logical deductions from our experimentally determined premises.

It yet remains for us to investigate as to what are the conditions outside of the body which will especially favor the production of active micrococci, and also to study the effects of agents in killing these organisms; for it is very apparent that local treatment of the throat must often be of the utmost importance, and that it will be far more effective if it be of such character as to kill the micrococci, and not simply be anti-phlogistic in its action.

SOLAR PARALLAX.

In an elaborate paper, given in full in the *American Journal of Science*, for November, Professor William Harkness draws the following conclusions:—

For convenience of reference the limiting values of the solar parallax, found by the various methods described in the foregoing pages, are presented here. It should be remarked, however, that in selecting these values the results of all discussions made prior to 1857 have been omitted; except in the case of the transit of 1761, and the smaller of the two values from the transit of 1769.

I.—Trigonometrical methods.

Mars, meridian observations	8″.84	—	8″.96
" diurnal observations	8.60	—	8.79
Asteroids	8.76	—	8.88
Transit of Venus, 1761	8.49	—	10.10
" " 1769	8.55	—	8.91
" " 1874	8.76	—	8.85

II.—Gravitational methods.

Mass of the earth	8″.87	±	0″.07
Parallactic Inequality	8.78	—	8.91
Lunar Inequality	8.66	—	9.07

III.—Photo-tachymetrical methods.

Velocity and light equation	8″.72	—	8″.89
Velocity and Aberration	8.73	—	8.90

To obtain a definite value of the solar parallax, it would now be necessary to form equations of condition embodying the relations between the various elements involved; to weight these equations; and to solve for it by the method of least squares. But what is the use? It is perfectly evident that by adopting suitable weights, almost any value from 8″.8 to 8″.9 could be obtained, and no matter what the result actually was, it would always be open to a suspicion of having been cooked in the weighting. We only know that the parallax seems to lie between 8″.75 and 8″.90, and is probably about 8″.85. Attack the problem as we will, the results cluster around this central value. All the methods give a probable error of about ±0″.06, and no one of them seems to possess decided superiority over the others. We have nearly exhausted the powers of our instruments, and further advance can only be made at the cost of excessive labor.

In the beginning of the eighteenth century the uncertainty of the solar parallax was fully two seconds; now it is only about 0″.15. To narrow it still further, we require a better knowledge of the masses of the earth and moon, of the moon's parallactic inequality, of the lunar equation of the earth, of the constants of nutation and aberration, of the velocity of light, and of the light equation. All these investigations can be carried on at any time, but there are others equally important which can only be prosecuted when the planets come into the requisite positions. Among the latter are observations of Mars when in opposition at its least distance from the earth, and transits of Venus.

In 1874 all astronomers hoped and believed that the transit of Venus which occurred in December of that year would give the solar parallax within 0″.01. These hopes were doomed to disappointment, and now, when we are approaching the second transit of the pair, there is less enthusiasm than there was eight years ago. Nevertheless the astronomers of the twentieth century will not hold us guiltless if we neglect in any respect the transit of 1882. Observations of contacts will doubtless be made in abundance, but our efforts should not cease with them. We have seen that the probable error of a contact observation is ±0″.15, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. On the other hand, the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only ±0″.55. If the sun is visible for so much as fifteen minutes during the whole transit, thirty-two negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. In view of these facts, can it be doubted that the photographic method offers as much accuracy as the contact method, and many more chances of success?

The transit of 1882 will not settle the value of the solar parallax, but it will contribute to that result, directly as a trigonometrical method, and indirectly through the gravitational methods with which the final solution of the problem must rest. As our knowledge of the earth's mass may be made to depend upon quantities which continually increase with the time, it will ultimately attain great exactness, and then the solar parallax will be known with the same exactness. Long before that happy day arrives the present generation of astronomers will have passed over to the silent majority, but not without the satisfactien of knowing that their labors will contribute to that fullness of knowledge which shall be the heritage of their successors.

EPHEMERIS OF THE SATELLITES OF MARS FOR THE OPPOSITION OF 1881.*

By H. S. Pritchett.

Owing to the greater distance from the Earth and the Sun, the present opposition of Mars will not be so favorable as the two preceding ones ; still these distances will be sufficiently small to permit many useful observations of physical phenomena, and, in the case of large telescopes, observations of the satellites. In one respect, the planet is much more favorably situated than in the former oppositions referred to, since it reaches this year a declination of 26° north, and hence will be observed at a much higher altitude. Physical observations, either measures or drawings, by amateur astronomers with good glasses, if carefully made and published, will be useful when finally reduced and compared.

During the last opposition several series of micrometric measures of the diameter of the planet were made by observers with good telescopes which showed curious differences both between themselves and when compared with the results obtained from the heliometer. Some of these measures seemed to show an appreciable flattening at the poles, while others showed no such flattening. It will be interesting to have these measures repeated during the present opposition, with a careful discussion of the sources and effects of personal error.

The satellites were observed last opposition with at least one of the large reflectors, with the great refractor at Washington, with the 15-inch refractor of the Harvard College Observatory, and with the 12¼ inch refractor of the Morrison Observatory, and were seen with other instruments. Before December 1st of this year the satellites will be considerably brighter than when last observed in 1879 with the Harvard College refractor, and also brighter than when last observed with the Morrison Observatory refractor. It seems possible, therefore, that they may be seen this year with telescopes even of moderate size.

The following ephemeris (derived from the elements of Prof. A. Hall, A. N. No. 2394) has been computed at the request of several observers, and will be found convenient for any who may wish to observe these satellites. In connection with the discussion of the relative merits of reflectors and refractors, excited by the observations of these satellites, it may be interesting to many to try if they can see them.

In the case of Deimos, the outer satellite, the ephemeris gives the Washington mean times of the east and west elongations, together with the position-angle and distance at the time of elongation. In the case of Phobos only the times of western elongations are given, as the revolution time is very short and the times of eastern elongations may be obtained by a simple interpolation. The aberration time is not included in the time given, but it may be taken from the table at the end if desired, the effect of the aberration being to make the satellites about five minutes late at each elongation. The relative brightness on different days may be obtained from the same table, taking the brightness on Nov. 20 as unity. As was shown by the observations of 1879, Prof. Hall's elements are very nearly correct, so that the correction to this ephemeris will be quite small.

* Read before the St. Louis Academy of Sciences.

Deimos.

Date.	Direction of Elongation.	Wash. M. T.	Pos. Ang.	Dist.	Date.	Direction of Elongation.	Wash. M. T.	Pos. Ang.	Dist.
		H. M.					H. M.		
Dec. 5	E	16 33	-----	----	Dec. 22	E	2 2	-----	----
6	W	7 42	-----	----		W	17 10	-----	----
	E	22 50	----	----	23	E	8 19	-----	----
7	W	13 58	-----	----		W	23 26	-----	----
8	E	5 7	-----	----	24	E	14 33	-----	----
	W	20 14	250°.2	52".4	25	W	5 41	-----	----
9	E	11 23	-----	----		E	20 49	-----	---
10	W	2 31	-----	----	26	W	11 57	-----	----
	E	17 39	-----	----	27	E	3 5	-----	----
11	W	8 47	-----	----		W	18 12	-----	----
	E	23 55	-----	----	28	E	9 20	-----	----
12	W	15 3	-----	----	29	W	0 28	-----	----
13	E	6 11	-----	----		E	15 36	-----	----
	W	21 19	249°.7	53".2	30	W	6 44	-----	----
14	E	12 27	-----	----		E	21 52	-----	----
15	W	3 35	-----	----	31	W	13 0	-----	----
	E	18 43	-----	----	Jan 1	E	4 7	-----	----
16	W	9 31	-----	----		W	19 15	245°.9	52".4
17	E	0 59	-----	----	2	E	10 23	-----	----
	W	16 7	-----	----	3	W	1 31	-----	----
18	E	7 15	-----	----		E	16 39	-----	----
	W	22 23	-----	----	4	W	7 47	-----	----
19	E	13 31	-----	----		E	22 55	-----	----
20	W	4 39	248°.5	53".7	5	W	14 3	-----	----
	E	19 47	-----	----	6	E	5 11	-----	----
21	W	10 54	-----	----		W	20 19	-----	----

Phobos.

Date.	Wash. M. T.	Pos. Ang.	Dist.
	H. M.		
Dec. 2	1 40	251°.5	20".3
	9 19	------	----
	16 58	------	----
3	0 37	------	----
	8 16	------	----
	15 56	------	----
	23 35	------	----
4	7 14	------	----
	14 53	------	----
	22 32	------	----
5	6 12	------	----
	13 51	------	----
	21 31	------	----
6	5 10	------	----
	12 49	------	----
	20 28	------	----
7	4 7	------	----
	11 46	------	----
	19 25	250°.4	20".9
8	3 5	------	----
	10 44	------	----
	18 23	------	----
9	2 2	------	----
	9 41	------	----
	17 20	------	----
10	0 5[illegible]	------	----
	8 39	------	----
	16 18	------	----
	23 57	------	----
11	7 36	------	----
	15 15	------	----
	22 54	------	----
12	6 33	------	----
	14 13	------	----
	21 52	------	----
13	5 31	------	----
	13 10	------	----
	20 49	249°.7	21".3
14	3 28	------	----
	12 7	------	----
	19 46	------	----
15	3 25	------	----
	11 4	------	----
	18 43	------	----
16	2 23	------	----
	10 2	------	----
	17 41	------	----
17	1 20	------	----
	8 59	------	----
	16 38	------	----
18	0 17	------	----
	7 56	------	----
	15 35	------	----
	23 14	------	----
19	6 53	------	----
	14 33	------	----
	22 12	------	----

Date.	Wash. M. T.	Pos. Ang.	Dist.
	H. M.		
Dec. 20	5 51	248°.5	21".5
	13 30	------	----
	21 9	------	----
21	4 48	------	----
	12 27	------	----
	20 6	------	----
22	3 46	------	----
	11 25	------	----
	19 4	------	----
23	2 43	------	----
	10 22	------	----
	18 1	------	----
24	1 40	------	----
	9 19	------	----
	16 58	------	----
25	0 37	------	----
	8 16	------	----
	15 55	------	----
	23 34	247.°3	21".4
26	7 13	------	----
	14 53	------	----
	22 32	------	----
27	6 11	------	----
	13 50	------	----
	21 29	------	----
28	5 8	------	----
	12 47	------	----
	20 26	------	----
29	4 5	------	---
	11 44	------	----
	19 23	------	----
30	3 3	------	----
	10 42	------	----
	18 21	------	----
31	2 0	------	----
	9 39	------	----
	17 18	------	----
Jan. 1	0 57	246.°0	21".1
	8 36	------	----
	16 15	------	----
	23 54	------	----
2	7 33	------	----
	15 13	------	----
	22 52	------	----
3	6 31	------	----
	14 10	------	----
	21 49	------	----
4	5 28	------	----
	13 7	------	----
	20 46	------	----
5	4 25	------	----
	12 4	------	----
	19 43	------	----
6	3 23	------	----
	11 2	------	----
	18 41	------	----
7	2 20	------	----

DATE.	Brightness.	Semi-diam. Mars.	Aberration Time.
			M.
Dec. 2.0	1.15	7.3	5.3
8.0	1.21	7.5	5.2
14.0	1.24	7.6	5.0
20.0	1.26	7.7	5.0
26.0	1.24	7.7	5.0
Jan. 1.0	1.18	7.6	—5.1

From this it will be seen that Phobos, even on the most favorable date, will be only about 14" distant from the limb of the planet. In 1877 this satellite was observed with the 12¼ equatorial of the Morrison Observatory when only 7" distant. In the present opposition the satellite will be much fainter, but on the other hand the brightness of the planet will be considerably diminished. It seems possible, therefore, that this satellite may be seen with glasses of moderate size.

WASHINGTON UNIVERSITY, *Nov.*, 1881.

ELEMENTS OF QUATERNIONS.*

BY A. S. HARDY, Ph. D., Professor of Mathematics, Dartmouth College.

The American press may be expected to teem for the next twenty-five years with elementary treatises on quaternions, and as this work of Professor Hardy's is, we believe, the first of the series, it merits on this account the more attention. The book has a quite neat and attractive exterior, and the mechanical execution is very fair, though a few defects in letter press and engraving are noticeable. The experiment of printing small Alphas with an oblique line through them seems to be a failure. See pp. 45 and 60.

We cannot think the title happily chosen. There is an incongruity, if not positive impropriety, in assigning to a scant text-book intended for beginners in the class-room a name associated these fifteen years with the great and classic work of Hamilton. This however, is a matter of taste. One of the most important and difficult steps in the logical development of the calculus of quaternions, to which their inventor gave no little attention, is that of assigning a versor power to a vector, or of representing rotation by a symbol that had hitherto been appropriated exclusively to vection or translation. This, in the book before us, is disposed of in a few lines, when, even in a treatise where brevity must be studied, it is well worthy of as many pages. There is, also, throughout the work, an unfortunate fondness for the plane, where quaternions are often at a disadvantage, and where their real power and usefulness cannot be exhibited. The author may have intended to thus avail himself of the student's greater familiarity with the geometry of the plane, while introducing him to a new method; but it ought to be borne in mind that one of the chief claims quaternions have on the teacher of geometry is that they are specially fitted to free the student from the too prevalent restriction of his conceptions to two dimensions. A curious example of this tendency of the book is afforded near the end in applications to loci. Here the author systematically interprets equations as relating to the conic sections, when in reality they frequently relate to quadrics of revolution, the restriction to plane loci having been eliminated in the process of their formation; and when he comes "to transform the proceeding equations into the usual cartesian forms," instead of substituting a trinomial for the variable vector, he imposes a restriction to two dimensions by adopting a binomial, and of course comes out with a plane section in place of the surface itself. Notwithstanding these imperfections, Prof. Hardy has evidently studied his subject and written his book with some care, and with a view to the requirements and opportunities of those for whom it is intended, and it will doubtless prove useful as an introduction to quaternions.

ALEX. S. CHRISTIE.

U. S. COAST & GEODETIC SURVEY,
WASHINGTON, *November* 11, 1881.

* 8°, pp. VIII, 230. Boston, Ginn, Heath & Co., 1881.

LARGE TELESCOPES.

PROFESSOR EDWARD C. PICKERING makes the following suggestion in regard to mounting a telescope on a new plan. He says:—"The small amount of work accomplished with large telescopes has often been the subject of unfavorable comment. This criticism applies with especial force in America, where there are nearly a dozen telescopes having an aperture of a foot or over, besides two of the largest size now in course of construction, and two of twenty-six and twenty-four inches aperture which are unmounted and have been for several years perfectly useless. Among so many it seems as if one might be spared for a trial of the following plan, which, if successful, would produce at a small expense far more work than could be obtained with a mounting of the usual form.

Suppose that the telescope is placed horizontally at right angles to the meridian, and that a plane reflector inclined to its axis by 45° is placed in front of it. This reflector may revolve around an axis coinciding with that of the telescope. Such a mounting has been used in transit instruments, and gives much satisfaction in the meridian photometer of the Harvard College Observatory. The principal difficulty with a large instrument would lie in the flexure of the reflector. This difficulty has, however, been overcome in a great measure in reflecting telescopes by various ingenious devices. In the present case, since the reflector rotates only around one axis instead of two, the problem is much simplified. A slight motion at right angles of perhaps 5° would be a great convenience, as will be shown below, and would probably be insufficient to materially affect the flexure. It may be said that it is more difficult to make a plane surface than one that is curved. But the principal effect of a slight curvature would be to change the focus of the telescope, the aberration being much less than the effect of the varying flexure. Let us admit, however, that the best definition cannot be obtained, in considering the purposes to which such an instrument could be applied without disadvantage.

Many advantages will be apparent on comparing such a mounting with an equatorial. Great steadiness would be secured, since the mirror would be the only portion moved, and this would be placed directly upon a low pier. Instead of a large and expensive dome which is moved with difficulty, the mirror would be protected by a small shed, of which the roof could be easily removed. It would therefore be opened and ready for use in a very short time, and would quickly take the temperature of the surrounding air. The object-glass would be mounted directly upon a second pier, and, as it would not be moved, would be in very little danger of accident. The tube could be made of tin or other inexpensive material, as its flexure is of no importance. It could easily be protected from the changes of the temperature so troublesome in the tube of a large equatorial. If preferred it might even be exhausted of air, or filled with hydrogen, and the effect of the changes of temperature thus greatly reduced.

The eyepiece could be mounted on a third pier, and would be so far distant horizontally from the mirror and object-glass that there is no reason that it should not be inclosed in a room which may be warmed. The comfort in winter of working in a warm room will be appreciated by those who have used a large telescope in a cold climate. The result is sure to be an increased precision in

the observations, and a possibility of prolonging them over longer intervals. A similar effect is produced by the constant direction of the line of sight. No especial observing chair is needed. There is no limit to the size of the attachments which may be made to the eyepiece, since they need not be moved. This is a great advantage in certain spectroscopic and photometric measurements. A strong wind interferes seriously with many observations, as it is impossible to make a telescope so stiff that it will not be shaken by sudden gusts. In the plan here proposed the mirror alone is exposed, and its surface is too small to give trouble.

By means of a long handle the position of the mirror may be regulated from the eye-end, and the declination of the object observed read by small telescopes. If the mirror can be moved at right angles to the meridian 5° from its central position, an object at the equator may be followed for forty minutes, and other objects for a longer period. Without this motion an object may be followed for three or four minutes by moving the eyepiece alone. Clockwork may be applied to the mirror, or less easily to the eyepiece. The focal length may be increased almost indefinitely if desired, and certain advantages will be thus attained in the diminution in the defects of the object-glass, although those of the reflector will not be affected. If the telescope is to be erected at a great elevation the advantages of the present plan are at once apparent. Many nights of observation would be secured which otherwise would be lost owing to the wind and cold. The simplicity in the construction of the building would be a great advantage, as a large dome in so exposed a situation would be kept free from snow with much difficulty, and might be a source of danger in winter storms. If found impracticable to observe during the winter, it would be possible to have a duplicate mounting below, and remove the lens and mirror from one to the other.

It is evident that the saving of cost would be very great, not only in the observatory building aud dome, but in the tube, observing chair, clockwork, &c.

If a reflector could be constructed whose surface was the portion of a paraboloid whose abscissa equalled that of the focus, the instrument could be much simplified. No object-glass would then be required, the reflector taking the place both of mirror and lens. All the light intercepted by the objective would thus be saved, and but a single surface need be adjusted and corrected. With the advance in mechanical methods this does not seem wholly impracticable, especially with a mirror of long focus. Since the final correction must always be made by hand, it makes but little difference what is the exact form of the surface.

In any case it would be a great advantage that the mirror could be reground, repolished, or resilvered without moving it from its place. It would only be necessary to place it horizontally, and the grinding machinery could be kept permanently near it. If plane, the perfection of its form could also be tested at any time by setting it on edge, and viewing the image it reflected by a collimating eyepiece attached to the large telescope. Another method would be to place a heliotrope a few hundred yards to the north or south of it, and the light from this would form an excellent artificial star, available whenever the sun shone.

The greatest advantage is the rapidity with which observations could be made. No more time would be lost in identification than with a transit instrument, so that a large number of objects could be examined in the course of a single hour. Any one who has worked with a large telescope knows how much time is lost in opening and closing the dome and in finding and identifying minute objects.

Let us now consider to what purposes a large telescope mounted as suggested might be applied.

1. Sweeping. For the discovery of new objects this mounting presents especial advantages. It might be used for the detection of new double stars, of nebulæ, of red stars, or of objects having singular spectra, as planetary nebulæ, banded stars, and variables of long period. Suppose that the field of view had a diameter of somewhat over one minute of time, and that a small motor was attached to the mirror which would move it uniformly over 5° in declination in this time, and then bring it quickly back to its first position. The observer would then have presented to him a series of zones 5° long and one minute wide. The sweeps should overlap by a small amount, so that the entire region could be covered in a single evening. The observer could have a few seconds rest between each zone, while the motion of the mirror was reversed. If an object of interest was suspected, it could be located by merely noting the time at which it was seen. The right ascension would be given directly, and the declination would be found by interpolation from the time of beginning and ending the sweep. An examination of the object and a determination of its exact location should be made on another evening.

2. Measures of position. For many purposes positions could be determined with this instrument as in a transit circle. It would generally be better, however, to make the measures differential, leaving the mirror at rest and observing the transits of the object to be determined and of two or more companion stars. The method of the ring micrometer might be employed, or some modification of that with inclined lines. In the latter case the zero of position could be found by the transit of preceding stars, by setting the reticule by a divided position circle, or perhaps better by keeping it in a fixed position, determining the direction of the lines once for all, and applying a correction for the declination of the object observed. Stars could be compared differing nearly a degree in declination, as the eyepiece could be moved without danger of disturbing the reticule. For the same reason the star could be followed for three or four minutes, and its transit over a great number of wires observed. It is here assumed that the distortion produced by the mirror is not very great. A slight distortion would do little harm, as it would be the same for all stars of equal brightness. If the stars differ greatly in brightness, the observer should determine his personal equation between them in any case, and the same operation would eliminate the effect of the distortion. The large aperture of the instrument would permit the observation of stars quite beyond the reach of any meridian circle. The faintest asteroids could thus be readily measured, and could probably be followed in many cases on successive evenings to their stationary points. Zones of stars could be observed very conveniently for the formation of charts or catalogues, for the discovery of asteroids, stars with large proper motion, &c.

Probably the definition could not be sufficiently good for the measurement of the closer double stars, but if clockwork was attached, faint companions could be measured, or approximate positions of the coarser pairs determined very rapidly. The positions of nebulæ could also be observed with a view to detecting their proper motion. Stars having a large proper motion might be observed, and the observations so arranged that any very large parallax would be detected. A similar search for a large parallax of variable stars, short-period binaries, minute planetary nebulæ, or stars having singular spectra, might lead to interesting results. The argument that no ordinary star is very near does not apply to such objects.

3. Spectroscopy. The increased dimensions which could be given to the spectroscope, and its steadiness, would compensate in a great measure for a defect in definition. By Zöllner's reversion spectroscope the slit might be dispensed with, and also the necessity of clockwork. So many stars could be observed in a single evening that systematic errors could be in a great measure eliminated, and as the spectroscope would not be moved, we should have a great assurance that the deviations were

real. Of the 6000 nebulæ hitherto discovered we know nothing of the spectrum of more than 300 or 400, while the observation of all the others with a large horizontal telescope would not be a very formidable undertaking. It would also be interesting to observe the spectra of all the clusters. It is possible that some may consist of stars having singular spectra, or even of disconnected nebulous masses, in fact forming clusters of planetary nebulæ. The interesting discovery by Dr. Copeland that Burnham's double nebula in Cygnus is gaseous, shows the same tendency to aggregation in these bodies as in stars. Observations of the spectra of all the red stars and variables would also probably lead to interesting results.

4. Photometry. Should the instrument be devoted to photometry numerous problems suggest themselves. Variable stars could be observed near their minimum when too faint to be identified with an equatorial without great loss of time. Faint stars in zones or faint companions to bright stars could be measured very rapidly. The relative light of all the asteroids would be an interesting problem. Many coarse clusters appear to consist of stars of nearly equal brightness. Their light compared with their distances apart might aid our study of their formation. Another useful investigation would be to measure the brightness of all the nebulæ.

In the application of physics to astronomy doubtless many other problems will suggest themselves. Thus no satisfactory results have been obtained in the attempt to measure the heat of the stars with the tasimeter. The use of this instrument would be vastly simplified if it was placed on a solid pier near the ground, was not moved during the observation, and could be perfectly protected from other changes of temperature than those which it was intended to measure.

As either of the problems proposed above would occupy the time of a telescope for at least one year, it is obvious that there could be no difficulty in keeping such an instrument occupied indefinitely.

The horizontal mounting is especially adapted to an elevated position, and would permit the use of a telescope where an equatorial mounting would be quite impracticable. On the other hand, to an amateur, or for purposes of instruction, an instrument which could be set quickly from one object to another, and where the observers need not be exposed to the cold, would offer many advantages. The impossibility of observing far from the meridian would be less important with a large instrument, where the number of objects to select from is very great.

There are certain purposes to which this mounting could not be advantageously applied. The study of close double stars and other objects requiring long examination and very perfect definition could be better left to other instruments. The sun, moon, and planets can also generally be better observed off the meridian. If, however, the entire time of an instrument can be employed to advantage, and it can collect several times as much material as an instrument of the usual form, it is no evidence against its trial that there are certain problems to which it cannot be advantageously applied.

The working force required for such an instrument should consist of at least one observer, an assistant to record, and a number of copyists and computers to prepare the working lists, reduce the observations, prepare them for the press, and read and check the proof-sheets. A large volume of valuable observations could thus be produced every year, which would require at least double the time and money to produce by the same telescope mounted equatorially. The difference in the amount of work will be evident when we compare the number of objects observed with a transit instrument per night, with those observed with an equatorial. A hundred objects in various declinations might be examined in a single evening, while it is seldom that the same number could be identified and measured by an equatorial in a week.

ON MAXIMUM SYNCHRONOUS GLACIATION.*

By W. J. McGee.

In the development of knowledge of the cosmos, the tendency has ever been to look at first upon all phenomena as mystical and incomprehensible; and only after repeated observation and much study has it been decided that any class of phenomena may be the result of the operation of the identical laws whose existence is established by every-day observation. Thus, in geology, catastrophism prevailed long, but finally yielded to a rational uniformitarianism; in general biology the idea of special creation has only given way to that of derivation within the memory of a child; and in anthropology the mystical view yet generally prevails. The narrow domain of glacial physics, as embodied in the glacial theory, is still in the transitional stage. When that theory was first acceptably propounded by Agassiz, the details were so varied, the recognized relations so unique, and the whole conception so grand and startling, that even the more conservative of those who early became its advocates, forgot for the time the necessity for keeping all assumed data within the bounds of actual observation or legitimate induction; and hence not the least valuable of the later contributions to the theory are those which bring out its relation to established laws. Such is the aim of the memoirs bearing the above title; the particular phase of the subject discussed being that known as the "ice-cap theory."

The conclusion of Tyndall that such a supply of heat as may be necessary to produce large quantities of aqueous vapor, and an area of sufficiently low temperature to not only condense but congeal the vapor brought to it, are the first requisites for glaciation, is adopted at the outset; but it is shown that while the regions which furnish and those which congeal the vapor may be contiguous, they must be quite distinct. There is no other substance than water in the solid state which will abstract heat from the superfluitant vapor with such facility as to form, when spread over the surface, a condenser of sufficient power to meet the requirement of glaciation; and such a condenser must so far exceed in capacity any tax that may ever be placed upon it, that it will *immediately* condense and congeal all moisture that may be brought to it by aërial currents; for if the vapor is not immediately condensed it will cut off radiation from the ice below, and thus accelerate melting; and if the vapor is only condensed but not congealed it will fall as rain, and every pound of it will melt 143 pounds of ice before it is itself frozen. *The integrity of the condenser hence depends on its capacity being far in excess of the work it may be called upon to perform.* Now if a condenser formed of an ice-sheet 1,200 or 1,400 miles in diameter on any part of the globe be assumed, it is manifest that the tendency of the accumulating ice will be to form an annular belt of maximum thickness, gradually attenuating toward the center of the area; for if the vapor-laden air were not immediately robbed of its moisture in sweeping over the condenser, the marginal portions of the ice would soon be destroyed. But no matter how perfect the condenser, glaciation can never occur unless there are ample quantities of vapor supplied to it; and the greatest possible accumulation of ice at any latitude may accordingly be regarded as proportional to the moisture conveyed thither. It follows that the greatest possible accumulation of ice in polar regions can never have been nearly as vast as that at lower latitudes during the quaternary; and indeed it was probably never much greater than at present. Geological evidence, so far as accessible, corroborates this view.

Similar conclusions are reached by an independent line of investigation. Within an extensive area covered by ice or snow, both aërial and aqueous currents would be either stopped or so modified as to be practically inopera-

* Reprint from Proceedings of the A. A. A. S., Vol. XXIX.

tive as distributors of heat. The temperature would hence become approximately proportional to the solar accession, which has been computed by Much for the various latitudes, and may be roughly reduced to thermometrical degrees by means of an easily determined constant. Moreover the presence of the ice would greatly facilitate both radiation and direct reflection of solar energy. The general diminution of temperature produced in this manner is calculated for each latitude; and from a comparison of these figures with actual temperatures, as recorded by Dove, the temperature of the whole hemisphere when the ice-sheet extends to any latitude is also computed. From the several figures obtained it appears that if the globe were encrusted with ice, the crust would probably (and indeed almost certainly) never be melted unless by proper terrestrial heat; while the temperature in polar regions, as well as over much of the ice-covered hemisphere, would sink so low as to practically eliminate all aqueous vapor and effectually prevent the further accumulation of ice. The annual variations in solar intensity would not materially affect the values obtained.

Since the results reached in the manner indicated embody values widely different from those of existing temperatures, and are hence *a priori* improbable, a detailed investigation of certain meteorological phenomena is undertaken in order to verify these results. The observed and computed temperatures of the northern hemisphere are first compared, and are found to indicate that the temperature-equalizing agencies are 1.5 times as effective in summer as in winter. The effect of atmospheric dryness in diminishing the effectiveness of these agencies is then found to be still greater. The values developed in the investigation of this subject demonstrate that the climatal perturbations previously pointed out as the necessary result of the considerable extension of a polar ice-sheet do not differ in kind, but only in degree, from those whose constant occurrence is a matter of authoritative record; and analogy with observed phenomena moreover indicates that the calculated extent of these vicissitudes is in perfect harmony with the magnitude of the formulated course.

The figures obtained incidentally demonstrate the existence of an empirical meteorological law, which may be stated as follows: *Any increase in thermometrical range is accompanied by a diminution in mean temperature.* Since the law strongly corroborates the results reached by the second line of investigation, it is quite fully considered, especially in its application to the present eondition of the two hemispheres. That hemisphere whose winters occur in aphelion experiences a greater variation in solar accession and consequently in temperature than the opposite one, and hence, according to the law, ought to have a lower mean annual temperature. The southern hemisphere is so situated at present; and accordingly, notwithstanding more favorable geographical and other conditions, its temperature is lower than that of the northern hemisphere. The bearing of the law on Croll's celebrated theory of secular variations in terrestrial climate is manifest.

Since it is developed in both lines of investigation that the accumulation of glacier ice is dependent upon, and in a general way proportional with, precipitation, the maximum accumulation at any latitude may be roughly computed. The final determination is as follows:

Latitude	40°	18,594 feet.
"	50	9,777 "
"	60	5,728 "
"	70	2,800 "
"	80	1,799 "
"	90	1,440 "

It may accordingly be concluded that a sufficient accumulation of polar ice to displace seriously the earth's center of gravity, or to influence the motion of middle-latitude glaciers, never can have taken place.

The nature and course of ice motion are discussed at some length; and the phenomenon is shown to be analogous to those exhibited by all classes of substances, though generally in a less striking degree. The "viscous theory" of Forbes is adopted with some modifications; and the principal objections thereto are considered. It is also pointed out that ice-streams are necessarily in tension, and hence that the central mass of an ice-field can exercise an influence on the motion of its peripheral portions. The assumption of a vast polar ice-cap to explain the motion of the quaternary glaciers accordingly appears to be not only unnecessary but incompetent.

GLUCOSE.

By Albert E. Ebert, Peoria, Ill.

The process of making glucose, or grape sugar, is as follows: corn, after being shelled, is placed in large tubs and soaked in hot water from a day and a half to five days, or even longer, the time depending on the hardness of the grain. If fermentation is not wished, the water is changed when the substance begins to sour. It is then ground, while wet, with ordinary burr stones, and with a stream of water running into the hopper with the corn. The meal, or "chop," is then run on vibrating sieves, made of fine silk bolting cloth, also fed with streams of water. By this treatment the starch, which washes through the sieves, is separated from the gluten and cellular matter, which waste portions go over the tail of the sieves, and after passing through rollers which squeeze the mass, and return the water to the sieves, is sold for feed. The portion which went through the sieves is run into tanks and settled, the water drawn off and the sediment again mixed with clean water and treated with alkali, about one pound of caustic soda, (more or less, according to the hardness of the water), being used for each bushel of corn. This treatment separates any traces of gluten from the starch, which is then run into metal-lined troughs or gutters about eight inches deep, from fifteen to thirty-six inches wide, and usually from one hundred to one hundred and fifty feet long. These are inclined slightly, and the water runs off at the lower end, leaving the starch as a sediment at the bottom. In some factories this starch mixture goes direct from the sieves to the gutters or "tables," as they are usually called. It is left to dry somewhat in the tables, and is then shoveled out. At this stage of the process it is known as "green starch." It is quite solid, but moist, containing about fifty per cent. of water.

Up to this point the process is the same as starch-making. Starchmakers take the green starch and wash it, some several times, by mixing it with clean water and allowing it to settle, then drawing off the water, and repeating the process. It is then sometimes bleached by chloride of lime or sulphurous acid, and after washing, settled, made into blocks about eight inches square, when it is dried in a kiln. For the finer grades, about half an inch of each side of the cake is shaved off when partially dry, the rest of the cake being wrapped in paper and put back into the kiln until it forms into little sticks or pipes.

For glucose, however, the green starch is made quite thin with water, and run into converters, usually after several additional washings. The converters are large wood tanks or tubs, where it is treated with acids, sulphuric being usually used, although muriatic, nitric, or even oxalic may be substituted. Sulphuric is preferred, as it is cheap and easily gotten rid of in an after stage of the process, when it has performed its work. The acid does not combine with the starch, but merely exerts a catalytic action; therefore the necessity of providing for its removal. While under the acid treatment the contents of the converters are heated to the boiling point by

means of steam pipes coiled inside the tubs, or by steam jets. Some use pressure converters, which are iron or copper tanks like a boiler, when the conversion is much quicker. The operator makes frequent chemical tests to determine when the starch is entirely converted into sugar, and when this is accomplished the mixture is drawn into another vat where the acid is neutralized with some form of carbonate of lime, as marble dust, chalk or whiting. The liquid is sometimes bleached by the use of sulphurous acid at this stage of manufacture. It is now a very dilute solution of glucose, and besides incidental impurities, contains sulphate of lime formed by the action of the sulphuric acid on the carbonate, and whatever carbonate of lime was used in excess of the sulphuric acid present. These are separated by straining through cloth or bag filters and afterward percolating through columns of bone charcoal, eight or ten feet deep. When decolorized, it is drawn into the "vacuum pan," which is a large, strong tank of iron or copper, with steam pipes coiled inside for heating, and from which the air is partially exhausted by an air pump, and in which the syrup is boiled down at a temperature of 100° to to 145°. When concentrated to a specific gravity of about 1400 it is drawn off and again strained or filtered, and is ready for the market as glucose, this being the commercial term for the syrup only. The term grape sugar is applied to the dry glucose, and this is produced by carrying the conversion further before neutralization.

The syrup is used, principally, for mixing with dark colored cane syrup for making light colored table syrups (nearly all the table syrups now sold contain it, and frequently from 75 per cent. to even a larger quantity), and also in making wine, ale, beer and vinegar. On a smaller scale it is used in tobacco manufacture, the adulteration of honey, fruit preserving, etc. Both the solid and liquid forms are largely used in candy making, for which it has several marked advantages. A syrup is prepared expressly for this use, in which the conversion of the starch into sugar is only partial, the syrup containing, of its solid matter, about eighty per cent. of the intermediate product, dextrin, and twenty-of glucose. The large consumers of glucose require slightly different syrups. Wine growers, for instance, use a syrup free from dextrin. Brewers desire a very small proportion of it, to give body to the beer, while vinegar makers use a syrup free from gum. The dry glucose, or grape sugar, seems, aside from its legitimate use in candy making, to be most largely in demand for the adulteration of cane sugar. No objections, save of a moral and financial nature, can be urged against this, but it is well to remember that for its value as a sweetener, compared with cane sugar at ten cents per pound, glucose is worth but four cents. So much has been written against the manufacture of glucose, on account of its use as an adulterant of cane sugar, that it is, perhaps, only just to say that it is certainly the least objectionable of any of the articles used for that purpose. It is perfectly wholesome, being in fact the physiological sugar, and has about two-fifths the sweetening power of cane sugar, which is more than can be said of terra alba, starch, bone dust, sand, etc., while its most probable impurity, calcium sulphate, can, from its insolubility, be present only in minute quantity, probably not more largely than in most potable waters, and is not in any sense noxious.—*The Druggist.*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING NOV. 19, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

NOVEMBER.	BAROMETER. Mean for the Day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun
Sunday, 13	29.636	29.790	12 p. m.	29.542	1 a. m.	51.6	49.0	59	0 a. m.	58	0 a. m.	42	12 p. m.	42	12 p. m.	115.
Monday, 14	29.937	30.002	9 p. m.	29.790	0 a. m.	46.3	43.6	53	2 p. m.	48	2 p. m.	40	5 a. m.	40	5 a. m.	106.
Tuesday, 15	30.214	30.442	12 p. m.	29.976	1 a. m.	41.3	38.3	46	4 a. m.	43	3 a. m.	36	8 a. m.	34	8 a. m.	104.
Wednesday, 16	30.500	30.550	9 a. m.	30.442	0 a. m.	39.7	38.0	45	4 p. m.	41	4 p. m.	33	6 a. m.	33	6 a. m.	101.
Thursday, 17	30.327	30.464	0 a. m.	30.138	12 p. m.	47.6	45.7	55	3 p. m.	51	4 p. m.	37	8 a. m.	37	8 a. m.	110.
Friday, 18	29.869	30.138	0 a. m.	29.690	12 p. m.	58.3	55.6	61	2 p. m.	58	12 p. m.	52	0 a. m.	50	0 a. m.	82.
Saturday, 19	29.669	29.798	12 p. m.	29.600	1 p. m.	50.3	49.3	61	0 a. m.	58	0 a. m.	45	12 p. m.	43	12 p. m.	62.

Mean for the week .... 30.021 inches.
Maximum for the week at 9 a. m., Nov. 16th .... 30.550 "
Minimum " at 1 a. m., Nov. 13th .... 29.542 "
Range .... 1.008 "

	Dry.		Wet.	
Mean for the week	47.8 degrees		45.6 degrees.	
Maximum for the week, at 2 pm. 18th	61.	"	at 12 pm 18th, 58.	"
Minimum " " 6 am. 16th	33.	"	at 6 am 16th, 33.	"
Range " "	28.	"	25.	"

NOVEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in Miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of Vapor. 7 a. m.	2 p. m.	9 p. m.	Relative Humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of Rain and Snow in Inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0 10
Sunday, 13	w. s. w.	w. n. w.	w. n. w.	241	7	3.00 pm	.389	.282	.275	93	62	92	0	1 cir. s.	0	-----	----	-----	--	0
Monday, 14	w. s. w.	w.	e. n. e.	197	7¼	3.30 pm	.235	.269	.251	91	66	84	0	7 cir. cu.	0	-----	-----	-----	--	5
Tuesday, 15	w. n. w.	n. n. w.	n. w.	369	19½	7.30 am	.190	.186	.203	74	67	82	7 cu.	4 cu.	0	-----	-----	-----	--	2
Wednesday, 16	n. w.	w. s. w.	w.	143	1	2.00 pm	.188	.208	.231	100	75	83	0	0	0	-----	-----	-----	--	0
Thursday, 17	s.	s.	s. s. w.	170	4¾	9.30 pm	.229	.295	.334	100	73	86	0	1 s.	7 cu.	-----	-----	-----	--	2
Friday, 18	s. w.	w. s. w.	s. s. w.	258	5¾	11 15 am	.362	.412	.456	86	77	88	9 cu.	9 cu.	10	-----	-----	-----	--	0
Saturday, 19	n. n. e.	n.	n. w.	118	6¼	8.00 pm	.335	.374	.309	92	100	85	9 cu.	9 cu.	10	5.15 pm	9 pm	3.45	.03	3

Distance traveled during the week .... 1,496 miles.
Maximum force .... 19½ lbs.

Total amount of water for the week .... 0.05 inch.
Duration of rain .... 3 hours, 45 minutes

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

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SATURDAY, DECEMBER 3, 1881.

SUGAR ANALYSIS*

This is an admirable manual of sugar analysis, and will prove a great boon to everyone engaged in sugar work. The *resumé* of the chemistry of the sugars with which the book opens is of especial value to the student who wishes to get a clear idea of this complicated subject. Perhaps it had been desirable to have had the book delayed a little longer in order to have incorporated the results of the last year's study in sugar analysis, but this objection would obtain equally against any book published at any time. Another valuable feature of the book is its collection of tables referring to all conditions of sugar analysis, viz, specific gravities, solubilities, etc. The author is careful to cite authorities for his statements, and thus anyone wishing to pursue any given topic further can readily do so without being at the trouble of hunting up each theme for himself.

There is, however, a vast mass of French and German literature on certain sugar compounds which might be very appropriately drawn upon in an exhaustive study of the chemistry of sugar, and which is of no use whatever to the analyst. There is much of this in the book before us, and while it detracts nothing from its merit as a help to the analyst, it certainly adds nothing to it.

That portion of the work which is devoted to the description of the optical examination of sugars is to be highly recommended. We have, however, used for three years a Schmidt & Hänsch polariscope, and were therefore a little startled to read "ordinary lamp light, and not the monochromatic flame, is required."

We doubt very much whether, in testing the accuracy of the scale of a polariscope, quartz plates of various thicknesses are better than solutions of pure sugar. First of all, the plates themselves would have to be tested, and this would require as much work and trouble as testing the scale directly with sugar solutions. If quartz plates could be secured which were absolutely accurate, of course this objection would not occur.

Among the sources of monochromatic light the author omitted to mention the new double burner of Laurent, which leaves nothing to be desired in the steadiness and intensity of the sodium flame.

There is one statement which the author makes in a note (p. 137) on Clerget's method of analysis that seems calculated to mislead. It is: "it must be remembered that the process is entirely inapplicable when any optically active body is present besides cane or invert sugar, and also if the invert sugar itself exists in an inactive condition as regards polarized light." In point of fact, any optically active body may be present without rendering the process inapplicable, provided it is not affected by the process of inversion. Thus, by Clerget's method we can accurately determine cane sugar in the presence of dextrose, maltose and glucose. In polarizing an inverted cane sugar, too, metal tubes should be used, since the temperature is more accurately obtained from an external thermometer than in a glass tube.

The author's directions for estimation of raw sugar and syrups are those which are generally recommended and employed. The description of these methods is full and admirably arranged. In fact, this praise can be bestowed on every part of the work. The only trouble about the methods is that if applied to the ordinary syrups of commerce they will give the most alarming errors.

The great fault of the work, in fact, is found in its failure to give reliable methods for the examination of the mixed sugars and syrups which are on the market to-day.

Perhaps, however, we should not say this is a fault of the book, it is rather a fault of science. To determine cane sugar, invert cane sugar, dextrose, dextrine, and maltose *exactly*, is a great problem which the author leaves untouched and which demands the careful attention of sugar chemists.

In papers read before the A. A. A. S., at the Boston and Cincinnati meetings, and published in the proceedings for the Boston meeting and in this Journal, Nos. 65 and 66, Prof. Wiley has shown the relation between rotating and reducing power in commercial starch sugars and also a series of determinations of cane sugar in mixed sugars.

Since the polariscope has grown to be the chief instrument in sugar analysis and starch sugars and syrups a common article of commerce and consumption the omission of any reference to those papers is a matter to be regretted.

* Manual of Sugar Analysis, by J. H. Tucker, Ph. D., Van Nostrand, New York, 1881.

NEW YORK ACADEMY OF SCIENCES.

November 7, 1881.

REGULAR BUSINESS MEETING.

The President, Dr. J. S. Newberry, in the Chair. Twenty-nine persons present.

A paper by Prof. P. T. Cleve, University of Upsala, Sweden, was read, by Prof. D. S. Martin, entitled

OUTLINES OF THE GEOLOGY OF THE NORTHEASTERN WEST INDIA ISLANDS.

(Abstract.)

Prof. Cleve's paper contained a resumé of his observations made during 1868–9, in and around the Virgin Islands, and published in the Swedish language in the *Trans. R. Acad. Sci.* of Stockholm, in 1871. He regards the whole group as of Cretaceous and Tertiary age, with the exception of Anegada, which, like the Bahamas, is post-pliocene.

The strike of the rocks, and the trend of the entire group, are approximately east and west. The rocks are various, largely eruptive and metamorphic. Of these, Prof. Cleve discussed somewhat fully the character and distribution of the following kinds:—1, Diorite; 2, Felsite; 3, "Blue-beach" (a peculiar volcanic breccia, locally so-called); 4, Diabase.

All these rocks have great thickness, and indicate long-continued volcanic activity. As in modern lavas, they present two types, basic and acidic.

Metamorphic slates are next described; and then a partly metamorphic limestone, occasionally with recognizable fossils, sufficient to fix the age as certainly Cretaceous.

Santa Cruz Island is then described, and referred to the same series as the Virgin group. All these islands thus indicate, by their east and west strike, and the great upturning of their rocks, that they were formed by a north and south pressure, forcing the Cretaceous and associated volcanic beds into a great line of anticlinal and synclinal folds. This period seems to have been about that of the white chalk; but the force continued to act during the succeeding Eocene time, though with diminishing intensity, as is shown by the less inclination of the Eocene beds. The Miocene strata are little disturbed, and the force would therefore seem to have spent itself by that period.

Prof. Cleve then refers briefly to the occurrence of similar metamorphic and volcanic rocks in the interior of the Great Antilles, and regards the entire series as having been formed by the same general movement of Cretaceous folding, the Virgin Islands forming the eastern extension of the line of elevation.

The Eocene strata are then taken up and discussed, as they occur in the islands of St. Martin and St. Bartholomew, just east of the Virgin group. Professor Cleve regards these islands as wholly of Eocene age, claiming that the eruptive rocks of which they mainly consist, are *interstratified* with the limestones, which contain fossils of the age of the Calcaire Grossier, of the Eocene of Paris. He then traces the occurrence of Eocene strata in Antigua, Guadaloupe, parts of Trinidad, and largely in Jamaica; and re-affirms his conclusion that the movement which raised the Great Antilles and the Virgin islands continued during the early Tertiary, though with lessening force.

The Miocene formation is then considered. It forms the small island of Anguilla, and occurs on several of the islands, south to Trinidad; but has immense development in the Great Antilles. It is chiefly a limestone series, is generally little altered from a horizontal position, and at times may be seen resting uncomformably on the Eocene. By this time, evidently, the disturbing movements had ceased to make themselves felt.

The later Tertiary rocks, Pliocene and Post-pliocene, have not been very clearly marked off from each other or from the Miocene. But to the Post-pliocene period are referred the Bahamas, Anegada, and the remarkable series of volcanic outbreaks that characterize the islands of Saba, St. Eustatius, St. Kitts, Nevis, Monserrat, Guadaloupe, &c. On St. Kitts, Prof. Cleve describes a limestone with over forty species of fossil shells, all but one of which are identified with living species of the Caribbean sea. The same is true of Anegada.

The elevation of the Miocene strata of the Great Antilles took place apparently by a "continental" uplift, whereby large areas of marine deposit were raised without folding or disturbance. Professor Cleve suggests that this movement may have been accompanied by a sinking of part of the sea-bottom in the Caribbean region to the south-east, and that on the limit between the areas of rise and of depression, fissures and faults may have occurred, through which these volcanic outbreaks of the Leeward islands found exit, in the Post-pliocene time.

DISCUSSION.

Mr. A. A. Julien confirmed the accuracy of these petrographical distinctions of the rocks of the Lesser Antilles, from the results of observation during a residence of four years on Sombrero and vicinity. The island of St. Eustatia consists mainly of volcanic ashes in a thick tabular and horizontal stratum with vertical faces along its coast. This is flanked on the south end by a volcanic cone with extinct crater, of which the bottom is occupied by a plantain plantation, but the sides are bare, and consist of a dark basaltic rock; and on the north end by two lower cones, not visited but probably volcanic. On the island of Saba the rock is light colored, rich in crystals of sanidine, and apparently a trachyte, constituting a remarkably sharp volcanic cone, with its sides deeply furrowed from top to bottom by eroded ravines; certain depressions upon the summit, resembling craters, present in some localities sulphur deposits which have been found of commercial importance.

However, the conclusion of Prof. Cleve, as to the recent age and eruptive character of most of the crystalline rocks of this region, appeared surprising in view of their metamorphic associates, and of their similarity to those of the Archæan areas identified by Hartt in Brazil. It was a question whether a nucleus of Archæan, or, at latest, metamorphic pre-Silurian rocks, in general highly tilted, does not form the axis of such islands as St. Martin, St. Barts, etc.

Prof. D. S. Martin questioned whether a corresponding movement of disturbance should not be also found in the Cretaceous strata of a region no farther removed than that of the vicinity of our own Gulf coast.

Dr. J. S. Newberry remarked that the importance of the subject of the age and origin of these crystalline rocks still demanded their re-examination and a review of Prof. Cleve's conclusions by some worker of experience in this peculiar field. One of the most interesting topographical features on this continent consisted in the line or axis of elevation marked by the Windward Islands, separating the deep basin of the Gulf of Mexico on the one side from the abyss of the Atlantic Ocean on the other. It presents a prolongation and connection of the mountain chains which run along the eastern border of the North and South American continents, in a course imperfectly parallel to that on the western border of these continents, with the gulf lying enclosed between these two great ranges. This axis has been the scene of violent volcanic action and has been supposed to mark the place of that mythical area of sunken land, styled Atlantis by the ancients. The tradition long current, recorded by Herodotus and others, points to a densely populated land west of Europe, covered with cities, and threatening the civilization of the Eastern hemisphere, which was punished by the gods by being sunk beneath the sea. According to the recent observations of an

English geologist, Mr. Thomas Belt, this legend may have had some foundation in the former existence of a continent, now submerged beneath the Caribbean sea, through which the peaks represented by the Lesser Antilles, constituted a mountain chain. Local disturbances have certainly affected this area, but we fail to find any evidence of corresponding disturbance in the Cretaceous strata of our southern States. except perhaps in continental elevations and depressions. Messrs. Guppy, Gabb, and others have studied the rocks of the region, but, up to this time, no one trained to the examination of the difficult phenomena and problems under discussion.

NOV. 14, 1881.

The President, Dr. J. S. Newberry, in the chair. Twenty-four persons present.

A paper was read by Dr. Alexis A. Julien on

THE EXCAVATION OF THE BED OF THE KAATERSKILL, N. Y.

(ABSTRACT.)

This paper was supplementary to one read before the Academy two years ago, concerning the phenomena of erosion, glaciation, etc., in the Catskill Mountains, in the vicinity of the Kaaterskill Clove.

Flexure of Strata.—Prof. James Hall has indicated the existence of four lines of flexure, running from N.E. to S.W., the synclinals occupying the summits of ranges, and Prof. Arnold Guyot locates one of these at Slide Mt. The dips at the entrance of the Clove vary from 8° to 10° to the W. N.W., becoming only 3° four miles to the westward, *i. e.*, more nearly horizontal towards a shallow synclinal fold supposed to occupy Hunter Mt.

One of the most interesting discoveries of Guyot was the linear series of three maxima of altitudes above 4000 feet, Slide Mt.; Hunter Mt. and Black Dome. The gentle flexure of the whole stratum required to produce this line of maxima may be thus shown in the range running S.E. and N.W. through Hunter Mt., 35 miles long. Toward the S.E., the descent from the crest of Hunter Mt. (Alt., 4038 feet), to Overlook Mt. (3150 feet), is 888 feet, in 9½ miles, equivalent to 1 in 56, or about 1°; toward the N.W., from Hunter Mt. to Utsyanthe Mt. (3203 feet) the descent is 835 feet in 25 miles, equivalent to 1 in 158, or less than ½°.

Another similar series of maxima, however, occurs further to the westward, consisting of Graham Mt. (3886 feet), Bear-pen Mt. (3545 feet), and Ashland Pinnacle (3420 feet), distant respectively 9, 12, and 15 miles westward of the former series. This southward convergence of the axes of these two folds may probably account for the increased protuberance and greater elevations in the Southern Catskills.

Newly determined altitudes.—Many new determinations have been made of points in the vicinity of the Clove by means of an excellent aneroid, with constant reference to the numerous stations in the vicinity whose altitudes have been accurately obtained by Guyot. A few are here subjoined:

	Feet.
Hotel Kaaterskill, on South Mt	2466
Parker Hill, summit	2565
Parker Mt., "high ledge."	2874
Clifton House	2101
Newman's ledge, on North Mt	2486
Gap between E. and W. peaks, North Mt	3116
Toll-gate on Mt. House road	760

Glaciation of summits.—All the crests near the Clove have been now examined. On none above an altitude of 2900 feet have glacial striæ been found, in part because they consist of thinly laminated flags deeply disintegrated by frosts. The highest striæ discovered were found on Parker Mt., "High ledge" (2874 feet), running S. 18° W. (magnetic), and under the roots of a large tree on the SE. slope of Round Top, at an elevation of 2871 feet, running S. 35° E. However, in all cases, a marked difference exists in the slope of different sides of a peak, the E .and S.E. sides presenting a precipitous face, and the other sides more or less of a gentle slope.

The highest striæ yet found in the Catskills occur on Overlook Mt., at an elevation of about 3100 feet, implying a depth of ice in the Hudson river glacier of about or at least 3200 feet. Within the Kaaterskill basin, several miles distant from the Hudson valley, the overflowing ice stream became shallower, having an altitude of about 3000 feet. It thus appears that the surface of the glacier inclined westward over these mountains, with a slope of 200 feet in 3 miles, 1 in 84, say about ½°.

The conclusions of the former paper have been confirmed by recent observation, viz., that two glacier streams have swept over these mountains, the Continental Glacier from the N. W., submerging and carving out the highest peaks, and the Hudson Valley Glacier from the N., later, more shallow, bearing along vast quantities of materials derived from the crystalline and lower Silurian rocks of the Adirondacks and of the Helderberg Mts., and strewing the whole region with their boulders; and that no local glaciers have existed in the Catskills after the retreat of the Hudson Valley Glacier.

Tilting of the Catskill plateau.—In the previous paper an explanation had been given of certain facts which seemed to indicate that the whole formation had been gently inclined to the East and then to the South-east, before assuming its present W. N. W. inclination, at a period far anterior to the Glacial epoch. A profile section of the ancient Kaaterskill valley, was exhibited, reaching from Haines' Falls nearly to the junction of the N. and S. branches of Schoharie creek, proving the gentleness of the slope, the absence of rock, and the existence of a deep and narrow buried cañon, now filled up with moraine material and a capping of peat.

A comparison of the altitudes of Prattsville (1164 ft.), a point on the Western axis, 12 miles distant from the Kaaterskill Clove, and of the lip of the stratum above Haines' Falls, (1857 ft.), at the head of the Clove, shows that a depression of the latter point below a line connecting these two points, even to the extent of a single degree, would cause a descent of nearly 700 feet from Prattsville to Haines' Falls, *i. e.*, toward the East. The excavation of the deep Kaaterskill and Plaaterkill Cloves could hardly have been effected by the small streams now occupying their beds. It is more probable that the Schoharie creek formerly flowed, at a higher level, to the east into the Kaaterskill Clove, and afterwards to the south-east into the Plaaterkill Clove, before the latest tilting of the plateau to the W. N. W. caused a reversal of the flow of the stream, in the very opposite direction, through the greater part of the same valley. An objection to this theory presented itself in the obstacle which has created a turn to the S. W. in the North branch of Schoharie creek, near its junction with the South branch. But on recent examination this was found to consist not of rock but of a huge mass of coarse moraine material deposited during the Glacial period on the southern slope of the Schoharie valley.

Sculpture of the plateau.—In a terrane consisting of strata which dip at varying and perhaps very high angles, the carving out of ranges and production of ravines and gaps may generally be assigned to the occurrence of flexures, of dykes or faults, or of beds whose material is unusually soft, fragile, or rich in minerals of easy decomposition. But the problem of topographical sculpture is less easily solved in a stratum like that of the Catskills consisting of a regular succession of layers which are horizontally homogeneous and from which the phenomena of disruption are absent. The original disintegration and erosion of the mass which resulted in the production of the ranges was perhaps mainly influenced by the direction of the jointage. With this the trend of the ranges in the vicinity of the Kaaterskill Clove appears to

coincide. The ravines, cloves, and deepest notches and valleys may be attributed to the streams of the present hydrographical basins, or to those connected with the ancient eastward and south-eastward inclination of the stratum already considered. But recent observations on the juxtaposition and coincidence of the highest gaps in successive parallel ranges may possibly indicate the remnants—in cross-section—of the beds of ancient streams at that level (about 3000 feet); this conclusion, if confirmed, would signify an inclination of the plateau to the N.N E. (or to the S.S.W.?) at a still earlier period, that immediately succeeding its elevation.

Kames.—In the upper basin of the Kaaterskill, several isolated hills of gravel, etc., occur at an altitude of 1924 feet, especially on the bank of the stream near the head of the Clove, which are probably kames; their materials, though largely angular, show traces of imperfect stratification. Near "Blythewood," on the North branch of the Schoharie creek, a curious conical and steep isolated kame rises 102 feet above the stream, made up of rounded pebbles of the Catskill grit, rarely a foot in length, overlying a layer of coarse moraine. Its elevation above the sea (1944 feet) exceeds that of any other kame yet observed, those of the Fintry Hills in England reaching 1280 feet, and those of the Androscoggin Lakes, in Maine, 1600 feet. A very interesting series of from eight to twelve very low kames—like parallel ridges, often curving, made up of large rounded boulders—was also found to follow the course of the Kaaterskill near Palenville, in the Hudson Valley, at the mouth of the Clove, at an elevation of about 700 feet; these probably mark the course of the sub-glacial stream which issued from the mouth of the Clove. The paper concluded with observations on a deposit of laminated sand underlying the ground moraine: on the feeble erosion of the slopes of the Clove during the period which has elapsed since the close of the Glacial epoch; and on a new section of the strata of South Mountain obtained from a new road-cutting.

DISCUSSION.

Prof. E. C. H. Day observed that one portion of Dr. Julien's remarks reminded him of an idea which had struck him many years ago with regard to the surface geology of a valley on the south coast of England, near Charmouth, in Dorsetshire.

The stream in the valley referred to finds its way to the sea through a narrow pass, which, as attested by the rapid wearing of the coast line and its present configuration, could only have been of (geologically speaking) very recent origin. How the valley could have been drained prior to the existence of this outlet was a question which might be met by various hypotheses, and one of these was that there might have been a slight unequal local change of level, sufficient to have had the effect of tilting the surface of the valley so that its waters were shed then in a direction opposite to that which they now take. This was nothing more than the veriest hypothesis made many years ago, without any subsequent attempt at verification. It may suggest, however, the possibility of such slight local changes occurring, in addition to the greater movements already distinctly recognized, and the desirability of careful investigation to discover whether such may not be traced in the altered direction of streams and in the existence of ancient and unused water courses—even in our own neighborhoods. It may be added that such local tiltings of parts of the earth's crust would necessarily influence the course of subterranean as well as of subærial waters, thus altering the distribution and force of springs at the surface.

Dr. J. S. Newberry stated that the Catskills presented a more complex bit of topography and geology, and one that had been more discussed than perhaps any other of similar area in the country. It was once supposed that these mountains were composed of a single geological formation, which, from this fact, was called the Catskill group; and it was supposed to be a detached table land, deeply carved by erosion. The late Col. Jewett, of Albany, found strata containing Chemung fossils in the Catskills, and from this inferred that the mountains were composed of Chemung strata. Prof. Hall and Prof. Guyot, with their assistants, then made a careful study, running through several years, of the topography and geology of all the surrounding region. Their labors established the fact that the Catskills are not an isolated mountain group, but belong to the Alleghany system and are formed by a series of folds or arches composed of the Chemung and Catskill rocks. Of these folds, the convex arches, as is usually the case, were cracked and broken and, therefore, yielded readily to erosion, while the concave arches, protected and solid, yielded less readily and, in time, by the wearing away of their surroundings, were left in relief, forming ridges with a synclinal structure. Hence it will be seen that the topography of the Catskill region is chiefly the result of erosion.

So far as regards the changes of level from subterranean causes, referred to by Mr. Julien, it would certainly be strange if the foundations of the Catskills were proved to be stable. The old name, "*terra firma*," once applied to the crust of the earth, is a complete misnomer, and it is really a type of instability. Probably throughout the globe local subsidence and elevation are constantly in progress. In the interior of continents we have no evidence or measure of these, but along coast lines the water line tells us that changes are constantly and everywhere taking place, in the relative level of land and sea. About New York the coast is sinking, though very slowly, while further north, in places, it is rising, and Greenland is sinking again. Back from the coast there is no such nilometer, and yet we have no reason to suppose that the earth is more fixed. Some indication is given by the reports of those who dwell in mountainous regions, of changes of level, which have shut from their view that which was before visible or revealed what was before concealed, but these observations have not been made with accuracy and cannot be depended upon.

In a recent paper before the Academy he had shown the vast changes which had occurred along the coast in this vicinity, viz., that the land once stood 600 feet higher than at present: that the Hudson river had then flowed by the city through a channel from 300 to 500 feet deep, now in large part silted up: that the Palisades then stood from 700 to 800 feet above the river: that the Housatonic then flowed through the East river into New York Bay: that a sub-tropical climate then prevailed throughout this region, with a varied and rich fauna and flora, extending up even to the Arctic Sea: that then a depression of the temperature and great change in the climate ensued, with a corresponding alteration of the fauna and flora; but that these changes were very slow and progressive—the snows, which at first rested temporarily upon the Catskill Mountain summits, became at last permanent, and resulted in local glaciers. These glaciers produced extensive erosion, cutting the channels along which they moved, deeply. A partial obliteration of their work then ensued through two agencies. First, a continental glacier advanced southward, overtopping all the mountains, grinding down the asperities of the surface, fillng old valleys, and banking up a great mass of debris along its margin—a part of which is now Long Island. Afterward the climate becoming milder, local glaciers were again formed similar to those which preceded the great Glacier, and partially obliterated or modified the results of the ancient erosion. It is a complex problem now to distinguish between the phenomena which have been respectively produced by all these glaciers in varied succession, by the erosion of streams, by flexures of the earth's crust, etc.

The excavating power of glaciers had been denied by Prof. Whitney, but ice, hundreds of feet and sometimes miles in thickness—as it was in the old glaciers—moving

with irresistible force, and having sand, gravel and boulders beneath it, or frozen into it, was the most potent agent of erosion known. The eroding power of the ancient glaciers, which once reached southward to Trenton and Cincinnati, was attested not only by the planed down rocks, but by the immense sheet of transported debris left by the glacier in its retreat.

The glaciated, planed, and polished rocks in the Western States are generally covered by a thick layer of clay, abounding in glaciated boulders. There are also other water-worn materials which have been transported, perhaps thousands of miles, representing the gravel bars, sand beds, etc., produced by sub-glacial rivers. Although the materials are entirely of glacial origin, all the stones are here usually rounded. We find in these deposits, called kames or eskers, the evidences of the action of running water produced by the melting of ice, their accumulation in heaps, ridges, etc., having been effected by local causes, waterfalls, streams upon or under the ice, etc.

The finer material produced by the same grinding action has been deposited along our coast in the vast masses of the Champlain clays. It is well known that the drainage of all glaciers results in milky streams; *e. g.*, those which descend from the Alps impart an opalescence to the Lake of Geneva, and the streams from the Cascade Mountains are clouded with silt derived from the small glaciers at their heads. So, during the Glacial period all the fine material was sometimes washed out of the glacial drift, leaving banks and ridges, kames, hogsbacks, etc., of gravel and boulders, and carried by streams to the coast and there deposited along shore in the Champlain clays. The fine flour and bran ground by the glaciers have been sometimes referred to different epochs, but they are produced simultaneously. The Glacial or Champlain clays are of great economical importance to the city as they are the brick clays of Croton Point, Haverstraw Bay, and other points along the Hudson. Their thickness reaches 100 feet along the lower portion of the Hudson river, 400 feet on Lake Champlain, 500 feet at Montreal, 800 feet at Labrador, 1000 feet at Davis' Strait, and 1800 feet at Polaris Bay. This indicates that the continent was depressed to this extent at each of these points, that the waters of the ocean extended through these valleys, and that here was dead water into which the glacier drainage flowed and was deposited.

In the vicinity of New York City it is evident that the glaciers everywhere overrode and disregarded the underlying topography. All the surface of the island is strewn with materials derived from the N. N. W., and the rock has been planished and striated with grooves running in that direction. The hills back of Yonkers are covered by trap boulders which have been conveyed across the river from the Palisade range on its western side, and it is plain that the Glacier completely disregarded the depression of the Hudson valley, filled it up to a greater or less extent with *debris*, and so rode smoothly over it. Afterwards this and the other valleys were more or less cleared out by the present streams, but a portion of their contents is generally left in their beds, the tunnel between this city and Hoboken being now driven in fact through a part of this clay deposit. On the east side of the city a narrow cañon, 300 to 400 feet deep, has been proved to underlie the East River; and it would have been a wiser and cheaper plan to construct a tunnel through the clay bottom, for communication with Brooklyn, in place of the present costly and to some extent insecure bridge.

Dr. Newberry finally expressed his interest in the careful study of the erosion and sculpture of the Catskills and desire for its continuance.

MRS. J. M. FISKE has left a bequest of $40,000, to establish a hospital for the use of the students of Cornell University.

RINGING FENCES.*

By PROF. S. W. ROBINSON, Ohio State University.

THIS sketch is mainly of a simple fact of observation. My attention was one day suddenly arrested while walking on a hard road alongside a picket fence by the peculiarity of the sound which reached my ear immediately following each step. This sound was first noticed to be very different from that perceived at other parts of the sidewalk. On instituting an inquiry for the cause of this difference the only one discoverable was a change in the construction of the yard fences along the sidewalk.

The peculiarity observed in change of sound was very marked when passing from a portion of the sidewalk opposite a board fence to parts opposite a picket fence. In the former position a quick drop of the foot upon the walk was accompanied by a simple sound or noise of short duration. But when opposite the picket fence the noise following each footstep was prolonged into a curious musical tone of initial high but rapidly lowering pitch, and with a duration of perhaps a quarter of a second.

This singular musical tone following, and due to the noise of a simple foot step, could only be accounted for on the supposition that each picket of the fence reflected the sound reaching it from the foot, the rapid succession of which, from the several pickets of the fence, resulted in the sound observed.

The duration of the sound reflected from the pickets at each step is evidently due to the different distances of the pickets from the ear of the observer, and the greater length of time required for the sound to travel to and from the more distant pickets. For instance, suppose the observer is walking along a stone or mastic walk at a distance of eight feet from the fence, the latter extending either way some distance along the street. The sharp noise of the footsteps returns from the nearest pickets first. Here the differences in distance from the adjacent pickets is slight, and hence the succession of reflected noises is rapid. But from more remote pickets the difference of distance is greater, and the succession in reflection less rapid.

In studying the nature of the resulting tone it is at once seen that the initial pitch is due to an almost infinite number of reflections or vibrations per second, while at the end of a quarter, or half, second the lines of advance and return of the sound are nearly parallel to the fence, and hence the pulsations have an interval of time equal twice the constant distance between pickets divided by the velocity of sound. For instance, if the pickets be four inches apart, or one-third of a foot, the terminal pitch would be one of about 1500 vibrations per second. The law of retrogression of pitch may be of interest. To express it as a function of passing time, let

d = distance from observer to fence.
a = constant distance between pickets.
v = velocity of sound.
n = number of vibrations per second.
t = the time following the initiation of the reflected tone.

Then by aid of a diagram we easily obtain the following relation between the above quantities, viz.:

$$\frac{v^2}{4\,a^2\,n^2} = 1 - \frac{d^2}{(v\,t + d)^2}$$

The curve for this equation is not easily classified. But by computing quantities and constructing a curve it is found to be very much like a hyperbola referred to its asymptotes, which indicates that the pitch falls rapidly at first, and less so subsequently.

Not only is the above described phenomenal reflection observed in connection with fences, but from any series of flat surfaces in steps, as, indeed, in the case of stairs under proper conditions. Such echoes have been observed from the steps in front of the State House, at Columbus, O.

*Read at A.A.A.S., Cincinnati.

THE AYE-AYE OR CHEIROMYS OF MADAGASCAR.

During the present year the Menagerie of the Botanical Garden has been enriched by the addition of several rare animals, and among others, by that of three Lemurs of Madagascar belonging to the strange species commonly called Aye-Aye, and scientifically *Cheiromys Madagascariensis*. The Museum of Natural History already possessed in its collection, not only the same species, obtained more than a century ago, by the explorer Sonnerat, but also several specimens preserved in alcohol or reduced to skeletons, which have been presented at more recent dates by M. de Lastelle, M. A. Grandidier and M. Meurand. The study of these different specimens and of those which the British Museum has procured, has at length led naturalists to the discovery of the true affinities of the Aye-Aye, and has decided the place it should occupy in classification, namely in the order of Lemurs. But, before coming to this conclusion, there was a great deal of hesitation and groping in the dark. Sonnerat, who discovered the Aye-Aye in Madagascar, considered it a squirrel having some connection with the Makis, soon after, Buffon found in it certain resemblances to the Tarsier, then Gmelin placed it decidedly in the genus *Sciurus* or Squirrel, under the name of *Sciurus Madagascariensis*; later still, E. Geoffroy Saint-Hilaire made a particular genus for this strange animal, namely *Daubentonia*, which G. Cuvier, agreeing with Geoffroy, afterwards changed to *Cheiromys*; finally Blainville maintained that the Aye-Aye belonged to the order of Lemurs, and he was successful, though not without difficulty, in establishing his views.

The Aye-Aye has nearly the height of a cat and resembles in a slight degree the Feline tribe in its short and globular head, erect and uncovered ears, eyes very open, nostrils oblique and pierced at the extremity of the nose, although it resembles a Squirrel in its slender toes, the color of the hair, and its bushy tail. But it differs entirely from both of these animals by the arrangement of its paws. In the Aye-Aye, indeed, the posterior limbs terminate as in the great majority of the Lemurs, in real hands, the thumb, however, being a little less developed than in the latter animals, and the anterior limbs present, at their extremities, singular anomalies. Here the thumb is no longer opposable to the other fingers and it carries, as the latter, a true claw, the middle finger is so slender that it seems withered, the fourth finger is a little less slender but still larger than the other, and finally, the little finger is very extended.

The dentition of the Aye-Aye differs in several respects from that of the other Lemurs; there are, indeed, two strong and sharp incisors in each jaw, four molars on each side of the superior maxillary and three molars only in the inferior maxillary; but no trace of canines, superior or inferior, can be seen in the adult, so that a wide space separates the incisors from the molars. Yet, this anomaly does not exist to the same degree at all periods of life, and, in the young, the dental system is least removed from the ordinary type, owing to the presence, in the superior maxillary, of a pair of small incisors.

Arrived at its full development, the Aye-Aye measures more than a metre from the end of the nose to the extremity of the tail, whose length is about equal to that of the body. It has smooth and slightly wart-like ears, uncovered nose, and lips ordinarily half open, exposing to view the incisors which, meeting two by two in a very prominent angle, resemble the beak of a parrot. Hair, bushy and very long, covers the whole of the body, the limbs and the tail, but does not present in every spot a uniform color; the hairs of the head and of the back are often white at their ends, while those of the breast and of the flanks are of a more or less brown, deepened by a yellow base. In the young, the whole front is also of a silvery white, and the dorsal line is marked by a band of the same color.

In Sonnerat's "Voyage aux Indes et à la Chine," some of the principal characteristics of the Aye-Aye are spoken of, but the portrait leaves much to be desired, and the author mentions but a few things on the habits of this curious representative of the fauna of Madagascar. This animal, says Sonnerat, appears to burrow, it does not see in the day, its eye is reddish and fixed like that of a screech-owl. It is very lazy and, consequently, very gentle. I have had the male and female and they lived only two months. I fed them with boiled rice, and they used in eating, the two slender toes of the fore-feet as the Chinese use chop-sticks. They are timid, fearful, like a great deal of heat, always roll themselves up in sleeping, lie on the side, the head between the fore-limbs. They were always lying down, and it was only on shaking them several times, that they would move at all.

Although this animal is very slow in movement, and seems to be torpid during the day, it has no relation to the *Unau* and the *Ai* of M. de Buffon. The name of Aye-Aye, which I have kept for it, is a cry of exclamation and astonishment among the inhabitants of Madagascar. This animal has been known to us but for a few years, because the western side, the part which it inhabits, is but little frequented; the inhabitants of the eastern side assured me that it was the first they had ever seen.

Fortunately, the successive observations on the *Cheiromys* at the Zoological Gardens at London and at Paris have completed the information given by Sonnerat on the manners and the diet of this species. The Aye-Aye is essentially a nocturnal animal; in captivity, it sleeps during the whole day, lying on the side, its body curled up and entirely covered by the bushy tail. During the night, on the contrary, it moves about continually, scratching and gnawing the walls of its prison. Frequently it hangs by its hind claws, and, in this position, it performs its toilet in the manner of certain Bats. In this operation it uses the third finger of the fore-feet, which it bends in the form of a hook in order to comb the tail and to adroitly wipe its front, the corners of its eyes, the nose, mouth and ears.

In eating, the Aye-Aye exclusively employs the left hand; it thrusts into the semi-liquid food which is given to it, the fourth finger, the longest of all, holding the third raised above the others, and the thumb, on the other hand, very low. The extremity of the anterior limb, thus arranged, describes a singularly rapid motion to and fro, and the lateral face of the fourth finger, passing every moment between the lips of the animal whose head is inclined on one side, places the food in the buccal cavity, over the tongue. At the same time, the cheeks and lips are in continual motion. "The Aye-Aye," says Mr. Bartlett, "can also advance its lips and lick in the manner of cats; but it does this but rarely. I have never heard it utter a single cry, emit any sound, during the long hours of night, and I never have observed that he was made uneasy by my presence. This Lemur seeks no species of insects, but readily feeds on a sort of pap made of milk, honey and eggs; it appears to love semi-fluid substances, soft and mucilaginous, while it rejects with contempt worms, grass-hoppers, and the larva of hymenopters. I have then the right to state that, in a state of nature, the Aye-Aye is not insectivorous. Seeing its strong and sharp teeth, I am inclined to believe that it cuts grooves in the bark of trees, in order to make the sap flow; it receives this in its mouth and it forms its principal nourishment. In support of this opinion, I state the fact that the animal frequently returns to the same spot on the branch or on the trunk, which it first attacked. It must also be stated that the Aye-Aye pays, so to speak, no attention to what it carries to its mouth. Having on several occasions withdrawn the dish which contained its pap, while it was eating, I saw with astonishment that it continued to direct its hand towards the spot where its food had been, and that it did not search for the latter until after having, for a long time, mechani-

THE AYE-AYE, OR CHEIROMYS OF MADAGASCAR.

cally executed prehensile motions. Such a stupid manner of acting is in complete contradiction to that which is observed among animals that devote themselves to the pursuit of other animals and feed on living prey; I presume, therefore, that the Aye-Aye feeds on vegetable substances. I have often seen it, after having swallowed a certain quantity of liquid food, devour a piece of bark."

The Cheiromys, at the Botanical Garden, given by M. Humblot and M. Archambault, act exactly like the one which has been so conscientiously studied by Mr. Bartlett, the Superintendent of the Zoological Garden of London. They sleep during the whole day, which is very annoying to the visitors desirous of seeing these strange animals, and, when the keeper tries to arouse them from their sleep, they show their ill-humor by attempting to bite and by endeavoring to retreat to the most obscure corner of their cage.

In Madagascar, the Aye-Aye inhabits the large forests, and are found not only in the western region, as Sonnerat thought, but also on the southeastern side, where it has been observed by M. Grandidier. According to the natives, it builds a real nest, of a spherical form, in which the female deposits and raises her young. This assertion without doubt merits belief, since in 1877, M. Soumage brought to France one of these nests, which was built on the forked-head of two branches, and which contained a female and her young. The walls of this nest were formed of rolled leaves of the Ravenala or *Tree of the Traveler*, covering an interweaving of twigs; it has on one side a very narrow opening.

The smallest of the other Lemurs—the Chirogales, the Microcebes and the Lepilemurs—have, it appears, similar habitats, and also interweave, with twigs and leaves, a home for their progeny, while the Makis, and all the higher orders of Lemurs, build no nests, and carry their young attached to their back or hung against their breast.—*Translated from La Nature.*

DETECTION OF OLEOMARGARINE.*

By P. Casamajor.

In the *Moniteur Scientifique* for April, 1881, is an article on Butter Analysis, in which are given the processes, used at the Municipal Laboratory, attached to the Prefecture of Police in Paris, for the detection of foreign fats in butter. This is followed by an account of an areometric method, used for the same purpose and based on the difference of density between butter and the fats with low melting point, extracted from tallow, which are made to resemble genuine butter, and which are known under the commercial name of *Oleomargarine.*

The sale of Oleomargarine has become so extensive in this country, that a purchaser of butter is never sure whether he is getting true butter or its imitation. In view of this fact, I have thought it useful to give a process, based on the difference of specific gravity between butter and oleomargarine, of such simplicity that it can be easily applied by any person having rudimentary ideas of manipulation.

Processes of this character are those which can be used with greatest efficiency to check adulterations. I have, in previous communications, given such processes for the detection of Starch Sugar mixed with Cane Sugar, and for the detection of starch sugar syrup, mixed with sugar house syrups.

Although my concern is principally with the difference of density between butter and oleomargarine, I propose to very briefly call attention to the processes used at the Municipal Laboratory of the Prefecture of Police, as these show important differences in chemical composition between true butter and its adulterant, which confirm the difference in the specific gravity. Such an important character as the specific gravity would not differ to any marked extent, without a corresponding diversity in the composition of the two substances.

One process used at the Municipal Laboratory is the following: the sample of butter to be tested is melted, so as to separate water, salt etc., which are deposited, and a certain amount of scum, which comes to the surface. Of the clear melted fat, under the scum, about 3 or 4 grammes are taken and saponified by 1 or 2 grammes of potassic hydrate. The fat and potassa should be mixed with 50 C. C. of alcohol. In about 5 minutes the saponification is complete, and the cautious addition of water should not produce any turbidity. If any takes place, the operation must be begun anew. The soap formed is afterwards decomposed with weak sulphuric acid, and the insoluble fat acids are collected and weighed. The result of a great number of experiments is that in butter the percentage of fat acids thus obtained is usually 86.5 to 87.5 per cent., and that sometimes, it is as high as 88 per cent. In animal fats from tallow the percentage of insoluble fat acids is 95½. The difference $95\frac{1}{2}-87\frac{1}{2}=8$ per-cent., is attributed to the absence in tallow of volatile and soluble fat acids which exist in butter.

Another process is given in which the result is obtained volumetrically, by estimating the quantity of potassa used in saponifying the fat. One gramme of butter requires 225 to 232.4 C. C. of potassa solution, while 1 gramme of tallow, or other animal fat of the same nature, requires from 195 to 197 grammes of the same potassa solution.

Mr. Charles Girard, director of the Municipal Laboratory, considers as adulterated any butter requiring, for saponification, less than 221.5 C.C., of the potassa solution. In some unfavorable cases this volume may represent nearly 30 per cent. of foreign fat.

The method for detecting the difference between butter and oleomargarine by the difference of specific gravity, is one proposed by Messrs. Leune and Harburet.

The butter to be tested is first melted so as to separate the pure fat from water, salt, etc. The clear melted fat is placed in a cylinder, heated by the vapor escaping from a water bath, kept boiling, but no part of the cylinder is to be in the boiling water. I understand that by heating in this way, the temperature of the melted fat remains at about 93° C. To determine the density of this fat an areometer is placed in it. This areometer is graduated in such a way that, in butter, it will sink to the lowest mark of the scale, while oleomargarine corresponds to the highest point in the graduation. The intervening space is divided into ten equal parts, each one of which corresponds to $\frac{1}{10}$ of oleomargarine, mixed with butter. More than 600 experiments made by Messrs Leune and Harburet with artificial mixtures show that, within an approximation of ten per cent., the instrument gives correct results.

Soon after this areometric method was published, it was announced that the difference of the specific gravities of butter and of oleomargarine, was too slight to distinguish the one from the other. As Messrs. Leune and Harburet had not stated what the specific gravity of each was, it was impossible to judge of the truth of this statement, and it became interesting to ascertain the facts of the case. The following process is the result of my attempts to determine the specific gravities of butter and of oleomargarine. I chose in the first place to ascertain the specific gravity of each at 15° C, which is the usual temperature for such determinations. The process consisted in finding for each a liquid in which, at 15° C, a portion of butter or of oleomargarine, freed from impurities by previous melting and containing no air bubbles, would remain in equilibrium in any portion of the liquid, without any tendency to rise to the top or sink to the bottom. The readiest liquid for this purpose was a mixture of alcohol and water, as this is easily prepared and it has no dissolving action on the fats to be tested. As the density of the liquid in which a body remains in equilibrium is the density of the body itself, the problem was narrowed down

* (Read before the American Chemical Society Sept. 1881.)

to finding the difference of density between two mixtures of alcohol and water of different strengths. It was found that pure butter, at 15° C, would remain in equilibrium in alcohol of 53.7 per cent. This corresponds to specific gravity 0.926. This butter was obtained from a gentleman, at whose country place the butter was made. I obtained oleomargarine from melted warm beef suet by pressure. At a temperature of 25° C, this expressed fat had the consistency of butter. The alcohol which at 15° C, would hold it in equilibrium had a strength of 59.2 per cent., which corresponds to a specific gravity of 0.915.

The question of the possibility of distinguishing butter from oleomargarine becomes equivalent to the possibility of distinguishing alcohol of 53.7 per cent., from alcohol of 59. 2 per cent. As this difference is 5.5 degrees of Gay Lussac's alcohometer, it is very evident that the specific gravity is a sufficient character for distinguishing butter from oleomargarine. This difference may appear more clearly to persons not familiar with alcohometry by stating that it is the difference between 0.926 specific gravity and 0.915.

By means of the tables of Gay Lussac and of Tralles,* it is a very easy matter to prepare alcohol of the required strength at any temperature, to be kept in bottles for future use.

As the expansion of fats is different from that of alcohol, it is advisable to bring the alcohol to 15° C, when making an observation, which can be easily done by any one provided with a thermometer.

To deliver the sample of fat on the alcohol, I have found that the best plan is to melt the fat and let a large drop of it fall into the liquid. The fat should be melted in a little spoon or a little scoop, and the drop should be delivered by bringing the spoon or scoop close to the surface of the alcohol. It requires a little practice to do this neatly, so as not to get an air bubble in the ball of melted fat. When an air bubble becomes imprisoned in the fat, I have had no difficulty in removing it with a strip of paper, while it lies on top of the alcohol. Sometimes the globule of fat only partially sinks in the alcohol; the top of it becomes flat and remains exposed above the liquid. A slight tap on the side of the glass is then generally sufficient to form a wave and sink the globule.

If we take alcohol of 56½ per cent., which represents equal volumes of alcohol of 53.7 per cent., and of 59.2 per cent., and if we deliver on the surface of this alcohol a globule of melted butter and one of oleomargarine, the butter will sink to the bottom and the oleomargarine will remain at the top, while the two globules are still warm and liquid. Afterwards, if the alcohol has a temperature of about 30° C, the butter will become solid, while the oleomargarine may still remain liquid. Then the butter will rise to the top of the alcohol, which is due to the expansion of butter on solidifying. If the alcohol be then kept for a few minutes, at 15° C, the oleomargarine will become opaque and remain at the top while the solid globule of butter will sink to the bottom.

If instead of taking alcohol of 56 per cent. we use alcohol of 59.2 per cent., oleomargarine will remain on top and butter will sink to the bottom at all temperatures above 15° C. At 15° C., oleomargarine will remain in eqilibrium in any portion of the liquid in which it may be placed.

If oleomargarine was always sold pure, the foregoing indications would be sufficient to distinguish it from butter, but the oleomargarine found in the market is always more or less mixed with true butter to improve its taste and appearance. This being the case, alcohol of 59 per cent. is not the proper liquid to detect oleomargarine. We should use alcohol of 55 per cent. and consider as oleomargarine any so called butter which will not sink to the bottom in alcohol of this strength at 15° C. This is founded on the fact that not more than ⅓ of butter is ever mixed with oleomargarine to improve its taste and appearance.

*See the excellent tables of Prof. Mc. Culloh.

Bearing in mind the experiments of Messrs. Leune and Harburet, already cited, the proportion of butter and of oleomargarine in a mixture could be easily detected by finding what strength of alcohol will hold in equilibrium at 15° C, a globule of fat under examination. As the difference of 59.2 and 53.7 is 5. 5, the proportion of oleomargarine is the difference between the strength of the alcohol and 53.7, divided by 5.5, or more conveniently multiplied by 0.18. If the alcohol required to hold a globule of fat in equilibrium at 15° C, has a strength of 57 per cent., then: $(57-53.7)\times 0.18=3.3\times 0.18=5.95$, or say $\frac{6}{10}$ of oleomargarine. If the alcohol had a strength of 58, then $58-53.7\times 0.18=4.3\times 0.18=7.72$, or about $\frac{8}{10}$ of oleomargarine.

The proportions of butter and oleomargarine in a mixture may be also determined without the aid of an alcohometer, by using the two solutions of 53.7 per cent. and of 59.2 per cent. These may be placed in graduated glasses and poured cautiously into a third glass, until an alcohol of sufficient strength is obtained to keep in equilibrium a globule of the fat under examination at 15° C.

The relative volumes of the two solutions used in making the mixture, give the proportions of butter and oleomargarine.

The accuracy of these calculations rests entirely on the results obtained by Messrs. Leune and Harburet. I have not verified them by experiment, and I do not clearly see their utility. When we buy butter it is interesting to know whether what we buy is pure butter or not. It is no palliation to the offence of selling oleomargarine for butter that the oleomargarine contains ¼ or ⅕ of real butter.

FILARIA OF THE HUMAN BLOOD.

The members of the Pathological Society, of London, recently enjoyed the rare opportunity (in this country) of seeing the filaria sanguinis hominis in the living state from a patient in the London Hospital suffering from hæmato-chyluria, under the care of Dr. Stephen Mackenzie. They were also enabled to hear from Drs. Cobbold and Vandyke Carter the facts at present known concerning filarious disease, whilst the observations related by Dr. Mackenzie, most patiently and carefully pursued for two months upon the case in question, were a valuable addition to these facts. In one important point these observations have resulted in a further discovery, to which we shall refer again. Our present purpose will be simply to gather up briefly the facts as detailed by these speakers, and to indicate their bearings upon the pathology of the obscure affections of the lymphatic system with which they are connected. In the first place we have now—thanks to the discoveries of Bancroft, Lewis, and Manson—a complete knowledge of the life history of the parasite. Like so many similar creatures, it presents us with an example of alternation of generations; or more correctly speaking, of the need of two hosts for its full development. The minute almost structureless worms found in the blood of the human subject in such vast numbers are the *embryonic* forms of the filaria which requires the mosquito in which to develop into the sexually mature worm. The mosquito feeding on the blood at night, when the filariæ are generally alone to be found, becomes gorged with them. Their growth in the mosquito has been traced by Lewis and Manson, and it is presumed that they are only liberated from the body of their host by its death in the water to which it always finally resorts. The nematoid is thus set free, and possibly undergoes further development; for the mature worm measures some three inches in length. Its passage into the human body is easily explained; and the analogy in this respect with the larger nematoid—the guinea-worm—is one which Dr. Vandyke Carter ably illustrated. Once within the human body, the worm lodges in the tissues, but as to its migrations, and, indeed, its ultimate resting-place, but little is known. It

would seem, from its discovery in a lymphatic abscess by Bancroft, and in a lymph scrotum by Lewis, to have a peculiar aptitude for selecting the lymph channels for its habitat; a selective power not more remarkable than that which urges the trichina to lodge in muscular tissue. This is further borne out by the fact that its embryos—the filaria sanguinis hominis—are met with in the blood and the urine of the subjects of chyluria and nævoid (or lymphatic) elephantiasis.

Now, although the various discoveries which have been made—at the expense of so much patient research and at such various times that, as Dr. Cobbold remarked at the meeting, they form each distinct "epochs"—have enabled us to form the above complete sketch of the life-history of the parasite, there are lacunæ still to be filled up. Thus knowledge is wanted upon the growth and migration of the parent worm after it has gained entrance into the human body, also as to its duration of life, and particularly as to the question whether it can take on the power of a sexual reproduction, and if so, for how long a time. The myriads of filariæ that are probably daily reproduced in the body of such a patient as that under Dr. Mackenzie's care seem to demand such a fact as alternate generations, and also to raise the question as to the time during which the process of reproduction can continue. There is no reason to believe that the embryonic filariæ in the blood can undergo further development within the human body; indeed, analogy, as well as the remarkable discovery of an intermediate host in the mosquito, are opposed to this notion. Again, filariæ have been found in the blood apart from chyluria or any outward manifestation of lymphatic derangement; but this is explicable if it be admitted that the adult worms may lodge in other parts of the body in communication with blood vessels alone. Conversely, chyluria may exist without filaria, and the case mentioned by Dr. Mackenzie, where the parasite was found in the man's blood in India, but could not be found when he came to England, is explicable on the view that though the parent organism might have perished, or yielded no more embryos, yet the change excited by its presence in the lymphatic channels, and therefore the chyluria, might still have persisted.

The precise mechanism of chyluria still requires to be explained, and until it is elucidated an important part of the subject will remain obscure. The question of the pathology of chyluria was, however, barely touched upon on Tuesday, Dr. Mackenzie limiting himself to the statement of the facts observed in his case; the most important in connection with the urine being that besides having all the chylous characters, it invariably contained more or less blood,—that passed by day containing more blood and filaria, that passed by night being more milky; and that filariæ were found in it, especially in connection with blood coagula. The most remarkable feature of the whole case lay in the periodicity shown by the filariæ in their time of appearance in the blood. During the whole period of the man's stay in the hospital his blood had been examined regularly every three hours, with the constant result that, by night, the filariæ abounded and by day were entirely absent. From 9 A. M. to 9 P.M. they were absent; they appeared at the latter hour and increased up to midnight, then decreased till at the first-named hour none were found. These observations entirely confirmed those of Manson, and particular stress was laid upon their nocturnal wanderings and the habits of the mosquito. It is certainly singular that the time selected by the mosquito should coincide with the presence of the parasite in the blood stream, and the connection of these two facts is not the least wonderful chapter in the life-history of the parasite. But whatever the explanation of the periodicity—Dr. Vandyke Carter pointed out that it was not invariable,—a valuable addition to our knowledge of it has been made by Dr. Mackenzie. He found that whereas the time of ingestion of food bears no relation to it, it is otherwise with the time of rest and sleep, for when the patient was up during the night and slept during the day the period of filarial migration was similarly inverted. Dr. Mackenzie did not venture to speculate upon these curious points. He wisely contented himself with laying the facts he had observed before the Pathological Society.—*Lancet.*

A VERY REMARKABLE METEOR.

On the evening of Wednesday the 16th, while sweeping the western heavens in search of comets, I was startled by a brilliant illumination to my right; looking up hastily, a bright meteor was seen moving rapidly along the north-eastern heavens. It started from a point about 3° north of Capella, and, traversing a path of about 10°, passed some 2° above *delta Aurigæ.* The flight of the meteor did not exceed three seconds, when it burst with a dazzling brilliancy, to be compared only to the whiteness of the electric light. At the moment of exploding it must have been at least five or six times brighter than Venus at her maximum. There remained in its wake—covering the full extent of its path—a thin reddish train; this drifted slowly among the stars towards the north-east, gradually collecting into a lightish cloud at its N.E. end. Noting the remarkable permanency of the train, I turned the telescope (a 5-inch refractor) upon it, and was surprised to see a very brightly glowing mass of pinkish smoke; the same material lay stretching out toward the southwest in a long, straggling strip; this trail was about one-fourth a degree in thickness, and could plainly be seen with the telescope for a distance of at least ten degrees. This mass of smoke drifted northeasterly over the stars, curling slowly, like a mighty serpent. It was knotted in several places with cumulous forms which were due probably to minor explosions in the meteor. The outlines of this wonderful train of celestial smoke were well defined; it did not diffuse itself in the atmosphere, but faded gradually from view. During the whole of its visibility it retained its pinkish color. The appearance of the meteor was at 48m. past 6. The train remained visible to the naked eye for about six minutes. In the telescope it was very distinct up to seven o'clock. At three minutes after seven it was still visible in the instrument. Meanwhile it had drifted about 4° to the north-east, becoming more crooked each moment as it curled about in the air. The remarkable duration of the train of smoke from this meteor—over fifteen minutes—deserves being recorded. E. E. BARNARD.

NASHVILLE, Tenn., *November* 21, 1881.

INSTRUCTIONS ISSUED BY THE INTERNATIONAL CONFERENCE FOR THE OBSERVATION OF THE TRANSIT OF VENUS OF 1882.

Contributed by M. BENJAMIN, Ph. D.

ARTICLE 1.—It is desirable, from a theoretical standpoint, that the telescopes used should be of as large aperture as possible. In practice, the difficulty of transportation on the one hand, and the necessity of observers at different stations having similar instruments, limits the apertures to from 0.12 metre to 0.15 metre (about 4½ to 6 inches).

In all cases the objectives should be as perfect as possible. Observers should give an exact description of the quality and defects of the objective, as also the eye-piece employed. Towards this end they should determine:

1. The form of the image of a good star in focus, as also the image of the same star at a point before and after coming into focus.

2. The separating power of the objective for double stars.

It will be useful to know also if the telescope is able to show the solar granulations on any favorable opportu-

nity, and also the degree of visability of these granulations during the transit.

ARTICLE 2.—It will be well to employ a reflecting prism, or a polariscopic eye piece, to diminish the heat and consequent danger to the observer's eyes.

If it be decided to use a silvered objective, a method which offers the great advantage of eliminating all the obscure heat rays and doing away with errors from distortion arising from heating of the interior of the tube, the excess of light may be absorbed by a neutral tint glass composed of two glasses of similar thickness, one being colored and the other colorless.

ARTICLE 3.—The eye-pieces should be positive, achromatic, and of a power of 150. The observations of contacts should be made in a field sufficiently clear to show plainly projected on the solar disc, two wires separated by a distance of 1″.

Means should be employed to remove as far as possible the effects of atmospheric dispersion.

The setting point of the reticule should be previously ascertained on the stars or by means of a collimator focussed to stars.

In cases of observation by projection, correspondent means should be employed.

ARTICLE 4.—The times corresponding to internal contacts may be defined as follows:

Ingress. The moment when an evident and, at the same time, persistant discontinuity in the illumination of the apparent limb of the sun joining the point of contact with Venus, disappears.

Egress. The moment of the first appearance of an evident and, at the same time, persistent discontinuity in the illumination of the solar limb joining the point of contact.

If the limb of two stars coming into geometrical contact, without obscuration or deformation of the interposed thread of light, the instant previously defined is that of contact.

If there be produced a black drop or ligament, well defined and as dark as the body of the planet, the precedingly defined instants, are for Ingress, that of definite rupture, and for Egress, that of the first apparition of the ligament.

Between these two extreme cases, other appearances may be produced when the instants of contact may be noted as follows:

If the limbs remaining without deformation, there is produced an obscuration of the luminous thread, without the shadow, however, being as dark as the body of the planet, the observer notes the instant of geometrical contact. The moment of the formation or disappearance of this shadow should also be noted.

If the shadow is almost or becomes quite as dark as the planet, the precedingly defined instant is that when this equality ceases or is established.

The observer should also note whether there is produced on the luminous thread, any fringes or any other distinct phenomena, and should note the moment of their appearance and disappearance.

It is generally desirable to note the time of occurrence of any distinct phenomena about the time of contact. Nevertheless it is a grave mistake, and one that should be guarded against, to multiply the noting of times near the occurrence of a contact.

The time of appearance of phenomena of a distinct character, should be mentioned only in such a manner as to be readily separated from other phenomena observed about contact.

It will be useful in all cases that the observer should illustrate his notes with a drawing made immediately after each complete observation of contact, in order to show more clearly the interpretation which he attaches to his description of the phenomena.

ARTICLE 5.—As the limb of Venus falls internally on the solar disc during internal contact, as has been noted in Article 4, the observer should indicate as closely as possible whether the moment when the limbs of Venus and the sun, apparently coinciding, seem to be lengthened out.

This observation, though rough, is still desirable as a check to the principal noted phase.

ARTICLE 6.—Notwithstanding the fact that observations of external contacts are subject to considerable uncertainty, the conference recommends that they be observed either by direct vision or by means of the spectroscope, and that the point on the solar disc, where the first contact takes place, be determined in an appropriate manner.

COMPTES RENDUS, Oct. 17, 1881, t xciii., p. 569.

BACTERIAN NOTES.

So long as the makers of microscopes do not place at our disposal much higher powers, and as far as possible without immersion, we shall find ourselves in the domain of the Bacteria, in the situation of a traveler who wanders in an unknown country at the hour of twilight, at the moment when the light of day no longer suffices to enable him clearly to distinguish objects, and when he is conscious that, notwithstanding all his precautions, he is liable to lose his way.—Cohn.

Bacteria were regarded as animals up to the time of Dujardin (1841), a kingdom—the *Protista*—midway between the animal and vegetable, being created by Haeckel for their especial benefit. Duvaine (1859) was, however, among the first to show clearly their alliance with the algæ. Cohn holds them to belong to the algæ, although from want of Chlorophyle, approaching the fungi. Magnin says, "If there are still some differences of opinion among naturalists as to the place of the Bacteria among the cryptograms, there is but one opinion of their vegetable nature." Sachs, however, solves the difficulty by uniting the algæ and fungi in a single group, the *thallophytes*, in which he establishes two series exactly parallel —one comprising the forms with chlorophyle, the other, the forms which are deprived of it."

"The Bacteria, then, resemble green plants, in that they assimilate nitrogen contained in their cells by taking it from ammonia compounds, which animals cannot do, They differ from green plants in that they cannot draw their carbon from carbonic acid, and only assimilate organic substances containing carbon, above all the hydrates of carbon and their derivatives; and in this respect they resemble animals."

Ehrenberg was the first to maintain that the motion of Bacteria depended npon the presence of vibratile cilia (observed by him in *spirillum volutans*), but although the cilia, denied at first by most microscopists, have been since seen in nearly all the bacteria, recent researches permit us to say that cilia exist without doubt in all true bacteria; the botanist who has best studied them, M. Walming for example, recognize that it is scarcely probable that these organs are the cause of their movement, for one meets some examples in which the body remains motionless, while the cilia are in violent agitation, and others in which the body moves, while the cilia remain inert or dragging behind.

Cohn explains the origin of the gelatinous substance in which the bacteria are included as being produced by a thickening or jellification of the cell membrane, but a more plausible view is that it is produced by a secretion from their protoplasm.

MR. A. AGASSIZ has printed in the *Proceedings of the American Academy* a biographical sketch of the late Count Pourtalès, together with a biographical list of his principal publications. Mr. Agassiz has also written a Review of Professor Haeckel's Monograph of the Acalephs in the August number of the *American Journal of Science.*

ATOMIC PHYLLOTAXY.

Gerber (*Chemical News,* XLIII, 242–43), says that "no simple relation exists among" his divisors, therefore they "have no value in themselves." There is, however, a relation which he failed to discover, for they are phyllotastic, as will be seen by the following comparison:

Gerber's Divisors.		Phyllotastic Divisors.	
H	.9997	$H = 0 \div 2^4$	.998
D_1	.769	$\frac{5}{13} \times 2\,H$	.768
D_2	1.995	$2\,H$	1.996
D_3	1.559	$\frac{5}{2} \times \frac{5}{8}\,H$	1.559
D_4	1.245	$\frac{5}{2} \times \frac{1}{2}\,H$	1.247

PLINY EARLE CHASE.

HAVERFORD COLLEGE, *Nov.* 10, 1881.

ON September 13, 1881, a red star was noticed at the Harvard Observatory in R. A. 16th, 31. 5 m, Dec. + 72° 32′. From the similarity of its spectrum to that of several known variable stars, it was presumed to be variable; and the suspicion was confirmed both by its absence from the catalogues, and by subsequent observation, which showed that its brightness was increasing. Information respecting it was sent by telegraph to Dr. Copeland, at Strassburg, by means of the telegraphic cipher devised by Messrs. Chandler and Ritchie.

THE Museum of Comparative Zoölogy, of Harvard College, has received a collection of ninety species of fossil plants from Cannelton, Pa. The species have been identified by Mr. Lesquereux, who pronounces the series one of the best made by Mr. Mansfield from that locality.

THE annual report to the government of India on the progress of the cinchona cultivation and its practical results is a document not only of great importance, but also of considerable interest. The success which was chronicled in previous reports has been well maintained under the superintendence of Dr. King, who has the responsible charge of the cinchona cultivation in Bengal. It will be remembered that the object to which efforts hitherto have been chiefly directed is the manufacture of a cheap febrifuge from the bark of the cinchona succirubra. The plantations of this tree which are now in existence are so extensive as to suffice for present and probable requirements, so far as the febrifuge is concerned. They contain more than four millions of trees, and from them 267,335 lbs. of red bark were obtained during the year. The yield per acre (1510 lbs.) is not, however, considered to be very good; 9296 lbs. of the febrifuge were made during the year, and 8653 lbs. were consumed, 5500 lbs. being used in the Government services and 3150 lbs. sold to the public. The demand for the febrifuge steadily increases, a satisfactory proof of its value, and the total amount manufactured from the commencement to March 31, 1881, is 36,639 lbs.

Financially the enterprise, initiated with such pains by Mr. Markham, must be regarded as a complete success. The actual profit on the year's working was eight per cent. on the capital of the plantation. This does not, however, represent the whole gain of the year. The price of quinine was very high, and the cost of the 5550 lbs., which would have been used by the Government had the febrifuge not been available, would not have been less than £48,000, while the cost of the febrifuge was only a sixth of the amount, representing a saving of at least £40,000.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING NOV. 26, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

NOVEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Maximum. Time.	Maximum. Wet Bulb.	Maximum. Time.	Minimum. Dry Bulb.	Minimum. Time.	Minimum. Wet Bulb.	Minimum. Time.	Maxi'm In Sun
Sunday, 20	30.269	30.386	9 a. m.	29.798	0 a. m.	37.0	36.0	45	0 a. m.	43	0 a. m.	35	12 p. m.	35	12 p. m.	100.
Monday, 21	30.137	30.378	0 a. m.	29.988	12 p. m.	40.0	39.0	43	3 p. m.	42	3 p. m.	35	0 a. m.	35	0 a. m.	84.
Tuesday, 22	30.185	30.296	8 p. m.	29.982	2 a. m.	34.3	33.3	41	0 a. m.	40	0 a. m.	26	12 p. m.	26	12 p. m.	90.
Wednesday, 23	29.941	30.280	0 a. m.	29.548	12 p. m.	30.6	30.6	35	9 p. m.	35	9 p. m.	25	3 a. m.	25	3 a. m.	45.
Thursday, 24	29.573	29.822	12 p. m.	29.496	4 a. m.	32.0	32.0	38	12 m.	37	12 m.	25	12 p. m.	25	12 p. m.	80.
Friday, 25	30.145	30.200	9 p. m.	29.822	0 a. m.	27.3	26.7	30	3 p. m.	30	3 p. m.	23	7 a. m.	23	7 a. m.	92.
Saturday, 26	30.036	30.196	0 a. m.	30.000	2 p. m.	38.3	33.7	43	3 p. m.	37	4 p. m.	29	0 a. m.	28	0 a. m.	97.

Mean for the week........ 30.041 inches.
Maximum for the week at 9 a. m., Nov. 20th........ 30.386 "
Minimum " at 4 a. m., Nov. 24th........ 29.496 "
Range........ .890 "

	Dry.		Wet.
Mean for the week	34.2 degrees		33.0 degrees.
Maximum for the week, at 0 am. 20th	45. "	at 0 am 20th,	43. "
Minimum " " 7 am. 25th	23. "	at 7 am 25th,	23. "
Range " "	22. "		20. "

NOVEMBER.	WIND. Direction. 7 a. m.	Direction. 2 p. m.	Direction. 9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Force. Time.	HYGROMETER. Force of vapor. 7 a. m.	Force of vapor. 2 p. m.	Force of vapor. 9 p. m.	Relative humidity. 7 a. m.	Relative humidity. 2 p. m.	Relative humidity. 9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND *SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0 10
Sunday, 20	n. w.	n. w.	n. e.	309	20½	1.00 am	.191	.203	.204	90	82	100	6 cir. cu.	6 cir. cu.	0	-----	----	-----	--	0
Monday, 21	w.	w. s. w.	s. w.	135	2¼	1.15 pm	.212	.231	.235	100	83	91	7 cir. cu.	8 cu.	10	-----	-----	-----	--	10
Tuesday, 22	n. w.	n. n. w.	e.	244	6¾	11 15 am	.216	.170	.153	90	80	100	3 cir.	3 cir. s.	0	0 am	1 am	1.00	.01	5
Wednesday, 23	n. e.	n. n. e.	n. e.	183	3	9.00 pm	.141	.174	.204	100	100	100	8 cu.	10	10	12 m	12 pm	12.00	.59	10
Thursday, 24	n. n. w.	n. n. w.	w. n. w.	275	13½	3.10 pm	.188	.204	.153	100	100	100	10	8 cu.	0	0 am	5 am	5.00	.07	10
Friday, 25	w. n. w.	w.	w. s. w.	317	6	0.00 am	.123	.130	.160	100	78	100	0	1 cir. cu.	0	-----	-----	-----	--	7
Saturday, 26	s. w.	w. s. w.	w. s. w.	317	7½	9.15 pm	.144	.113	.147	79	42	56	2 cir.	4 cir. cu. s	0	-----	-----	-----	--	0

Distance traveled during the week........ 1,780 miles.
Maximum force........ 20½ lbs.

Total amount of water for the week........ 0.67 inch.
Duration of rain........ 18 hours, 00 minutes.

* Wednesday, 23d, slight.

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SATURDAY, DECEMBER 10, 1881.

Sir—On the evening of November 24, I noticed that the spectrum of the star DM. $+36°$ 3987 has a bright band in the blue. The star, accordingly, belongs to the small class of objects which comprises Rayet's stars in Cygnus (near this one) and Oeltzen 17681, discovered here in 1880.

On November 25 I found a small planetary nebula, undistinguishable from a very faint star by the ordinary eye-piece, bnt detected by the character of its spectrum. Its place for 1880 is in R.A. $20^h\ 6^m\ 26^s.4$, declination $+37°\ 3'\ 25''$. It follows W. XX 200 eight seconds, three minutes of arc farther south, and is followed respectively $2^s.6$ and $2^s.3$ by two faint stars north $37''$ and south $20''$ of the nebula.

Harvard College Observatory,
Cambridge, *December* 1, 1881.

Edward C. Pickering.

SHALER AND DAVIS' "GLACIERS."[1]

By W. J. McGee.

I. Introduction.—The extensive superficial modification of the globe accomplished through the agency of water in its three states of aggregation has been rendered possible by certain properties peculiar to this substance, chiefly (1) its powers of assuming the several forms of solid, liquid, and vapor within the narrow range of terrestrial temperature, (2) its enormous capacity for heat, and (3) its power of dissolving other substances.

The temperature of the earth's surface is indeed largely determined by the aqueous vapor contained in the atmosphere; for if it were not for this vapor the solar energy falling upon the earth would be radiated away almost as quickly as received, and could exercise but little influence upon temperature. The narrow range of terrestrial temperature since the beginning of the organic record attests the enormous capacity and marvelous delicacy of this temperature—equalizing agent, for within the limited bounds of the space separating earth and sun, the temperature varies from a hundred thousand degrees above to two hundred and fifty degrees below the Fahrenheit zero; though accidents in this adjustment are attested by the traces of successive ice periods in the geological history of the globe. The influence of liquid water in producing the various phases assumed by the earth's surface, during geological time has long been the subject of study; but it is only within the last forty years that the newly commensurate influence of ice has been detected.

II. *The existing glaciers of the earth.*—The most accessible of the existing glaciers are those of the Swiss Alps; and the best route for the student to pursue in entering this region is to pass up the valley of the Rhone.

Here, aside from the more obscure evidence of the former great extension of the glaciers, the various works of ice-action became constantly fresher in ascending the river until they disappear beneath the wall of ice constituting the terminal portion of the glacier. At the foot of this ice wall is an irregular mass of stones and earth—the *terminal moraine*—lying across the valley, cut in twain by the muddy stream emerging from a cavern in the basal portion of the glacier; and the ice itself is gullied by tiny rills and soiled with sand and dirt, and hardened with pebbles and rock fragments, which from time to time roll down its steep front, to the morainal heap below. When the glacier shrinks for several successive seasons, as occurs when the weather is unusually dry and warm, the stream flowing from it becomes a torrent, and the moraine may be separated from the ice front by a belt of striated and polished rock, but sparsely covered with coarse *debris*; but when the ice advances for a number of years the stream dwindles, and the sheet of earth and stones is pushed forward and crumpled up into a mighty embankment, rising into a range of irregular hillocks. Many such ridges attest the various periods of temporary advance in the history of most of the secularly retreating glaciers. On ascending the ice stream itself, the superficial rock-fragments, pebbles, and earth are found to lie mainly in parallel bands, or *medial moraines*; and on tracing these to their origin, each is seen to consist of the two lines of matter constantly tumbling down the valley sides or *lateral moraines* which are brought into contact whenever two glaciers meet and merge into one. Thus the number of branches uniting to form any glacier can be determined from the number of parallel bands on its surface. The ice-stream occupies a crooked and irregular valley, the rate of its motion varying with the declivity, regularity, and width of the channel, just as does that of liquid rivers; though wherever there are considerable irregularities in the channel the strain produces cracks and fissures which gradually widen and form *crevasses*, or even, where there is a sudden increase in declivity, separates the ice into a mass of irregular pyramidal blocks, or *seracs*; but when a more uniform stretch of gentle slope is reached the seracs re-unite, and the crevasses close, transforming the fragmentary mass again into a solid,

[1] "Illustrations of the earth's surface. Glaciers; by Nathaniel Southgate Shaler, professor of Paliontology, and William Morris Davis, instructor in Geology, in Harvard University, Boston. James R. Osgood & Co., 1881." Very large 4°, pp. i—vi and 1-198, pl. i—xxv and one unnumbered, with twenty-five unnumbered leaves discriptive of plates.

homogeneous whole. The channel is rarely so regular as to allow the ice to be altogether free from crevasses, however. Such fissures are invariably at right angles to the line of greatest tension, and greatly facilitate melting by increasing the exposed surface; and when the rills formed by superficial melting flow into them they may be converted into cylindrical shafts, or *moulins*, extending to the base of the ice. Thus both crevasses and moulins remain practically stationary, or rather, when either has passed beyond the obstruction or irregularity of the channel which produced it, it gradually closes, and another forms in the same place, with respect to the valley and not to the moving ice, as that which it originally occupied. Aside from the longitudinal medial moraines the surface of the ice is often indistinctly marked by depressed transverse bands within which wind-blown sand or dust accumulates (known as *dirt-bands*); which bands curve downward medially more and more toward the debouchure of the glacier, and thus attest the differential motion of the various parts of the ice-stream. There are, moreover, occasional scattered blocks of stone and small pebbles lying upon the surface of the ice. The larger blocks prevent superficial melting of the ice on which they rest, and hence become apparently lifted on columns of ice, forming *glacier-tables*, which sometimes reach a height of some feet; while the smaller pebbles, on the other hand, facilitate melting, and thus gradually sink into miniature wells perhaps several inches in depth.

Glaciers of the alpine type are supplied by the perpetual snows accumulating in the elevated valleys and plains intervening between the highest peaks. Over these snowfields—the *névé* or *firn*—the snow is generally granular and contains much air, especially near the surface; though where it is thick its basal portions may approximate true ice in structure. It is only when the névé passes over the considerable declivity generally separating the snow-field from the ice-stream proper, and descends below the snow-line, however, that it becomes compacted, deprived of its air, and diminished in volume, so as to constitute a veritable glacier. A glacial area may accordingly be divided into two distinct regions on this basis alone;—the névé, the locality of no melting but of constant addition; and the glacier proper, the locality of constant decrease. In polar regions the glacial phenomena are more varied. Thus, in Greenland, the transition may be observed from glaciers of the characteristic alpine type to those of the characteristic polar type, in which the snow-line is at the sea level and the ice is essentially identical with the Swiss névé, though of vastly greater thickness. It is only slow-moving glaciers of the polar type that give origin to ice-bergs—the terminal portion extending into the sea "until the buoyancy of the ice causes a mass to break away from its attachments, rise to the surface, and float away, (p. 28,) scattering the debris frozen to its base over the sea-bottom as it gradually melts; for the bergs, as the névé of which they are formed, are generally destitute of superficial accumulations of earth and stones.

Since in the circumpolar regions the snow-line descends to the sea-level, the ice of winter may not be melted during the succeeding summer, but may remain *in situ* for years or ages, as in the paleocrystic sea seen by Nares. Now such a sea might be itself overspread with snow to such a depth as to depress the ice to the sea-bottom and to convert the whole mass into névé similar to that of northern Greenland, or into a true continental glacier. Indeed,—"it seems probable that the so-called antoretic continent is nothing but an immense sheet of ice such as this paleocrystic sea would become if it were to increase in depth until it fastened on the bottom of the sea." (p. 31.)

III. *Distribution of the existing glacier.*—In the Scandinavian mountains there are the large snow-field in the gostedal highland with many scattered glaciers of considerable interest, and, in lat. 70°, a vast snow-field with an immense ice-stream descending to the sea level; while on the opposite side of Russia the Ural range is without glaciers. In the Pyrenees the glaciers are much shrunken, and mainly confined to moist northern slopes, though about one hundred in number. In the Alps there are over a thousand glaciers, occupying, with the névé, about one-seventh of the mountainous alpine region. Eastward there are no glaciers until the Caucasus is reached, where a considerable snowy range, with ice-streams on both slopes, is found. A few scattered glaciers are known in Asia Minor, one in Persia (on the volcano Demarend,) and many on Hindu Kush; though these have been but imperfectly described. In the Himalayas the glaciers are of remarkable size and extent, though as yet but partially known. In the Southern Alps of New Zealand the glaciers are also of considerable extent and of great interest. On the western hemisphere glaciers occur along the western border of South America as far north as Upper Chili, where they mainly disappear, and are but meagerly represented along the Andes and Cordilleras until Oregon and Washington Territory are reached. Those occurring within the United States are of little prominence, however; but they increase in size and number northward, until at Mount St. Elias the ice reaches the sea level. In both Arctic and Antarctic regions there are also immense bodies of moving ice or névé, constituting glaciers of the polar type.

It thus appears that glaciers are mainly confined (*a*) to regions of great cold and considerable precipitation, (*b*) to mountain ranges along western coasts outside of the trade-wind zones in regions of heavy and frequent precipitation, and (*c*) to interior ranges of great height and considerable snow-fall; while (*a*) broad arid areas—even though "the ground is frozen to the depth of several hundred feet" (p. 36),—(*b*) interior ranges of limited snow-fall, and (*c*) regions having a hot and dry summer, are generally free from glaciers. The essential conditions for glaciation are hence, 1st., cold of considerable intensity; 2nd., considerable snow-fall; and 3rd., the absence of a dry season of sufficient length to melt the winters' snow.

IV. *Distribution of ancient glaciers.*—"The most remarkable fact that has been discovered by geologists during this century is, that at various times in the earth's history the glaciers, which now cover but a very small space on the earth's surface, certainly not over about one hundreth of its area of land, have been extended until they occupied a very large part of land and sea" (p. 38)." The glacial records are, however, so ephemeral that none save the last ice-period can ever be well known to us. During this period the accumulation of ice was most extensive in regions where glaciers yet prevail, or where the various meteorological conditions at least approach those which existing ice-fields indicate to be essential for glaciation, as in the Alps, the Pyrenees, and Scandinavia, over northern Europe, and in the Himalayas and New Zealand, on the eastern hemisphere; and over much of the northern portion of North America and a lesser area in the southern extremity of South America as well as isolated localities along the Andes and Cordilleras, in the western hemisphere. Over the plains of Switzerland an ice-sheet more than 4,000 feet thick swept its debris to the flanks of the Jura, a hundred miles away; but on the northern slope of the Alps the extension was less. Here the direction of motion was everywhere determined or at least modified by local topographical features. In the Pyrenees, the Appenines, the volcanic mountains of central France, and the Jura, in the Vosges, and in Corsica, the accumulation of ice was little more than the development of an extensive system of local glaciers; and north of the Alps there is little evidence of glaciation within inland Europe. The most complete testimony concerning European glaciation in the Quaternary, is furnished by Scandinavia and Great Britain. "Stretching from Scandinavia across the North

Sea, which it must have nearly closed, the North Europe glaciers extended over Scotland, all the north of England, and probably all of Ireland. On the north its limits were perhaps the polar ice itself, and in the west the deeper waters of the Atlantic. The southern limit of this ice-sheet was in the south-central part of England" (p. 40). This was probably "the southern edge of the polar ice tops [ice cap?] rather than a local system of glacial sheets" (p. 40). In North America the accumulation of ice was still more extensive, and of somewhat different character; "here the ice lay as a continuous mass, stretching down from the polar regions to the central parts of the continent, overlapping the shores for a great distance to the south along the Atlantic and Pacific coasts, and giving a continuous though irregular ice front across the land from sea to sea" (p. 41). The terminus of this sheet is yet marked by moraines as constituting the Banks of Newfoundland, George's Banks, Cape Cod, Martha's Vineyard, and Block and Long Islands, and extending thence across central New Jersey, and south as far as Washington. The attenuated margin left less distinct traces of its existence in the hills of southern Virginia, and thence into the higher Appalachians in North Carolina, whence it returned hugging the western mountain slope, and extending through West Virginia, crossing the Ohio river near the mouth of the Kanawha. Thence the southern edge of the ice skirted the north shore of the Ohio to Cincinnati, near which place it sent a lobe across the river a few miles into Kentucky. "West of Cincinnati the front of the ice sheet inclined rapidly to the north-west, and becomes hard to trace. It probably passed somewhat south of Chicago, through Iowa, and thence through Minnesota, following near the line of the Missouri to the Rocky Mountains" (p, 42). In the Cordilleras the ice was mainly confined to the higher mountains, and probably partook of the character of local or alpine glaciers within the limits of the United States; while north of our domain "we know little of its distribution" (p. 42). "There can be little doubt that the ice sheet was continuous from its southern face to the poles during the depths of the last ice time. * * * This glaciated region of North America includes more than half the continent; in fact over two thirds of its surface felt the weight of the ice during the last geological period, and works its work in the existing geography" (p. 44). The thickness of the ice is not definitely known except in the vicinity of Mt. Washington, where it exceeded a mile. In South America it is probable that continental ice never extended north of the Rio de la Plata over the plains, nor beyond the Chilian coast on the Andes.

V. *The work of the glacial time.*—Water, whether liquid or solid, is a most efficient agent of erosion; but the mode of action of the two forms is quite different. Liquid water itself operates in a two-fold manner: 1st, as a chemical agent, penetrating the earth and disorganizing its constituents, forming caverns, mineral veins, and residuary products; and 2nd, as a mechanical agent, loosening, removing, and comminuting the rocky particles, and finally bearing them to the sea to form new lands; but in the solid form only mechanical activity is manifested. There is, first, the enormous weight of the glacier (more than a ton per square inch beneath a glacier a mile in thickness), enough in itself to comminute rocks not strongly coherent and well supported laterally; there is then the abrading action of this tremendous weight dragged slowly forward—the ice being armed with fragments of rock frozen into its mass; and there is finally the corroding action of the sub-glacial streams (sometimes, perhaps, under great hydrostatic pressure) which constantly bear away the finer detritus and prevent the clogging of the grinding faces of the glacial mill. The rapidity of operation of these forces must be almost beyond conception. Even in the diminutive Alpine glaciers the sub-glacial streamlets are so fully charged with impalpable mud as to carry away more material in a few days than is moved by a sub-ærial stream of like size in a year. Now, since the erosive action of the ice is proportional to its thickness, and since, moreover, this action is most effectively supplemented by sub-glacial streams in valleys, it is manifest that the tendency of glaciation is to increase the depth of existing depressions, and thus intensify topographical irregularities. Accordingly, glaciated regions are characterized by deep bays and fords along the coast line, V shaped valleys intersecting mountainous areas, and elongated basin-like ponds and lakes dotting more uniform surfaces—the longer axes coinciding with the direction of ice-motion; while at the same time abrupt peaks and irregular knobs are replaced by gracefully rounded swells with trains of fragments to the leeward. Since the average rate of glacial erosion is so high (it was a foot in a thousand years, or more than seven times as rapid as sub-ærial erosion in New England) it would appear that important geographical changes ought to follow the visitation of an ice-sheet, not only by the carrying out of a new series of hills and dales, but by heaping up of piles of earth and stones of such magnitude as to necessitate the development of a new drainage-system; and accordingly just such geographical vicissitudes are abundantly attested in north-eastern United States and elsewhere.

The most conspicous evidence of glacial action is the mantle of *drift* occupying areas formerly overspread by ice. This drift consists of the materials torn up and indiscriminately intermingled by the glacier, and is generally a confused, unstratified mass of stones of all sizes and shapes, generally much worn, cemented together by sand and clay. It is sometimes heaped up in irregular moraines; some of which doubtless mark the lines of greatest extension of the ice, while others probably indicate temporary pauses or re-advances in its secular retreat. Along the coast this deposit has been re-arranged superficially by wave and tide, and has afforded material for immense accumulations of *terrace drift*; the unmodified basal portion being sometimes left in the form of gracefully arched *lenticular hills* of elliptical outline, the longer axis extending in the direction of motion of the ice. In regions not submerged at the close of the ice-period the upper portion of the drift has been modified by the action of running water and of vegetal growth. The moving water was rendered effective during the retreat of the ice, not so much by the increased volume to be borne sea-ward as by the imperfection of the nascent drainage system. The valleys were clogged with glacial waste, forming hosts of pools and lakelets which burst from time to time and shifted the hetrogeneous mass here and there in a series of pygmy debacles. The terraces of this period along the Connecticut river and its tributaries contain scores of cubic miles of drift thus re-arranged, and indicate by their altitude that much more material than that now remaining has been removed. Among the minor drift phenomena are the isolated hills and greatly elongated ridges of sand, gravel, or stratified clay, denominated *aasar, kames,* or *eskers.* "No sufficient explanation has yet been given of their origin" (p. 66). In the best instance known the deposit is probably a central terminal moraine, deposited in a valley of uniform slope, by a retreating local glacier. Other examples, however, appear to be not morainal; and it may be long before we understand the method of their formation" (p. 68).

VI. *The origin and nature of glacial periods.*—The earliest of the several hypotheses which have been put forth to explain the cause of the glacial period referred the phenomenon to the secular refrigeration of the globe; but the hypothesis is untenable, since it does not contemplate the several successive transitions from warmth to cold. A second hypothesis is that of Poisson, who suggested that in the proper motion of the solar system it might from time to time come into such proximity to, or recede to such a distance from, neighboring stellar bodies

as to materially affect the temperature of the planets. "This seemed a very reasonable view, and, indeed, it cannot well be questioned that if one-half of the heat that reaches the earth's surface comes from the stars, it is likely to be warmer near an aggregation of three suns than it is where we are now" (p. 70); but astronomical considerations show that the hypothesis as a whole is untenable. Next is the view of Lyell, who attributed climatal oscillations to changes to the relative position of sea and land; but the view is open to the sweeping objection that the formulated cause would produce opposite effects from those which the hypothesis attempts to explain. The effect of minor geographical alterations on climate cannot, however be denied. Tyndall and others have shown that slight variations in the quantity of aqueous vapor, carbonic acid, or some other substances contained in the atmosphere must materially effect terrestrial temperature; but any such variations which we are justified in assuming would probably be inadequate to alone explain the phenomena of the glacial period. The next hypothesis is that of Croll, who, recognizing the fact that the "orbit of the earth around the sun is not circular, as it might be if the earth were the only companion of the sun in the solar system" (p. 73), points out (1) that during periods of high eccentricity in the terrestrial orbit the precession of the equinoxes may lead to a considerable variation in the length of the seasons, and hence to an accumulation of snow and ice in the hemisphere having the long winter and the short summer; and (2) that when such accumulation was in the northern hemisphere the effect upon the trade winds would be such as to deflect the Gulf Stream feeders to the south of Cape St. Roque and thence into the Antarctic regions, and thus further refrigerate the northern hemisphere. The hypothesis is the most important yet enunciated, though it presents certain difficulties. Other views are that changes in the earth's axis of rotation, or in the obliquity of the ecliptic may materially affect the temperature of the globe; but these views can be mathematically proven to be inadequate. Yet another hypothesis is that which attributes the phenomenon to variations in solar emission, and which "seems a most likely cause of glacial conditions" (p. 90). Less important hypothetical causes are minor geographical changes affecting aërial or marine currents—for instance, the comparatively recent elevation of Sahara, and the probable late Quaternary depression and subsequent re-elevation of Alaska and Kamchatka; but it is clear that no conceivable array of geographical changes can explain the origin of the last glacial epoch.

(To be continued.)

THE HIPPOPOTAMUS.—Dr. Henry C. Chapman, of Philadelphia, has recently devoted much attention to the anatomy of the Hippopotamus, and has read an elaborate paper before the Academy of Natural Sciences, of Philadelphia. We notice that he draws the following conclusions: "beginning with the pig, we pass by an easy transition to the Piccary, which leads to the Hippopotamus, and thence in diversing lines to the Ruminantia on the one hand and the Manatee on the other. Paleontologists have not discovered a form which bridges over the gap between the Hippopotamus and the Manatee, but it will be remembered that certain fossil bones, considered by Cuvier to have belonged to an extinct species of Hippopotamus, *H. Medius*, are regarded by Gervais as the remains of the *Halithorerium fossile*, an extinct Sirenean of which order the Manatee is a living representative." Dr. Chapman adds further on, "I do not mean to imply that the Manatee has necessarily descended from the Hippopotamus," but he considers that "there is some generic connection between them."

PROF. C. V. RILEY believes that the diminished virulency of Phylloxera in sandy soils is due to its mechanical action on the insect, his own experiments showing the difficulty such insects meet with in soils of a sandy nature.

NEW YORK ACADEMY OF SCIENCES.

November 21, 1881.

SECTION OF BIOLOGY.

The President, DR. J. S. NEWBERRY, in the Chair.

Thirty one persons present.

The following paper was read by Prof. LOUIS ELSBERG, M. D.

ON THE CELL-DOCTRINE AND THE BIOPLASSON-DOCTRINE.

Mr. President and Fellows of the Academy, Ladies and Gentlemen.—Last May, at the meeting of the American Laryngological Association, I rendered account of some histological investigations of the cartilages of the larynx, a report of which is published in the October Number of the *Archives of Laryngology*. As the structure of hyaline cartilage has an important bearing on my subject of this evening, I crave your attention for a few minutes for a brief review of those investigations.

You know the larynx or voice-box consists of a framework of cartilage or gristle. This cartilage is called hyaline or glasslike, because it is opalescent and looks like milk-glass. Having frequently been examined under the microscope, it has always been looked upon as one of the simplest tissues, namely, as being composed of a hard matrix or basis-substance, in which are imbedded a number of small softer bodies. These softer bodies, the cartilage corpuscles, have since the establishment of the cell-doctrine been called cartilage cells. As these cells were known to be alive, the question which scientific men have had to try to answer was: how can they obtain nutrition, being isolated and enclosed in the firm, unyielding cartilage basis-substance?

Without going too much into details, I may say that it was assumed that nourishing liquid reaches the corpuscle either by imbibition and diffusion or else through canals or fissures in the homogeneous basis-substance. The idea of the existence of "juice-channels" originates with VON RECKLINGHAUSEN, although others before him had spoken of "pores" through which nutrient juices might pass. BUDGE and others believe in the presence of regular canals for this purpose, while TILLMANNS and many with him believe that hyaline basis-substance consists of fine fibrils so closely held together by a cement-substance that the mass appears to be homogeneous; it is supposed by some that this inter-fibrillar cement-substance is a viscous, soft material which permits the imbibition of nutrient liquid; by some that there are clefts or fissures, and by others that there are regular channels tunnelled in this cement-substance. On the other hand, HEITZMANN, SPINA, FLESCH and others have found that there are cilia-like offshoots or prolongations of the substance of the corpuscle penetrating into the basis-substance. Such prolongations might carry on nutrition. I have had the opportunity 6 or 7 years ago to repeat Heitzmann's observations under his own eyes and with his assistance; but the results as to their correctness at which I arrived, were to the best of my belief uninfluenced by him.

My own recent investigations have not only confirmed the existence of such offshoots and shown that they form an inter-connected reticulum or network throughout the basis-substance, but I have discovered in several specimens, small lumps in this network which, by all the tests applied to them, were proved to be lumps of living matter in various stages of existence! These investigations are illustated by the accompanying drawings viz.

Fig. 1. exhibits the appearance of a longitudinal section of the plate of the thyroid cartilage with an amplification of 100 diam.

Fig. 2. shows offshoots from the cartilage corpuscles and the network in the basis-substance with more or less large granules interwoven, as it were, in the network.

Fig. 3. shows granules of various sizes in the basis-substance with lower power of the microscope, which granules are seen with higher powers to be connected with the network of living matter, as shown by fig. 4.

Doubt as to the interpretation is impossible: instead of being a mass of basis-substance in which a number of cartilage corpuscles are imbedded, hyaline cartilage is a filigree of living matter, in the meshes of which a number of blocks of basis-substance are imbedded.

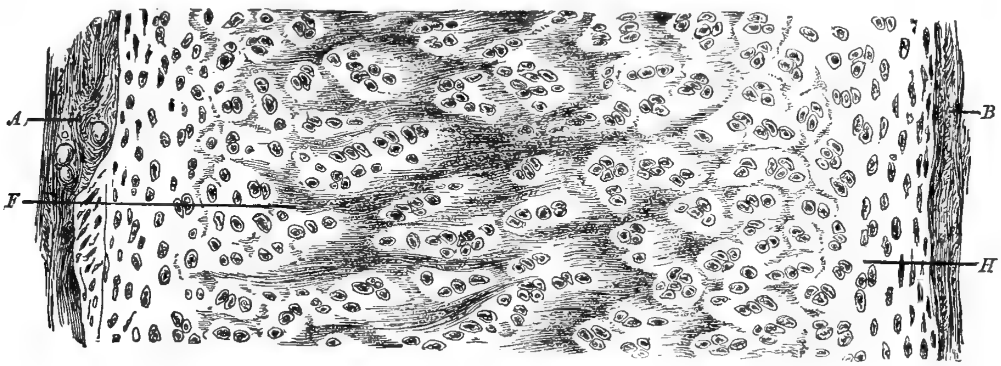


FIGURE 1.—Plate of the Thyroid Cartilage of Adult. Longitudinal Section x 100.

A. Perichrondrium towards the mucus membrane.
B. Perichrondrium toward the skin.
F. Fibrous portion of cartilage in the centre.
H. Hyaline portion, on either side, near the perichondrium.

Now, for our subject proper.

The founder of the Cell-Doctrine, Schwann, has recorded in the Introduction to his great work published in 1839 that the doctrine was based to a large extent upon investigation of the constitution of cartilage. After Johannes Müller had described cartilage-corpuscles that were hollow, and Gurlt had spoken of some as vesicles,—when Schwann had succeeded, as he thought, "in actually

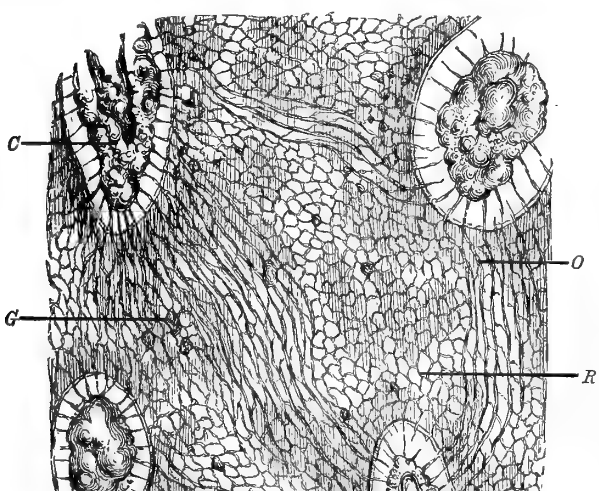


FIGURE 2.—Thyroid Cartilage of Adult, kept in strong Alcohol. Horizontal Section x 1200.

C. Shrivelled cartilage corpuscle.
O. Longitudinal off-shoots.
R. Reticulum in basis-substance.
G. Granules of living matter.

observing the proper wall of the cartilage corpuscles, first in the branchial cartilages of the frog's larvæ and subsequently also in the fish," he was led by these and other researches to conjecture "that the cellular formation might be a widely extended, perhaps a universal, principle for the formation of organic substances." And just as the study of cartilages has led to the cell-doctrine, which at the time of its establishment was a great advance in biological science, so the further study of cartilage has supplied the basis for a generalization which is a further development, and must take the place of the cell-doctrine. This is Heitzmann's doctrine of living matter, or, as I have named it, the *bioplasson-doctrine.*

When the term "cell" was introduced in 1838 and 1839, by Schleiden and Schwann, it was believed that on ultimate morphological analysis every plant and every animal would be found to consist of a number of minute vesicles or sacs, enclosing liquid contents in which is suspended a more solid body, the nucleus. For fully twenty years this idea has been known to be erroneous. In fact, *Goodsir*, nearly *forty* years ago—only a few years that is, after Schwann had established the cell-doctrine and attributed the vital power to the cell-membrane, I say,

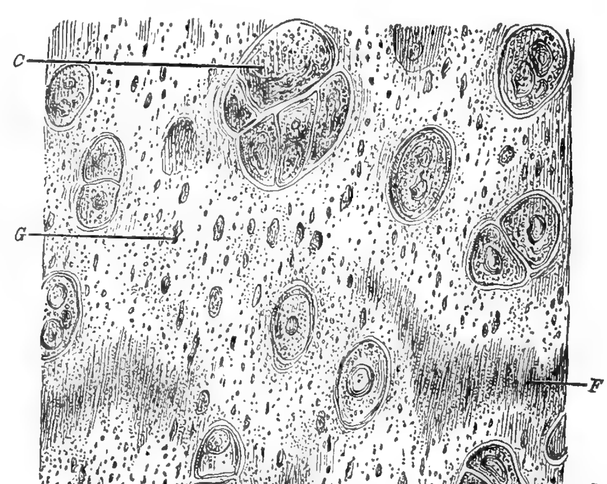


FIGURE 3.—Thyroid Cartilage of Adult. Horizontal Section x 600.

C. Cartilage corpuscle.
F. Fibrous portion of cartilage.
G. Granules of living matter.

nearly forty years ago Goodsir had experimentally determined that the seat of the vital process of secretion is ***not*** in the vesicle as such, but in the so-called cell contents; Naegeli, in 1845, and Alexander Braun, in 1851, had also shown the cell-wall to be comparatively unimportant; and in 1857 Leydig had declared the "cell" to consist only of a soft substance enclosing a nucleus. Certainly, twenty years ago it was proved beyond dispute by Max

Schultze, Beale, Hæckel, and others, that what was called a "cell" was not a vesicle, but essentially a jelly-like lump of living matter characterized by the presence of a nucleus; soon after, Robin, Brücke, Kühne, Stricker, and others, conclusively showed that not even a nucleus is an essential constituent of an elementary organism; and, biologists were compelled to transfer the power of manifesting vital properties to "living matter" instead of restricting this power to any definite form-element. As long ago as in 1861, Brücke proposed to discontinue the use of the word "cell" as being a misnomer and misleading, and offered as a substitute the expression "elementary organism." Beale proposed, instead, the term "bioplast" to designate any definite mass of living matter, and Hæckel the term "plastid." From the latter I devised the word "plastidule" as synonymous with ultimate molecule of the substance of living matter. Elementary living matter is called with Dujardin "sarcode," or with Von Mohl "protoplasm," or with Beale "bioplasm," or, still better (because it is a designation etymologically more nearly meaning living, forming matter), "bioplasson." Of these four synonymous terms "protoplasm" is the one best known; but has been used in other senses, as well as to designate, merely, elementary living matter. I therefore think that "bioplasson" is to be preferred. Of course, *dead* bioplasson is a contradiction in terms: bioplasson deprived of vitality is no longer bioplasson at all, but merely the chemical remains of what *once was* bioplasson. If this be remembered, there will be no confusion, even if the word be used in describing tissues, etc., after death. According to Drysdale, Dr. John Fletcher of Edinburgh was the first, who clearly arrived at the conclusion that "it is only in virtue of a specially living matter, universally diffused and intimately interwoven with its texture, that any tissue or part possesses vitality."

As Fletcher's work was published in 1835, several years before even the establishment of the cell-doctrine, we cannot but agree so far with Drysdale as to say that Fletcher has framed a "hypothesis of the anatomical nature of the living matter which anticipates in a remarkable manner" its discovery! In 1850, Cohn[1] recognized the protoplasm "as the contractile element, and as what gives to the zoöspore the faculty of altering its figure, without any corresponding change in volume." He concludes that protoplasm "must be regarded as the prime seat of almost all vital activity, but especially of all the motile phenomena in the interior of the cell." In 1853 Huxley[2] said "vitality, (the faculty, that is, of exhibiting definite cycles of change in form and composition), is a property inherent in certain kinds of matter." In 1856 Lord Osborne discovered carmine staining, and distinguished by means of coloring it the living formative matter from the formed material, a means which has borne important fruits in the discovery of Cohnheim's staining of living matter by gold chloride, and in that of Recklinghausen's staining all except living matter by silver nitrate.

In 1858, and in a number of later articles,[3] Max Schultze, by showing that, as had been hypothetically supposed by Unger, the movements of the pseudopodia and the granules are really produced by active contractile movements of the protoplasm, as well as by other observations, contributed much to the establishment of the theory of living matter. Hæckel has also for many years, and in various publications,[4] labored to maintain and extend the same theory, of which he thus expresses himself.[5] "The protoplasm or sarcode theory, that is the theory that this albuminous material is the original active substratum of all vital phenomena may, perhaps, be considered one of the greatest achievements of modern biology, and one of the richest in results." And says Drysdale[6]: "if the grand theory of the one true living matter was, as we have seen, hypothetically advanced by Fletcher, yet the merit of the discovery of the actual anatomical representation of it belongs to ***Beale***, in accordance with the usual and right award of the title of discoverer to him alone who demonstrates truths by proof and fact. * * * The cardinal point in the theory of Dr. Beale is not the destruction of the completeness of the cell of Schwann as the elementary unit, for that was already accomplished by others. * * * But that, from the earliest visible speck of germ, up to the last moment of life, in every living thing, plant, animal, and protist, the attribute of life is restricted to one anatomical element alone, and this homogeneous and structureless; while all the rest of the infinite variety of structure and composition, solid and fluid, which make up living beings, is merely passive and lifeless formed material. This distinction into only two

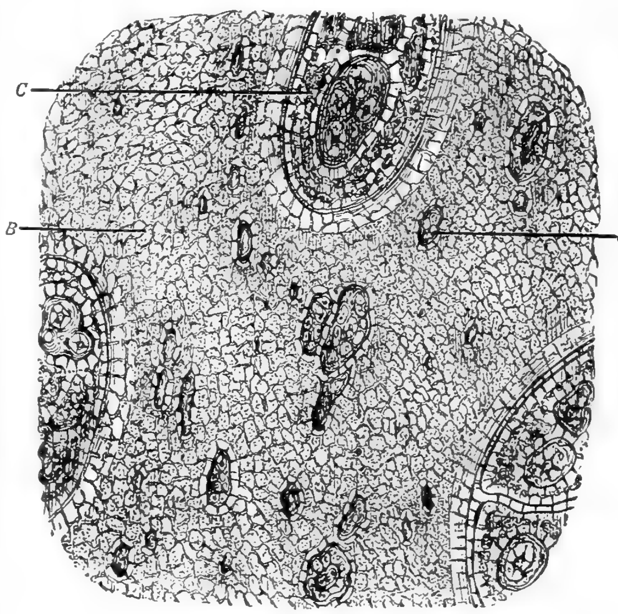


FIGURE 4.—Thyroid Cartilage of Adult. Horizontal Section x 1200.
C. Cartilage corpuscle.
B. Hyaline basis-substance.
G. Granules of living matter.

radically different kinds of matter, viz., the living or germinal matter and the formed or lifeless material, gives the *clue* whereby he clears up the confusion into which the cell-doctrine had fallen, and gives the point of departure for the theory of innate independent life of each part, which the cell-theory had aimed at, but failed to make good. The one true and only living matter—called by Beale germinal matter, or bioplasm—is described as 'always transparent and colorless, and as far as can be ascertained by examination with the highest powers, perfectly structureless; and it exhibits those same characters at every period of its existence.' * * *

The name of bioplasm, continues Drysdale, given by Beale, or protoplasm, as indicating the ideal living matter, cannot be given to any substance displaying rigidity in any degree, nor to anything exhibiting a trace of structure to the finest microscope: nor to any liquid; nor to

[1] "Nachträge zur Naturgeschichte des Protococcus pluvialis." *Nova acta Acad. Leop.-Carol.*, vol., xxii., part i., p. 605.

[2] "Review of the Cell-theory." *British and Foreign Medico-chirurg. Review*, Oct., 1853.

[3] "Ueber innere Bewegungs-Erscheinungen bei Diatomeen," *Müller's Archiv*, 1858, p. 330; "Ueber Cornuspira," *Archiv f. Naturgesch.*, 1860, p. 287; "Ueber Muskelkörperchen und das was man eine Zelle zu nennen habe," *Reichert und Du Bois-Reymond's Archiv*, 1861, p. 1; Das Protoplasma der Rhizopoden und der Pflanzenzellen, Leipzig, 1863.

[4] Monographie der Radiolarien, 1862, pp. 89, 116; "Ueber den Sarcodekörper der Rhizopoden," *Zeitsch. f. Wissensch. Zoölogie*, 1865, p. 342; Generelle Morphologie, vol. i, pp. 269, 289.

[5] Monographie der Moneren," *Jenaische Zeitschft f. Medicin und Naturwissenschaft*, 1868, iv, 1; translation in *Quarterly Journal of Microscopical Science*, London, 1869, vol. ix, p. 223.

[6] *Loc. cit.*, 42, *et seq.*

any substance capable of true solution. Thus, 'nothing that lives is alive in every part,' but as long as any individual part or tissue is properly called living it is only so in virtue of particles of the above-described protoplasm freely distributed among, or interwoven with the textures so closely that there is scarcely any part -1- of an inch in size but contains its portion of protoplasm. Thus we see realized the hypothesis of Fletcher, that all *living action* is performed *solely* by virtue of portions of irritable or living matter *interwoven* with the *otherwise dead* textures." The objection, however, urged by *Bastian* to Beale is so very pertinent, that it must also find a place here, but I shall not dwell upon other points on which Beale differs from the bioplasson doctrine; such as, that living matter exhibits the same characters at every period of its existence; and that it is always perfectly structureless. "It has always appeared to me," says Bastian,[1] "to be a very fundamental objection to Beale's theory, that so many of the most characteristically vital phenomena of the higher animals should take place through the agency of tissues—muscle and nerve, for instance—by far the greater part of the bulk of which would, in accordance with Dr. Beale's view, have to be considered as *dead* and inert."

In 1873, the morphological knowledge of living matter became exact. In that year, Heitzmann discovered the manner in which bioplasson is arranged throughout the body, and announced the fact that what had until then been regarded as *separate form-elements* in a tissue are really *interconnected portions* of living matter; that not only are there contained *no* isolated unit-masses in any *one* tissue, but *no tissue* in the *whole body* is isolated from the *other* tissues; and that the only unconnected particles of living matter are the corpuscular elements of liquids, such as blood, sperm, saliva, pus, etc., and so-called wandering corpuscles; so that, to use his own words: "the animal body as a whole is a connected mass of protoplasma in which, in some part, are imbedded isolated protoplasma-corpuscles and various not-living substances (glue-giving and mucin-containing substances in the widest sense, also fat, pigment-granules, etc.)." This announcement marked the commencement of a new era in biology.

Heitzmann discovered that the living matter as seen in an amœba is *not without structure*, as had, before his accurate investigations, been supposed; and that its structure, in all cases when developed, is that of a network, in the meshes of which the bioplasson fluid, or the not-contractile, not-living portion of the organism, exists. When there is a nucleus, it is connected by delicate threads with the extranuclear network; nucleoli and nucleolini inside of the nucleus, as well as granules outside, are portions of living matter: sometimes in lump, sometimes mere points of intersection of the threads constituting the intranuclear and extranuclear living networks, sometimes terminals of section of such threads, as first explained by Eimer,[1] and after him by Klein.[2]

Heitzmann discovered that what is true of the structure of bioplasson in the amœba, where a single small unit-mass of living matter constitutes the entire individual, is true also of the structure of bioplasson of all, even the highest, living organisms.

To be sure much had been previously known regarding protoplasm or living matter, but the knowledge was fragmentary, until Heitzmann demonstrated *not only*, that membrane, nucleus, nucleolus, granules, and threads *are really* the living contractile matter, but also, 1st, that this matter is arranged in a network, containing in its meshes the non-contractile matter, which is transformed into the various kinds of basis-substance, characterizing the different tissues of the body; and 2d, that the tissue-masses of bioplasson throughout the whole body are *interconnected* by means of fine threads of the *same* living matter.

The Beginnings of Life: being some account of the nature, modes of origin, and transformations of lower organisms. London, 1872, vol. i., p. 155.

[1] "Weitere Nachrichten über den Bau des Zellkerns." *Archiv f. mikrosk. Anatomie*, XIV, 1877, p. 103.

[2] "Observations on the Structure of Cells and Nuclei," *Quarterly Journal of Microscopical Science*, Jan., 1879, p. 128. "The intranuclear as well as the intracellular network having, of course three dimensions, includes fibrils that lie in the two dimensions of the plane of the field of the microscope, as well as fibrils placed vertically to it. The former appear, of course, as fibrils; but, I should like to ask, as what do the latter appear, *i. e.*, those situated vertically. Clearly as dots, because they are seen endwise; and for obvious reasons most of them lie in the nodes of the network."

Unless these two facts of Heitzmann's discovery are accepted, there cannot be urged much against the continued use of the word "cell," misnomer though it be. Ranke,[1] after speaking of the "cell-wall," "cell-nucleus," etc., says: "of these component parts of the cell, one or other may be wanting without the totality ceasing to be a cell. The nucleoli, the cell-wall, or the nucleus may be wanting, and yet we must designate the microscopic form a cell, or elementary organism." Drysdale thus comments upon this quotation, viz.: "if any one choose to describe a gun-barrel as a stockless gun without a lock, he is free to do so; but what good purpose can it serve? Or is there even any fun in it? The truth is, this clinging to the mere name of the cell-theory by the Germans seems to arise from a kind of perverted idea of patriotism and of *pietas* toward Schwann and Schleiden." But, I think Tyson[2] has the better of the argument, in saying: "the word "cell" has become so intimately associated with histology, that it is doubtful whether it will ever fall into disuse, nor does it much matter, so long as correct notions of the elementary part are obtained." Now, if *there were any* separate and distinct "elementary part," it certainly would matter little or nothing whether it were called "cell" or by any other name, provided the name be properly defined and agreed upon. It is not against the name but against the idea of any *isolated individualized form-element* that the objection lies. Virchow maintains[1] "that the cell is really the ultimate morphological unit in which there is any manifestation of life, and that we must not transfer the seat of real action to any point beyond the cell." Against this statement nearly every author nowadays protests, and insists that vital power must be transferred from the "cell" to "living matter"; yet, after all, the disagreement, though ever so strenuously declared, is a mere verbal one: so long as both parties hold that "every higher animal presents itself as a sum of vital unities"—no matter what these unities are called or how defined. Hæckel, one of the most avowed advocates of "the protoplasm or sarcode theory," clings to Virchow's politico-physiological comparison, that every higher organism is like an organized social community or state, in which the individual citizens are represented by the "cells" [no matter how he may define these], each having a certain morphological and physiological autonomy, although on the other hand interdependent and subject to the laws of the whole. Heitzmann's views necessitate the comparison of the body to a machine, such as a watch or a steam-engine, in which, though there are single parts, no part is at all autonomous, but all combine to make up one individual. Even Huxley, the popular champion of protoplasm as the physical basis of life, quite recently delivered an address, before the International Medical Congress in London, August 9, 1881, in which he used the following language: "in fact, the body is a machine of the nature of an army, not of that of a watch, or of a hydraulic apparatus. Of this army, each cell is a soldier," etc., etc. According to Hæckel and Huxley, the body is composed of *colonies* of amœbæ; according to Heitzmann the body *is one com-*

[1] The Cell-Doctrine; its history and present state. Philadelphia, 1878, p. 128.

[2] "Physiologie, 1872," quoted by Drysdale, *loc. cit.*, p. 104.

[1] Die Cellularpathologie in ihrer Begründung auf physiologische und pathologische Gewebelehre, Berlin, 1858, p. 3. (Translation by Chance, London, 1859, p. 3.)

plex amœba. I am very anxious to really make the difference between the cell theory and the bioplasson theory clear to every one of you. The essential point of the cell theory is, the idea that the body and each tissue of the body, every plant, and every animal, is made up of a number of distinct units, and the essential point of the bioplasson theory is, the idea that all the masses of living matter of each tissue of plants and animals are uninterruptedly connected, and that every tissue is connected with every other tissue by filaments of living matter. To accept Mr. Huxley's comparison, we must imagine that every soldier is indissolubly connected, hand and foot with every neighboring soldier of the solid army!

There is no *better test* of the TRUTH of the bioplasson doctrine than the structure of hyaline cartilage. If hyaline cartilage consisted, as "is generally believed," of "a homogeneous ground substance, in which are closed cavities harboring the corpuscles," [1] the bioplasson doctrine would certainly be erroneous. If it merely contained lymph, or juice-channels, no matter what their character, whether open or closed, whether lined or unlined, whether in "homogeneous basis-substance,' or "between layers of cells," or "in cement-substance," then, too, the bioplasson doctrine would be erroneous.

But the result of my observations, especially those illustrated in figs. 2, 3, and 4, admit of but one interpretation, and that an interpretation favorable to the bioplasson doctrine. It is unnecessary to more than mention that although I have placed on record so few, I have made many different examinations, under many different circumstances, and with varying powers of amplification. I need occupy myself here with only the two fields drawn in figs. 3 and 4, with an amplification of 600 and 1200 respectively. The remarkable specimens from which they are taken show more conclusively than it was ever before shown *what* the structure or constitution of hyaline cartilage *really* IS. I think I have *explained* this sufficiently, but its full significance appears in its *corroboration* of the *bioplasson* doctrine.

To be able to uphold the cell-doctrine, cartilage would have to be, using a homely comparison, like a cake composed of hard dough with raisins. No matter how widely we may extend the definition, to remain within the boundary of the cell-doctrine this metaphor must be applicable. Innumerable painstaking researches have led to various modifications of notions entertained regarding the structure of the two constituents of the cake and their relation to each other. It may be seen by the most recent publications on the subject, that the acceptation of the existence in the dough of cleavage in certain directions, of interlaminary and interfibrillar spaces, and of offshoots, even ramifying prolongations of the raisin-substance, or, at all events, of an ingredient of the raisins, is held to be not incompatible with the cell-doctrine. If, however, we can represent cartilage as a filigree or framework of raisin-substance, in the meshes or interspaces of which framework blocks of dough are imbedded, certainly the fundamental view of the ultimate construction of the tissue is changed, and we are no longer in accord with the cell-doctrine, even though we be inclined to use that term in the widest possible sense. Look for a moment at the two illustrations on the blackboard, as well as at figs. 2, 3, and 4. The upper figure represents a section of cartilage stained with gold chloride. This, as I have already alluded to, stains the living matter and leaves the basis-substance unstained. High powers exhibit the appearance, etc., etc. In regard to a NAME as a *substitute* for the term "*cell*," I would say that all corpuscular masses may be called, simply, corpuscles—thus we may speak of blood-corpuscles, pus-corpuscles, etc. For all the accumulations of living matter within the ordinary fields of basis-substance, but more especially for those smaller masses which, having as yet developed neither a network structure nor much vacuolation, are still homogeneous, or nearly so—I am quite willing to adopt either the designation of "plastids," proposed by Hæckel, or that of "bioplasts," proposed by Beale. Perhaps it would be well to restrict the word "bioplast" to a small mass of living matter exhibiting no differentiation, and distinguish from it as "plastid" the larger mass showing an interior structure more or less like the fully developed corpuscle. Thus, I would always use the term "plastid" in the place of "cell."

The result of my investigations as to the structure of cartilage is that in this tissue, beyond the possibility of a doubt, the living matter is arranged in the form of a network containing in its meshes the non-contractile matter. How is it with regard to the other proposition of the bioplasson doctrine, viz., that the living matter of the different tissues is interconnected? Examinations with high powers of such a specimen as that represented in fig. 1, showing the perichondrium, of horizontal sections through the larynx, or the neck, with skin and more or less of other tissues included, enable me to answer this question to the effect that fine filaments of living matter pass from one tissue to another in connection with the network of living matter in each. The details of these examinations are reserved for another time! But it has been suggested to me that I ought not to conclude without saying a few words as to the *practical* advantages of the Bioplasson Doctrine over the Cell-Doctrine. Well, *every exact* scientific investigation, even though at first of *theoretical value* only, sooner or later brings with it some practical benefit; and this doctrine of living matter, aside from the satisfaction which the perception of ABSTRACT *truth* grants—lying as it does at the foundation of our knowledge of living things—has advanced their physiology, and pathology at every point! In *Practical Medicine* it has already aided us in so many ways that their merest enumeration would require another hour's lecture. We know that the disposition of living matter is different in different persons, and that in the case of increased supply of food the *reaction* is different in strong and healthy people from that in the sick and weak. Upon this knowledge rests, to-day, the *whole doctrine of pulmonary consumption.* Now, the *amount of living matter* within the same bulk *varies greatly*, both in normal and morbid conditions. A small lump of bioplasson in the urine or expectoration, taken from an individual of *good* constitution, will show a *close* network with coarse granulations, or perhaps be almost *homogeneous*-looking under the microscope—owing to the *large amount* of living matter in the small bulk: while a plastid from a weak, broken down or phthisical person will be finely granular and exhibit a *network with large meshes* on account of the *relatively small amount of living matter* in it. Sometimes we thus from the examination of a *drop of blood* gain an insight into the condition and vital power of the whole individual; sometimes, recognize a disease before it is sufficiently developed to do much harm, and thus come a step nearer to the *highest aim* of the physician:—the *prevention* of disease.

DISCUSSION.

Dr. B. N. Martin remarked on the great value and important bearing of this investigation.

Mr. A. C. Elliot enquired whether the blocks of non-living matter in the cartilage were entirely separated.

Dr. Elsberg explained that the blocks were separate, their only connection being the interposed threads of the *reticulum* of living matter, and to the former is due the opalescent character of hyaline cartilage. He further stated that the condition of health of an individual might be inferred in a degree from a study of the character of the

[1] This statement of the general belief is quoted from the introductory paragraph of Thin's memoir "On the Structure of Hyaline Cartilage" (*Quarterly Journal of Microscopical Science*, xvi, 1876) in which Thin's own views are laid down to the effect "that layers of cells epithelial in arrangement exist in the substance of cartilage," "that both the stellate and the parallel systems of lymph-channels exist," etc.

network, a thin section of a very minute portion of the body often showing a difference of network in different persons, *e. g.*, in the thickness of the threads, the size of the meshes, the character of the points of intersection, etc. From the uniformity in the size of the meshes, etc., or from their variability, or from the proportion of corpuscles presenting a normal and abnormal character in their network, a good or bad prognosis was deduced by the physician, and even an indication of the progress of disease.

Prof. E. H. Day remarked on the wonderful character of protoplasm in its wide results in the construction of the most varying textures in the vegetable and animal kingdoms. The speaker's observations have brought the protoplasm of cartilage tissue into correspondence with that in the tissues of the sponge, of the plant, and all the lower forms of life. In protoplasm we are brought face to face with the most astonishing substance in nature.

Mr. J. D. Warner offered objections to the vague views of Virchow on the soul of the cell and its relation to the soul of the individual.

Dr. Newberry said that, having been educated as a physician, and having studied microscopic anatomy under Dr. Charles Robin, he had followed with great interest the progress of modern research into the ultimate structure of organic tissue, and the discussions of the origin and seat of vitality to which it has given rise; and he regarded such investigations as those of Dr. Elsberg as of the highest scientific interest and practical value. If we ever learn the causes of malarial and infectious diseases, or the cure of the morbid growths which are the scourges of humanity, cancer and tubercle, it will be through such researches. But he thought that much of the discussion which had been excited by these investigations had been irrelevant and confusing, especially that in regard to the seat and nature of life, into which microscopists and chemists had entered with great earnestness and some acrimony, but with no satisfactory result. In this discussion some writers had made the ultimate cell the seat of life, and had glorified and almost deified it. Others claimed that the cells were only portions of a general vitalized and automatic tissue; while others still contended that the phenomena of vitality were the mere manifestations of chemical changes taking place in structure otherwise lifeless.

With none of these views could he sympathize as there had really been no approach to an end in the effort to localize or analyze life. Unless we accept the materialistic theory of spontaneous generation advocated by Dr. Bastian, but rejected by most biologists, we must confess that no more is now known of the origin, nature and seat of life than was known to Aristotle. All we have done is to acquire a better knowledge of the *machinery* by which the functions of life are accomplished; most important knowledge truly since it enables us to distinguish between normal and morbid life action in the tissues where this action begins, and promises to point the way for promoting the one, and preventing the other—but limited to the methods in which the life force acts, not reaching the inscrutable and intangible force itself.

The work done by a microscopic cell is wonderful and incomprehensible to us, yet all cells work not as independent individuals, but as members of a community, and for a common end. For example, the terminal cell of the fibril of a plant root is a delicate vesicle—the cell in its simplest form, and yet when new born, and having existed but the fraction of a minute, it begins its special work of supplying certain food elements to the plant above; and this it does with a discrimination which is infallible. Water it absorbs by endosmosis, and when deficient begets progeny to send for it. It also appropriates other things that are necessary to the growth of the plant to which it belongs, whatever that be; if tobacco, an unusual quantity of potash; if grass, of silica. It always works to a pattern determined by the character of the plant whose general economy it serves, and is controlled by the influence which gives to that plant its special and recognizable leaf, flower and fruit, its noxious or alimentary qualities. So in all other parts of the structure the cell is doing its allotted work in a community of which it forms an integral part. It is therefore in no sense an independent individual. Our notions of what constitutes an individual or a community may seem to us quite clear, but they are in fact likely to be somewhat confused. Every man recognizes and asserts his own individuality, but we all know that men who live in communities often think and feel as one though many. A great grief crushes all alike, a great danger rallies all in defence. The social insects, ants and bees, retain their corporeal individuality, but are curiously linked together in a common life that makes each but a part of a whole. A tree is universally accepted as an individual, but as all know it may be divided to form an unlimited number of perfect trees which expand this individual into a forest and prolong its life indefinitely. The sponge is said to be a community of amœboid individuals, but these share a common skeleton, fashioned for the wants of all, and all unite in the general function by which the inhalent and exhalent currents are maintained, a function on which the life of all depends. In the corals which live in communities we find the common skeleton covered with a vitalized gelatinous integument on which are set here and there the individual polyps. These live to a great degree each for itself; each throws out its tentacles and forages for its own support, but at the same time it shares a life with its neighbors; an injury done to one affects those about it, and a misfortune involving a sufficient number destroys the life of the colony.

The elusive and intangible nature of the life which pervades plant tissue is well shown in the growth and decay of a tree. From a microscopic germ a young *Sequoia* springs into existence, and for a thousand years or more lives its life. All this time it is inspired by a power which acts in antagonism to the affinities of inorganic chemistry, in opposition to the force of gravitation, and which builds up a mass hundreds of tons in weight, mostly obtained by the breaking up of one of the strongest bonds in chemistry, that of carbonic acid, appropriating the carbon and setting the oxygen free. Every part of the huge structure is pervaded by this peculiar creative and conservative influence; and every cell of root or stem or leaf contributes its part to the harmonious whole. At length the time arrives when this peculiar influence which we call life deserts the structure it has created. The affinities of inorganic chemistry now assert themselves, all the ephemeral fabric is rapidly disorganized, and soon a heap of ashes—the inorganic matter woven into its composition—alone remains to tell of its existence. Who can tell us what was the nature of the enchantment which created this Aladdin's palace—whence it came, where it dwelt during its sojourn, and whither it has gone? We may say it resided in the terminal root cells; but these are inseparably connected with the leaves hundreds of feet above. The tie that binds them is a vital one; neither could live without the other, nor without the intervening chain which connects them.

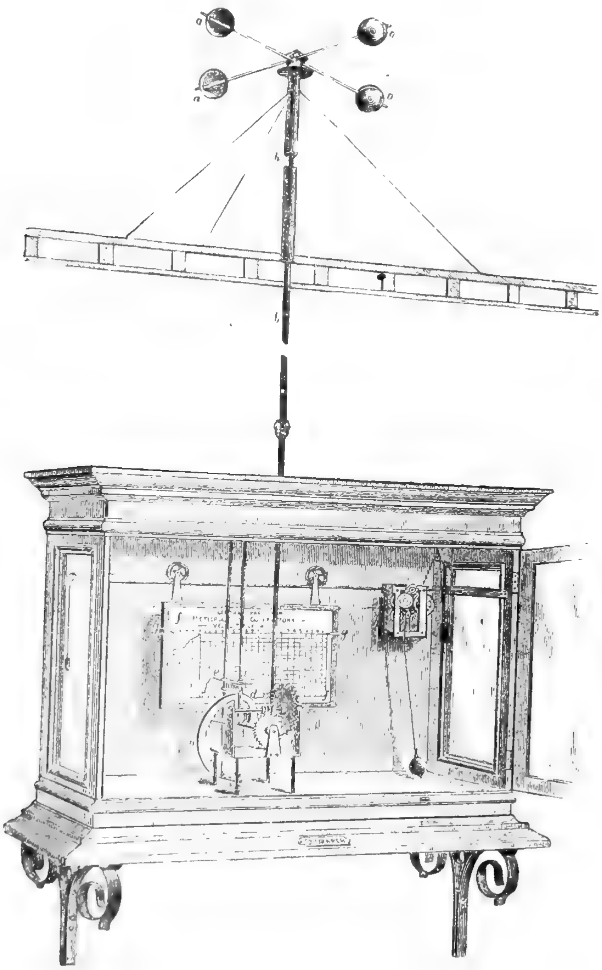
By studying the anatomy of plants and animals, we obtain a knowledge of the organs and laws, as we call them, of animal and plant life; that is, we get a knowledge of the machinery with which the functions of life are accomplished, a knowledge of the order and manner in which these functions are performed; but the *primum mobile*, the real "power behind the throne," remains as yet unseen and unknown to us.

SLIDES OF MARINE ALGÆ.—The Rev. A. B. Hervey, of Taunton, Mass., author of the very beautiful work on "Sea Mosses," will mail to any address for two dollars, a set of six slides showing the characteristic fruit of the six great groups into which Professor Agardh divides the Red Algæ.

DR. D. DRAPER'S INSTRUMENT FOR RECORDING THE VELOCITY OF THE WIND.

On the ends of a cross supported by a vertical shaft several feet above the roof of the building, are four hemispherical copper cups. These, whatever may be the direction of the winds, are caused to turn round with a speed, as has been determined by experiment, of about one-third the velocity of the wind. This portion of the contrivance was the invention of Dr. Robinson, of Ireland. It is used in the foreign observatories, and is known as Robinson's cups.

To the lower end of the shaft thus made to revolve by the cups is attached an endless screw connecting with a train of wheels, which move a cam. The wheels are so arranged that one turn of the cam answers to 15 miles in the movement of the wind. A pencil which rests on the edge of the cam, and bears lightly against a surface, is carried from the bottom to the top of the paper by each revolution of the cam. It should be understood that the paper is attached to a board drawn aside by a clock at the rate of half an inch an hour. The number of times that the pencil moves from the bottom to the top of the paper, multiplied by 15, gives the number of miles that the wind has moved in an hour or day.



The four hemispherical cups, *a a a a*, turned round by the wind, impart their motion to a vertical shaft, *b b*, at the bottom of which is the endless screw, *c*, its lower end resting in a small agate cup filled with oil, connected with the train of wheelwork turning the cam, *d d*. At *e*, is the pencil resting on the edge of the cam ; *f f* is a sheet of ruled paper attached to the board, *g g*, by means of small brass clamps, which is drawn aside at the above mentioned rate by the clock, *h*.

STATISTICS OF THE SUN.

The following *Statistics* of the Sun, comprising facts which can be stated in numbers, are selected from Professor C. A. Young's recent work "The Sun," being one of the last additions to Messrs. Appleton's International scientific series.

Solar Parallax(equatorial horizontal), 8.80″ ± 0.02″

Mean distance of the sun from the earth, 92,885,000 miles, 149,480,000 kilometres.

Variation of the distance of the sun from the earth between January and June, 3,100,000 miles, 4,950,000 kilometres.

Linear value of 1″ on the sun's surface, 450.3 miles; 724.7 kilometres.

Mean angular semi-diameter of the sun, 16′ 02.0″ ± 1.0″.

Sun's linear diameter, 866,400 miles; 1,394,300 kilometres. (This may, perhaps, be variable to the extent of several hundred miles.)

Ratio of the sun's diameter to the earth's, 109.3.

Surface of the sun compared with the earth, 11,940.

Volume or cubic contents, of the sun compared with the earth, 1,305,000.

Mass, or quantity of matter, of the sun compared with the earth, 330,000 ± 3000.

Mean density of the sun compared with the earth, 0.253.

Mean density of the sun compared with water, 1.406.

Force of gravity on the sun's surface compared with that on the earth, 27.6.

Distance a body would fall in one second, 444.4 feet; 135.5 metres.

Inclination of the sun's axis to the ecliptic, 7° 15′

Longitude of its ascending node, 74.°

Date when the sun is at node, June 4–5.

Mean time of the sun's rotation (Carrington), 25.38 days.

Time of rotation of the sun's equator, 25 days,

Time of rotation at latitude, 20,° 25.75 days.

Time of rotation at latitude, 30,° 26.5 days.

Time of rotation at latitude, 45,° 27.5 days.

(These last four numbers are somewhat doubtful, the formulæ of various authorities giving results differing by several hours in some cases).

Linear velocity of the sun's rotation at his equator, 2.261 miles per second; 2.028 kilometres per second.

Total quantity of sunlight, 6,300,000,000,000,000,000,000,000,000 candles.

Intensity of the sunlight at the surface of the sun, 190,000 times that of a candle flame ; 5300 times that of a metal in a Bessemer converter ; 146 times that of a calcium light, 3.4 times that of an electric arc.

Brightness of a point on the sun's limb compared with that of a point near the centre of the disk, 25 per cent.

Heat received per minute from the sun upon a square metre, perpendicularly exposed to the solar radiation at the upper surface of the earth's atmosphere (the solar constant), 25 calories.

Heat-radiation at the surface of the sun, per square metre per minute, 1,117,000 calories.

Thickness of a shell of ice which would be melted from the surface of the sun per minute, 48½ feet; or 14¾ metres.

Mechanical equivalent of the solar radiation at the sun's surface, continuously acting, 109,000 horse-power per square metre ; 10,000 (nearly) per square foot.

Effective temperature of the solar surface (according to Rosetti), about 10,000° Cent.; or 18,000° Fahr.

WE have to thank our English contemporary the London "*Lancet*" for its acknowledgement of the interesting nature of the articles published in "SCIENCE."

THE Governor of Texas has taken steps to form a permanent organization preparatory to the establishment of a State University.

FRENCH ACADEMY OF SCIENCES.

Nov. 7, 1881.

Action of cold on the voltaic arc.—According to M. D. Tommasi, the voltaic arc is considerably enfeebled if, by means of a current of water, the electrodes between which it is produced, are cooled. Its brilliancy is then not intense and the least breath extinguishes it. Its temperature also is relatively but little elevated. These facts are evident *à priori*. The magnet displaces and extinguishes it. It is the same on the approach of an inflamable body.

Molecular Physics.—M. Fizeau sent a communication relative to the variation in length of a bar of zinc brought to an even temperature after having undergone different actions. The application which these facts will receive is immediately seen in point of view of the construction of metallic measures.

The accents of the deaf and dumb.—It is known that the deaf and dumb can be taught to speak so that we can converse with them very nearly as well as with men who have possession of both of these senses. M. Félix Hément announces the fact that the individuals who can thus speak, are affected by the particular accent of their native province. As they have not acquired this accent by imitation, since they are deaf, the author thinks that the reason must be sought in the arrangement of the phonetic apparatus, special to each race.

Mechanics.—It is in a special way that M. Bertrand announces a memoire by M. Lévy, relative to the transportation of force to a distance by electricity. It is known that calculus demonstrates that a 16 horse-power engine can transmit force to a distance of 10 to 50 kilometres. M. Lévy states that a much superior result can be obtained.

INFECTED PORK.

It is not only Trichinæ which is to be dreaded in the hams exported by the United States; in one of the recent meetings of the Medical Congress, Dr. E. Ballard and Mr. E. Klein called the attention of the public to another still more dangerous parasite in the same meat. The *Journal of Hygiene* makes the following remarks on this subject:

In 1880, on the estates of the Duke of Portland, at Welbeck, twenty persons were taken seriously ill, after a dinner in which boiled ham had been served, with pork imported from America. Four persons died; others felt no evil effect. The morbid symptoms showed nothing very characteristic (choleric diarrhœa, vomiting, pains in the muscles, great prostration); the autopsy only revealed pulmonary congestion. In a piece of kidney, examined under the microscope, there were found traces of inflammation of the parenchyma, and in the capillaries of the Malpighian tufts, incrustations formed by masses of Bacilli.

In passing over the field of the microscope, particles of the raw ham and of the boiled ham which was infected, a species of Bacillus with its spores was found; the bacillian threads and the spores adhered closely to the muscular fibres and to the intermuscular tissue.

Experiments were made on animals: 1. By feeding and by inoculation, or by the two methods combined. 2. By inoculation after cultivation of the bacillary matter in the incubator. In every case sickness was caused, and at the autopsy, lesions of pneumonia or pulmonary hemorrhage were established.

A second series of observations was made on fifteen persons who felt serious symptoms after having eaten a leg of pork, roasted in the oven, bought at a second-class cookshop. One of them having died, Bacilli were found, at the autopsy, in the blood of the heart, in the blood pressed out of the pulmonary tissue, and in the blood extravasated around the pulmonary alveoli. The tissues of the stomach, the ileum, the spleen, and the kidneys, also contained Bacilli.

Experiment by inoculation with different liquids, on animals, caused morbid and often mortal symptoms. Bacilli were also found in the blood, and in the different tissues of the animals. Unfortunately, in this case, the suspected food could not be examined.

In the face of these facts, Messrs. Ballard and Klein do not hesitate to admit an acute specific affection, not to this day defined, and presenting marked characteristics, in point of view of the morbid phenomena, with the known cases of poisoning by damaged or trichinated meat.

Dr. Tripe, of London, recalled two febrile maladies which he had observed in his medical circumscription. In the first, sixty-six persons showed alarming morbid symptoms, after a dinner in which sausage containing a mixture of beef and pork fat, had been served. In the second it was pork fat alone which was the immediate cause of the sickness. Dr. Buchanan mentions cases of diseases, in which beef and mutton constituted the infected substances.—*La Nature*.

THE COMPARATIVE ACTION OF DRY HEAT AND SULPHUROUS ACID UPON PUTREFACTIVE BACTERIA.*

Pieces of woolen and cotton cloths and wadding were dipped in a solution of putrefying flesh and slightly dried; and after being shown to be infected by causing discoloration and development of bacteria in a Pateur solution, one portion was subjected to dry heat, and the other to the influence of a definite quantity of sulphurous acid. When these agents had operated for a certain time, the substances were brought into a developing liquid and again observed.

These experiments, which were conducted by Dr. Wermch, were as follows:

First. Fragments of the materials above referred to, treated as mentioned and dried, produced in sixteen experiments an exceptionably rapid disturbance of the test liquid. In four experiments with wadding this was somewhat retarded. It took place most rapidly in tubes which had been inoculated with woolen thread.

Second. After inoculation with the material which had been exposed one or two minutes to a dry heat of 284° to 300° F., clouding took place in four of eight experiments; but only after from two to three days. With material which had been exposed from ten to sixty minutes to a heat of 230°–244° F., in five out of six experiments a development of bacteria took place after the end of twenty-four hours.

Third. Substances which were exposed five minutes to a heat of 257° to 302° F. produced no infection whatever in ten experiments. The test liquid remained clear for eleven days from the time of inoculation.

Fourth. When the objects were exposed under a bell glass to the action of a percentage, by volume, of 1.5, 2.2, and 3.3 of sulphurous acid, in eight out of nine experiments a bacterial clouding was developed in the sulphurized material, whether the application had continued for one hour or twenty-two.

Fifth. In fifteen experiments, in which sulphurous acid constituted 4.6 and 7.15 per cent. by volume, of the contents of the bell glass, the introduction of the sulphurized material produced no cloudiness, when the experiment continued six hours and more. On the other hand an exposure of 20, 40, 60, and 200 minutes was followed by the development of bacteria.

In conclusion, the fact was considered especially interesting that the different fabrics gave up the infection concealed in them with different degrees of rapidity, the woolen fiber the quickest, the linen less easily, and the wadding with the greatest difficulty of all.

* From the proceedings of United States National Museum.

NOTES ON THE MORTALITY AMONG FISHES OF THE GULF OF MEXICO.

Prof. SPENCER F. BAIRD, *U. S. Commissioner of Fish and Fisheries, Washington, D, C.:*

SIR: Noticing in the Forest and Stream of 9th instant some answers to queries as to the cause of mortality among fishes in the Gulf of Mexico, I make bold to present the result of my observations.

After very heavy rains and overflowing of rivers, the inner bays on the Texas coast suffer a loss of from one-half to three-fourths of their stock of salt water fish, not including mullet, which live as well in fresh as salt water. In fact land-locked mullet in a fresh water pond or tank grow to a weight of nine or ten pounds.

Last winter, after a heavy rain and a freeze, *all* the salt water fish in the *Laguna del Madre* (a large sheet of water lying between Padre Island and the mainland) were found dead on the banks.

We have two causes for the destruction of fish here, vis., too much fresh water and too cold weather.

In the lagoon above spoken of, in a long drought, the water gets too salty for the fish, and they become covered with sores, and unless relieved by a rain they die from too much salt.

I have never known any serious mortality among fish on the Gulf coast where there was a free flow of water, except during violent storms, when many fish both small and large were beached and killed.

Very respectfully,

S. H. JOHNSON.

CORPUS CHRISTI, Texas, *June* 17, 1881.

ANGLO-AMERICAN ARCTIC EXPEDITION.

Commander Cheyne's paper, describing his proposed Arctic expedition in conjunction with Lieut. Schwatka, read before the N. Y. Academy of Sciences, on the 28th ultimo, will be printed in our next issue, with the regular proceedings of the Academy. We understand that Commander Cheyne is receiving a strong support from those interested in the subject, and that there is an early prospect of his plans taking a definite form.

The English Arctic Council for the organization of this expedition, meet on the 13th instant, and are awaiting a cablegram, informing them of the prospects of success, for international co-operation.

ADVICE TO ASTRONOMERS.—In Sir William Herschel's work "*On the Construction of the Heavens,*" the following line of conduct for astronomers is indicated: "in an investigation of this delicate nature we ought to avoid two opposite extremes. If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavor to keep a proper medium, but if I should deviate from that I could trust not to fall into the latter error.—*See Holden's and Hasting's Synopsis.*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING DEC. 3, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

NOVEMBER AND DECEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun
Sunday, 27	29.939	30.016	7 a. m.	29.848	6 p. m.	43.0	38.3	50	4 p. m.	43	7 p. m.	35	7 a. m.	33	7 a. m.	103.
Monday, 28	30.345	30.398	9 p. m.	29.976	0 a. m.	30.0	27.3	48	0 a. m.	43	0 a. m.	27	9 a. m.	25	9 a. m.	96.
Tuesday, 29	30.204	30.374	0 a. m.	30.098	12 p. m.	40.0	36.6	49	4 p. m.	45	7 p. m.	28	6 a. m.	27	6 a. m.	95.
Wednesday, 30	30.026	30.098	0 a. m.	29.908	12 p. m.	47.0	44.7	54	3 p. m.	49	3 p. m.	37	9 a. m.	37	9 a. m.	103.
Thursday, 1	29.754	30.082	12 p. m.	29.518	1 p. m.	49.3	47.3	56	4 p. m.	52	12 m.	43	12 p. m.	40	12 p. m.	69.
Friday, 2	30.284	30.306	9 a. m.	30.082	0 a. m.	39.3	35.7	44	3 p. m.	40	0 a. m.	36	7 a. m.	34	7 a. m.	104.
Saturday, 3	30.131	30.272	0 a. m.	30.088	6 p. m.	38.0	36.0	40	3 p. m.	38	3 p. m.	35	5 a. m.	33	5 a. m.	76.

Mean for the week .... 30.097 inches.
Maximum for the week at 9 p. m., Nov. 28th .... 30.398 "
Minimum " at 1 p. m., Dec. 1st .... 29.518 "
Range .... .880 "

	Dry.	Wet.
Mean for the week	40.9 degrees	37.9 degrees.
Maximum for the week, at 4 pm., Dec. 1st	56. "	at 12 m 1st, 52. "
Minimum " " 9 am., Nov. 28th	27. "	at 9 am 28th, 25. "
Range " "	29. "	27. "

NOVEMBER AND DECEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE.
Sunday, 27	w. s. w.	w. s. w.	w. s. w.	278	8	1.30 pm	.162	.143	.215	80	42	69	0	4 cir. cu.	8 cu.	-----	-----	-----	--	8
Monday, 28	n.	n.	e. s. e.	204	9	0.50 am	.136	.101	.119	88	58	68	0	0	0	-----	-----	-----	--	0
Tuesday, 29	n. n. e.	s. w.	w.	31	½	1 30 pm	.124	.179	.241	77	55	63	0	8 cu.	5 cu.	-----	-----	-----	--	0
Wednesday, 30	w. s. w.	w. s. w.	s. s. e.	58	⅛	3.00 am	.238	.245	.321	100	65	86	10	2 cir.	10	9.30 pm	12 pm	2.30	.01	0
Thursday, 1	e.	w.	n. w.	184	18½	7.30 pm	.309	.362	.241	85	87	83	10	10	0	0 am	3 pm	15.00	.51	6
Friday, 2	n. w.	n. e.	e.	141	3½	3.00 am	.170	.142	.173	80	51	72	1 cir.	5 cir.	0	-----	-----	-----	--	2
Saturday, 3	e. n. e.	n. n. e.	n. n. e.	138	2½	10.40 am	.149	.203	.207	70	82	90	5 cir. cu.	10	10	8.30 pm	12 pm	3.30	.01	10

Distance traveled during the week .... 1,034 miles.
Maximum force .... 18½ lbs.

Total amount of water for the week .... .53 inch.
Duration of rain .... 21 hours, 00 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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SATURDAY, DECEMBER 24, 1881.

THE glad tidings of the safety of a large proportion of the crew of the *Jeannette*, and the probability that the missing members of the company will probably be soon heard from, will be a relief to many aching hearts, and welcome to the general public who have taken a great interest in this expedition.

As the success of the expedition has not been referred to in the despatches, the probability is great, that the discovery of the North Pole is still a problem to be solved, but the experience of Captain De Long will doubtless prove very valuable in making future plans for Arctic explorations, and we trust that no time will be lost in obtaining authentic details of the expedition.

From what we know respecting the voyage of the *Jeannette*, and from other information to date, our opinion is that the route by Smith's Sound, is the most practicable for all who attempt to reach the North Pole, and we still maintain that the plans of Commander Cheyne present a higher prospect for success, than any other scheme which has been announced.

The first step which we advise, is to establish firmly a small colony at St. Patrick's Bay, where coal exists in abundance, and ample protection can be found for stores and shelter for men. This spot is less than 500 miles from the North Pole, and, with such a base of operations firmly established, the coveted prize can surely be won by continued and persistent efforts.

We approve of Commander Cheyne's proposal to utilize balloons, on the ground that no facilities which can be devised by practical scientific men should be neglected, and it is far from impossible that some means of aerial navigation may be invented, which may be at least sufficient for this purpose.

The establishment of the colony at St. Patrick's Bay, should be the immediate plan which should claim attention, without desiring by a rush to accomplish the remaining distance. Time should be given for traversing the 500 miles which intervene before reaching the Pole, and all the devices which science can suggest should in turn be put to the test.

We cannot conclude these remarks without giving a due acknowledgement to Mr. James Gordon Bennett for his liberal outlay in the cause of geographical exploration. Inspired by his generous hand, Stanley braved the horrors of tropical climates and penetrated to the unknown recesses of Africa, and by Mr. Bennett's aid De Long has added new laurels to the American flag, and increased our knowledge of the Arctic regions.

THE presence of fossil organisms in meteorites alleged to have been discovered by Dr. Hahn, was fully explained in "SCIENCE" (No. 50, June 11, 1881) by Dr. Rachael. Since the appearance of this article I have discussed the subject with many specialists, with the result of finding a general distrust of Dr. Hahn's discovery.

I, therefore, endeavored to obtain a portion of the Knyahinya meteorite which fell in Hungary on the 9th of June, 1866, as many of the most convincing specimens were obtained by Dr. Hahn from it, and by the aid of Messrs Ward and Howell of the Natural History Museum, Rochester, N. Y., a small fragment of this meteorite recently reached my hands. From this specimen two sections were cut, and ground down to a condition of transparency by a gentleman skilled in such preparations, and are now mounted as microscopical objects.

An examination which I have since made confirms in every respect the correctness of Dr. Hahn's statement, as to what he saw, and it therefore remains only to decide whether the deductions he made were correct. The doubtful forms are very clearly defined and sufficiently large to be examined with precision by a 1-inch objective; one prominent object, which to the uninitiated might be taken for a diminutive clam shell, is found to measure 1-25 by 1-20 of an inch. I was disappointed to find that high powers failed to develop structure which indicated decisively the nature of these forms, and to show the difficulty of arriving at a correct solution, I may state that the two persons to whom I have so far shown the specimens, differed entirely as to their interpretation; the one pronounced them veritable fossils, and the second was equally sure that they were merely interesting forms of crystallization.

I reserve an opinion until the section has been studied with more attention, and comparison made with other specimens now being prepared; in the mean time I shall be pleased to show the section to any person who is interested in this subject, or able by previous study to throw any light on the subject.

JOHN MICHELS.

THE AMERICAN CHEMICAL SOCIETY.

The annual meeting of the American Chemical Society was held on Friday evening, December 2nd, with Vice President Leeds in the chair.

After the reports from the various officers were read, the society proceeded to the election of officers to serve during the coming year.

The results were as follows:

President: J. W. Mallett.

Vice Presidents: A. R. Leeds, W. M. Habirshaw, E. Waller, L. A. Goessman, A. B. Prescott, N. P. Lupton.

Treasurer: T. O'C. Sloane.

The remainder of the ticket, as announced in the previous notice, were all elected with the single exception of the treasurer, whose name was substituted by that of Dr. Sloane, whose name on the nominating committee was replaced by that of Mr. A. P. Hallock.

The board of directors will be as follows:

P. Casamajor, Jas. H. Stebbins, Geo. A. Prochazka, H. Endeman, H. Morton, P. de P. Ricketts, T. O'C. Sloane, A. R. Leeds, W. M. Habirshaw, E. Waller, C. F. Chandler, J. B. F. Hernshoff, W. E. Geyer.

The reading of the papers announced for the evening was postponed until the conversazione, which will take place on the evening of the 16th inst.

"J. W. Mallett," says Prof. Silliman, "has for many years been an industrious worker, publishing original researches in chemical subjects, which form important contributions to our science."

Among the very first to work in the then newly isolated element, Tellurium, was Prof. Mallett. Under the direction of the celebrated Woehler these researches were made, and, in recognition of their merit, the university at Gottingen conferred the doctorate on the youthful scientist. Coming to this country, for Prof. Mallett is an Englishman by birth, he located himself at Philadelphia with Mr. J. C. Booth who, at that time, had among his students and assistants T. H. Garrett, the two Morfits, McCulloh and others whose names have since become distinguished.

Later on, in the records of American chemistry, the subject of our sketch was appointed Professor of Chemistry at the University of Alabama, and at present he fills the same position at the University of Virginia; he also lectures in applied chemistry before the students at the Johns Hopkins University. His printed papers are very numerous, most of the earlier ones may be found in *Silliman's Journal*, while those of a more recent date have been published in the *American Chemical Journal*. To this latter periodical he has been a faithful contributor since its commencement, and its columns have been enriched by his very interesting review "Of the Progress of Science Among the Industrial Arts During the Last Ten Years." Prof. Mallett served as one of the judges in Group III at the Centennial Exhibition, and furnished for the governmental reports a very satisfactory resumé of the sugar industry of the United States.

He is a member of the Royal Society of Great Britain, of the Chemical Societies of London, Berlin and Paris, as well as many other learned bodies both at home and abroad. The American Chemical Society have made a wise selection, and it is to be hoped that its new president will resume that desirable custom of presidential addresses, which unfortunately has been omitted during the past few years.

M. B.

COMMANDER CHEYNE has started on his trip to Canada, and will return to New York about the 20th of January; in the interval Mr. Henry Walton Grinnell, who has consented to become Secretary of the committee to be formed to promote this expedition, will attend to matters requiring early attention.

THE NEW YORK ACADEMY OF SCIENCES.

Dec. 5. 1881.

REGULAR BUSINESS MEETING.

The President Dr. J. S. NEWBERRY, in the Chair.

Twenty six persons present.

Dr. NEWBERRY exhibited an ancient perforated stone axe from Europe, consisting of dioryte, and remarked that the aboriginal tribes of America never attained to the degree of skill required in the perforation of stone implements for the insertion of wooden handles.

The following paper was read by Dr. ALEXIS A. JULIEN.

THE VOLCANIC TUFFS OF CHALLIS, IDAHO, AND OTHER WESTERN LOCALITIES.

(*Abstract*).

In a paper recently read before the Academy it was shown that a certain compact white almost structureless rock, often porcellanous in texture, occuring abundantly in the Western Territories and variously styled "trachyte," "rhyolyte," "porphyry," etc., (*e. g.*, at Leadville, Colorado, in the Black Hills of Dakota, etc.), is a sedimentary form of a highly silicious volcanic tuff, probably derived from the finest detritus of trachytes, rhyolytes, and quartz-porphyries. A series of specimens collected by Prof. NEWBERRY, during the last and previous summers, and kindly put in the author's hands for lithological examination, has furnished the material for the following additional notes on this interesting but neglected group of wide-spread American rocks.

1. *Coarse pumice-tuff of Challis, Idaho.*

The rock is quite compact, chistose, of a gray color with dull white spots. The latter consists of pumice in finely fibrous grains, from 1 to 5 mm. in length. Quartz and feldspar are seen in small angular flakes, sometimes reaching 0.5 mm. in length: hornblende commonly in fibrous black fragments, about 1 mm. in diameter: and much biotite, brownish-green, sometimes brownish-black, with greasy lustre, in hexagonal scales, often up to 2 to 3 mm. in size.

The thin sections present under the microscope numerous grains, generally angular, of several minerals, varying in size up to 3 or 4 mm.: pumice in rounded to sub-angular fawn-colored fragments lying at all angles, commonly made up of straight or curved fibres, and often including glass lenses filled with crystallites: a trichinic feldspar, in clear grains, sometimes including minute globules of glass, and possessing fine lamellation, beautifully striated in polarized light, the remaining traces of crystalline outlines indicating that these grains are all of fragmentary, never of indigenous formation: quartz, in water-clear angular grains, 0.2 to 1. 6mm. long, retaining more frequent and perfect traces of their crystalline forms, their sides being often very ragged, curiously and deeply eroded into rounded indentations, while within occur numerous inclusions of the ground mass and of scales of biotite, long greenish needles of hornblende, and sub-angular drops of a brownish-violet glass with one or several fixed bubbles of gas: biotite in abundant irregular scales, 0.2 to 1.3 mm. long, brown inclining to maroon or brownish-yellow, cloudy to opaque, with some dichroism remaining in the striated sections; hornblende in brownish-green, strongly dichroic, fibrous crystalline flakes: opacite, probably magnetite, and ferrite or iron-oxide, in dusty particles or groups in the biotite scales and among the pumice fibres. The fine groundmass is mainly composed of minute fragments, fibres, scales, etc., of all these minerals: also in large part of solid globules of fawn-colored glass, or of thin and apparently hollow shells, or of fragments of quartz or feldspar coated with a glass crust. Many of

these forms are found adhering in curious aggregations or with their sides crushed in.

The general constitution of this rock is similar to that of the volcanic tuff of the El Dorado Cañon, Cal.

2. *Fine green volcanic tuff, of Challis, Idaho.*

A very fine compact rock, with almost the texture of stoneware, with a pale, greenish-gray color, and a very thin parallel lamination. A few minute scales of biotite can be distinguished by the loup. The surfaces of fissures are mottled and spotted with bluish-green and ochreous, brownish-gray films.

The thin sections present the same constitution as that of the coarse variety of the rock, without the presence of pumice, the particles of quartz and feldspar varying in size from 0.06 to 0.25 mm. Biotite is abundant in scales 0.1 to 0.2 mm. in diameter, often of ochreous shades of brownish-yellow and maroon, through partial decomposition, and with curved fibres or wrinkles as if crushed in by pressure. To its abundance are due the fine lamination of the rock and, in part, its greenish color. The ground mass largely consists of globules of colorless glass, but in less degree than in the preceding variety, their size varying from 0.006 to 0.01 mm.

3. *Fine white pumice-tuff, of Challis, Idaho.*

A very fine compact rock, grayish, with a bronze shade, with a lamination so decided that it inclines to slaty. Under the loup the same constituents are visible as in No. 1.

The thin sections show a close relationship to those of No. 2. A little hornblende is present. Biotite occurs in distinct scales, sometimes hexagonal, not so minutely dispersed as in No. 2, generally 0.01 to 0.1 mm. in diameter. The fragments of quartz and feldspar, as a rule, present their longer axes in the schist plane, varying from 0.03 to 0.22 mm. in length. The glass inclusions in the quartz, ranging from 0.002 to 0.037 mm. The ground mass appears to be mainly composed of pumice, more or less altered, in very minute fibres and particles.

This rock strongly resembles the tufa of the lignite beds near Osarisawa, Akita, Japan.

4. *Pumice-tuff, Moore Station, Pancake Range, Moray, Nevada.*

This rock is decidedly ochistose, cream colored, nearly white, of a fine grain, intermediate between Nos. 1 and 2, most of the constituents being the same as in No. 1 and less than 0.5 mm. in diameter, though occasional grains of pumice, gray and red obsidian, and perfect crystals of quartz may reach from 2 to 8 mm. in length.

In the thin section the constituents are found disposed with great regularity: pumice, with its fibres often curved, as if crushed while still soft and plastic: quartz: trichinic feldspar: possibly sanidine: magnetite: ferrite: biotite, salmon colored, sometimes very cloudy: and volcanic glass in cellular network, often full of gas bubbles, elongated and distorted. In the ground mass, globules of glass and fibres and threads of pumice largely predominate.

The pumice in all these tuffs is not perfectly isotrope between the crossed nicols, but presents innumerable, though exceedingly minute glittering points, apparently crystallites formed by incipient devitrification. A few minute sphercelites were also detected.

5. *Stratified Rhyolye-tuff, Tempiute, Nevada.*

A snow-white Kaolinic variety, related to the preceding, which appears to consist principally of pumice. A few grains of black obsidian and red quartzite occur, the latter also as a somewhat rounded pebble, 34 mm. in length.

The thin section, transverse to the schist-plane, presents an interesting structure, made up of granular layers alternating with others possessing strong fibration.

The material of the former is mostly like that of No. 4: feldspar is sparsely scattered: quartz fragments abound, with the usual glass inclusions, and with sides deeply eroded and indented: also magnetite, ferrite, and minute colorless particles of a polarising mineral, perhaps Augite, in a predominant groundmass of particles and fibres of pumice and glass, rich in dark gas-bubbles.

The alternating fibrous laminæ consist of a true rhyolyte material, salmon-brown, with a marked fluidal structure around the few quartz-grains, and displaying in spots, and especially next the junction, with granular material, the constituent pumice-fibres whose partial interfusion or cohesion seems ordinarily to have produced the solid laminæ.

The arrangement of the glass fibres in parallel planes may have been produced by sorting in the air during their fall, by later superincumbent pressure while still hot and plastic, or it may be in some instances by the influence of overflowing lava-sheets. The cohesion produced by such downward pressure and interfusion has produced a structure which can hardly be distinguished from that of many obsidians and rhyolytes.

6. *Fine white pumice-tuff, from mouth of Bill Williams fork of Colorado River, Arizona.*

A compact white schist, with almost the fine texture of No. 3, traversed in places by brown curved impressions, apparently produced by rootlets.

The thin section mainly exhibits a very finely felted mass of short, straight fibres of pale brownish pumice. Besides these only a very few black particles of magnetite, feldspar, etc., were distinguished.

7. *Fine brownish pumice-tuff, from last locality.*

A brownish variety of the preceding, with abundant minute black particles. The slaty lamination is decidedly marked, with slight adherence over many planes at which the rock breaks easily, presenting remarkably flat surfaces.

The constitution displayed in the thin section is similar to that of the preceding specimen. Minute glass globules are abundant, and also more numerous angular particles of other minerals: colorless feldspar (sanidine?) showing cleavage: brownish and greenish augite: brownish and dichroic fibres of hornblende, and black particles of magnetite.

8. *Stratified pumice-tuff, from Black Mountains, Colorado river, Arizona.*

A coarser stratified tuff with brown and white layers, in which grains of pumice, obsidian, glassy feldspar, and quartz reach a diameter of 1 to 5 mm.

The thin section is rich in pumice in all its fibrous, curving, and reticulated forms, and in minute globules, threads, and shreds of volcanic glass: angular grains of finely lamellated plagioclase, water-clear quartz, and sanidine with well marked cleavage and often zonal stricture: particles of biotite, hornblende, magnetite and ferrite: abundant grains of augite, angular to rounded, sometimes retaining its optical characteristics in spots, but mostly decomposed and isotrope, colorless, brownish-yellow, light to deep maroon, etc., finely granular, thready, or fibrous, and more or less darkened by opacite even to complete opacity.

9. *Basalt-tuff, or peperino, Chenniti Mts., Texas.*

A fine-grained olive-green rock, with white streak, friable to arenaceous, with barely perceptible schist structure in the specimen. Under the loup, minute granules of feldspar, quartz, etc., are distinguishable, rarely 1 mm. in diameter, embedded in a grayish-green cement.

In the thin section the constituents are very much the same as in No. 8, with the exception of hornblende, and all the grains are in large part rounded. A few elongated rounded grains of a basaltic lava are also included, highly micro-crystalline with minute ledge of plagioclase scattered through a reddish-brown opaque base.

This specimen, and perhaps the preceding, represent the basic division of the tuffs, being ejections from an eruption of basaltic lava, though naturally composed of its more fluid, glassy, and acid scoria.

From these facts it may be concluded that enormous

masses of volcanic tuffs of widely varying character are dispersed throughout these regions in the West to an extent which could hardly be appreciated from the meagre references in our present petrographical literature.

In his discussion of the rhyolytes of the fortieth parallel, Zirkel remarks :*

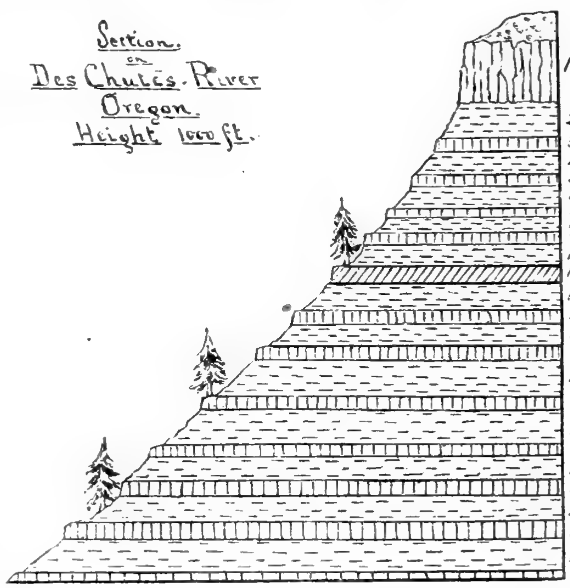
"The foregoing descriptions show in what abundance those fibrous bodies in which the fibres are not grouped radially around a centre, as in sphœrolites, but arranged axially along a longitudinal line, are disseminated through these rhyolites.... These axiolites usually consist of distinct, uniformly thin fibres, or of wedge-like particles.... We see in the arrangement of the fibres in these rhyolites four different types: *a*, centrally radial: *b*, longitudinally axial: *c*, parallel: *d*, confused and orderless. The development of fibres is, indeed, a phenomenon very characteristic of rhyolites, etc., etc.'

A comparison of these facts with those presented in my examination of these tuffs appear to me significant, not of the development of fibration, etc., in a fused mass, but of the fragmental origin of at least many rhyolytes, obsidians, etc., as suggested in the study of No. 5. The evidences of the hot and plastic condition of the fibres and drops of volcanic glass, with the occasional exception of a cooled outer shell, for a long time after their fall, and of a tendency to the growth of microliths, sphœrolites, etc., within them, may offer another mode of origin for the formation of axiolites and sphœrolites. The anomalous presence of augite in a quartzose rock like rhyolyte, to which Zirkel calls attention in the same passage, may also find explanation in the varied intermixture of minerals which prevails in many tufts, rather than to indigenous development within an acid lava.

Dr. NEWBERRY said that he had no doubt that Mr. Julien was quite correct in regard to the genesis of the peculiar rocks which he had described. He had collected the specimens and was able to supply some facts in regard to their mode of occurrence. They belong to a series of rocks, plainly volcanic, but of which the history has not been given by those who have studied the volcanic rocks of the West. The circumstances of their occurrence are briefly these: over a great belt not less than one thousand miles wide in some places, viz., from the crest of the Sierra Nevada to the eastern foothills of the Rocky Mts., and with a north and south extension of thousands of miles in British Columbia, the United States and Mexico, we have an extraordinary display of the products of volcanic action. This is the great silver belt of the world, and is also rich in mines of gold, copper, lead, etc. Throughout all the Paleozoic and Mesozoic ages this country was an unbroken though not entirely unwarped sub-marine or sub-aerial plateau, where the most continuous and extensive series of sedimentary rocks was deposited of which we have any knowledge. At the close of the Jurassic age the western portion of this region was folded up, to form the great chain of the Sierra Nevada and Cascade Mts., and along this line of fracture numerous volcanic vents were established, Lassen's Butte, Mt. Shasta, Mt. Hood, Mt. Baker, etc., which have continued in intermittent activity to the present day. In Tertiary times the plateau east of the Sierra Nevada was broken up by a series of north and south fractures resulting in the formation of the remarkable system of meridional mountain ranges which constitute the chief topographical features of the district. These mountain ranges are composed of blocks of Paleozoic limestones and sandstones --now converted into marbles and quartzites—set up on edge or at a high angle, or of volcanic materials which have welled up through some of the fissures. Along the lines of fractures are great numbers of hot springs, the representatives of thousands more which existed in former days, and to which we owe the great system of fissure veins of this country:—hot water charged with mineral matter gradually depositing this and filling the channels through which it flowed.

The volcanic rocks which have been poured out in so many places exhibit a great variety of physical and chemical characters, but have been grouped by RICHTHOFEN and ZIRKEL into five species—propylite, rhyolite, trachyte, andesite and basalt. Capt. DUTTON who has given great attention to the volcanic rocks of the West, has distinguished a larger number of kinds and has adopted a different classification. Aside from these massive rocks there is another group which constitutes a marked feature both in the topography and geology, and these are those which have been made the subject of Mr. JULIEN'S paper. They are generally soft in composition, often highly colored,—white, red, blue, green, gray or yellow—more commonly white, red or gray. They are often quite local and usually occupy the lowlands, frequently underlying much of the level surface between the mountain ranges; and their best exposures are seen in the banks of streams which have cut these lowlands. There they are shown to be often horizontally bedded and sometimes interstratified with lacustrine sediments and sheets of basalt. Typical exposures of these rocks may be seen at Eureka, Nevada, where houses and cellars are excavated in the soft material which forms the sides of the valley; at Challis, in the banks of Salmon River and Garden Creek, whence the specimens described by Mr. JULIEN came, and in the cañons of the Des Chutes and its tributaries in Oregon.

Economically these rocks have considerable inportance as they are extensively used in place of fire brick for lining lead smelting furnaces, being very refractory, and easily dressed into shape with an old axe.



The above section represents the filling of some of the fresh water lakes which formerly existed in Oregon just east of the great volcanic cones of the Cascade Mountains. Numbers 1 and 11 represent sheets of basalt, the even number softer tuffs and bed of diatomaceous earth, the odd numbers consolidated conglomerates of volcanic materials called "concrete" in my notes.

The study of a large number of outcrops of this series of rocks from Southern Arizona to the Columbia River has convinced me that they are generally volcanic ashes which have been washed down and more or less perfectly stratified in bodies of water which formerly occupied the intervals between the mountain ranges of the great basin. On the Des Chutes a section of more than 1000 feet shows 25 alternations of strata, many of which are examples of the rocks in question. Here they are interstratified with beds of tripoli, composed of fresh water diatoms, and layers of basalt. Some of the ash beds are almost entirely composed of lapillae of soft cottony pumice, others are finer, grey, red, white, etc., and contain the trunks of coniferous trees, and in some instances

* U. S. Geol. Expl. 40th Par., VI, Microsc. Petrog., pp. 201-205.

are pierced with holes which represent the stems of upright plants, thickets of which were buried by the descending showers or rapidly accumulating sediment of volcanic ash. Here the source of the materials is to be sought in the line of great volcanic vents which crown the summit of the Cascade Mountains, and from which, at intervals, were emitted either floods of lava, poured down on to the plain along the eastern border of the range, or showers of ashes which, borne inland by the prevailing westerly winds, fell on forest, savannah and lake, temporarily destroying animal and vegetable life and forming, when falling or washed into water basins, strata which alternate with fossil beds, the accumulations of quieter times. In other places these tufaceous deposits were washed from all the highlands into the valleys, forming local masses of considerable thickness without the intercalated beds mentioned above.

The accompanying section, copied from my report on the Geology of Northern California and Oregon (Pacific R. R. Report, Vol VI, Geology, p. 47), will illustrate the deposition of these tufaceous rocks in the lake basins where they are interstratified with the fossiliferous beds.

THE SCIENTIFIC SOCIETIES OF WASHINGTON, D. C.

THE PHILOSOPHICAL SOCIETY.—During the month of November three very important papers were read: on the Anomalies of Sound Signals, by President James C. Welling; on the Storage of Electric Energy, by Mr. J. C. Koyl; and on Barometric Hypsometry, by Mr. G. K. Gilbert.

The first named paper was a comprehensive review of the vexed discussion concerning the anomalies observed in the transmission of sound, and the summation of the result in a series of twelve aphorisms. The second paper was by a fellow of Johns Hopkins College, with reference to a series of experiments lately made by him in company with some Washington gentlemen upon an invention for the storage of electricity. Mr. Gilbert's communication had reference to a scheme of measuring altitudes by means of two barometric stations in the same vicinity, the one quite elevated, the other as low as convenient. By this means the influence of the thousand and one local causes affecting the barometer would be more thoroughly brought under the knowledge of the observer.

THE BIOLOGICAL SOCIETY.—The following communications have been made during the past month: on the Philosophy of the Retardation of Development Among the Lower Animals, by Prof. C. V. Riley; Antiquity of Certain Types of North American Non-Marine Mollusca, and the Extinction of Others, by Dr. C. A. White; Recent Explorations of the U. S. Fish Commission, by Mr. Richard Rathbun.

Professor Riley drew the attention of the society to a number of instances where the development of insects had been retarded in the embryo stage for a very long time. This did not refer to the well known retardations of whole broods, but to wholly exceptional cases. The speaker attributed the phenomena to evolutionary causes, and showed how a species might be saved from the wholesale destruction of a very severe winter or other disaster by this means.

Professor White's paper had reference to the survival from very high antiquity of many of the fresh water and brackish water forms, and to the total disappearance of others, for which events no adequate causes can be assigned.

Mr. Rathbun's communication was a review of the work of the Fish Commission from its foundation, illustrated by a map locating every dredging station; by a papier maché model of the Atlantic bottom as far out as the deep soundings, from the mouth of the St. Lawrence southward, and by specimens of the apparatus employed as well as the fauna discovered. The address was necessarily very comprehensive, but exceedingly interesting. At the same time the attention of the society was called to a pamphlet by Prof. G. Brown Goode, entitled "The First Decade of the United States Fish Commission, its Plan of Work and Accomplished Results, Scientific and Economical, Salem, Mass.: Salem Press, 1881."

THE ANTHROPOLOGICAL SOCIETY.—Three papers were also read before this society in November, to wit: How Shall the Deaf be Educated? by President E. M. Gallaudet; a Navajo Myth, by Mr. R. L. Packard; the Regulative System of the Zunis, by Prof. J. Howard Gore. The education of the deaf must be preceded by a proper classification of the heterogeneous group commonly called deaf mutes. The question of the relative superiority of the sign language and of visible speech was discussed with great minuteness. The author also treated the problem of heredity, of relative intelligence, and of the power of abstraction, with great ability.

Mr. Packard's myth was one taken by him last summer from one of the Navajo tribe and related to the origin of the Navajos.

Mr. Gore has spent some years upon the evolution of deliberative assemblies and the conduct of such bodies. Last summer, being in charge of a surveying party in New Mexico for the government, he availed himself of his opportunities to become familiar with the customs of the Pueblo Indians in such matters. These papers will be published in the proceedings of the Society.

DIAGRAMMATIC REPRESENTATION OF STEREOSCOPIC PHENOMENA.

(Continued from p. 548, Nov. 19th, 1881, in Transactions of N. Y. Academy of Sciences).

In a previous article (8) it has been shown that no reliance can be placed upon the theory of apparent distance in the stereoscope, elaborated by Wheatstone and Brewster, and applied in the diagrammatic explanation of stereoscopic phenomena in all our text books on Physics. We may well ask, therefore, to what extent it is possible, by any diagram, to represent the position of objects as they should appear in the stereoscopic field of view. So far as this is determined by the relation between the visual lines we may secure an approximation only by the following method, in which it must be assumed that we know also the relation between the camera axes at the time the photograph was taken. Since the visual lines may be practically regarded as special secondary axes to the crystalline lenses, it will be found convenient to call them visual axes, and their possible relations, axial convergence, parallelism, and divergence. It may be well also to restate two principles that have been sufficiently demonstrated elsewhere.

I. A point farther or nearer than the point of sight is necessarily seen double (9) and with imperfect focalization. If farther, the internal rectus muscles of the eyeballs must be slightly relaxed to make it the point of sight; if nearer, they must be contracted. Such relaxation is habitually associated with remoteness, such contraction with nearness, of the point fixed.

II. If an external point is imaged upon corresponding retinal points, the subjective effect is that of union of the two eyes into a central binocular eye, the nodal point of which is the point of origin in all estimates of direction and distance. (10)

A brief preliminary proof of a geometrical principle to be applied is also necessary. Let C and C′, fig. 1, be two fixed points, and E midpoint between them on a horizontal plane. Let this plane be cut by four vertical planes, parallel to each other, their traces being marked I, II, III, and IV. Let B and Q be any points of plane I, from which straight lines are drawn to E, piercing plane II at A and P respectively. Through C and C′ let pro-

jecting lines be drawn from A, B, P and Q to plane IV. It is required to find what relation exists between the horizontal displacements of the projections, a from b, p from q, a′ from b′, p′ from q′.

Since CC′ is horizontal, the projected horizontal displacements will be the same for all elevations of B and Q; these may hence be taken as points on the horizontal trace of plane I.

Since EC=EC′, we have

$Pq_2 = Pq'_2 = Ab_2 = Ab'_2$

$\therefore\ pq = p'q' = ab = a'b'$.

This equality will not exist if E be not midpoint between C and C′. It is true for all points in planes I and II respectively, through which straight lines can be drawn to E.

Let now C C′, fig. 2, be the distance between two camera lenses, for example 10cm; A and B, objects in the foreground and background of a landscape, on the median line from E; Aa, Aa′, Bb, Bb′ secondary axes extending to the sensitized plate. The stereoscopic displacements on the photographic negative, a b and a′ b′, are equal to each other and to those of any other points, P and Q, related to the midpoint E, as they are in Fig. 1.

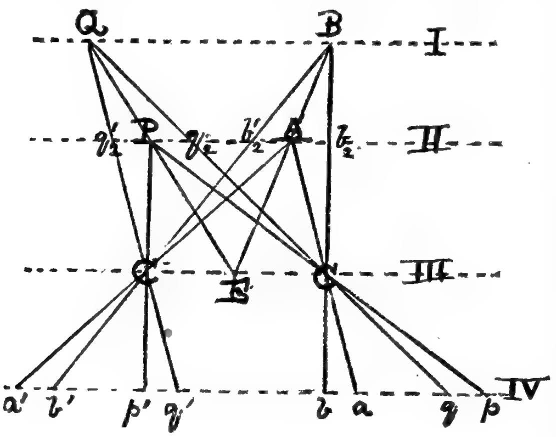


Fig 1.

The camera axes to A make a known angle, θ, with each other. Let E be midpoint also between a pair of eyes, R and L. If proofs from the negative be inverted and placed in front of R and L in such manner that the visual axes through a_1 and a'_1 shall form with each other an angle, α, equal to θ, then A_1 and B_1 are, as nearly as possible, the apparent positions of foreground and background respectively. If α exceed θ, A_1 will be nearer, if α be less than θ, A_1 will be farther; and this will be true if $\alpha = 0$, or $\alpha < 0$. Either of these last conditions is attained by simply increasing the distance $a_1a'_1$. Whether the visual axes are convergent, parallel, or divergent, the *subjective* effect is that of the union of R and L into a binocular eye which, for the geometrical reasons just given, can be nowhere else than at the midpoint, E. In this the dissimilar retinal images are superposed. If those of the points a_1 and a'_1 coincide, b_1 appears to the right eye projected outward on the right, to the left eye an equal distance outward on the left; to the combined eye it is a homonymous double image. Let A_2 be the external projection of the combined images of a_1 and a'_1, as seen by the binocular eye at E_2; if $\alpha = \theta$, the distance E_2A_2 is equal to RA_1 or LA_1, which differs but little from EA_1 unless α be large; it has been drawn equal to EA_1. Then B_2 and B'_2 will be the external projections of the uncombined images of b_2 and b'_2. If the attention be transferred to the background, the angle between the visual axes must be diminished by relaxing the internal rectus muscles, and this instantly suggests greater remoteness of the point of sight. The retinal images of b_2 and b'_2 coalesce and are projected to the more distant external point between B_2 and B'_2, while those of a_2 and a'_2 cross slightly to opposite sides and are projected as a heteronymous double image at its proper distance. The ratio of E_2A_2 to EA_1 must depend upon the variation in muscular tension due to the difference between the angles θ and α. The duplication of the images of the background points when the foreground is regarded, and *vice versa*, is easily perceived if a properly constructed diagram is viewed in the stereoscope, provided the observer be attentive to his own sensations, examining each point of the combined picture separately instead of regarding

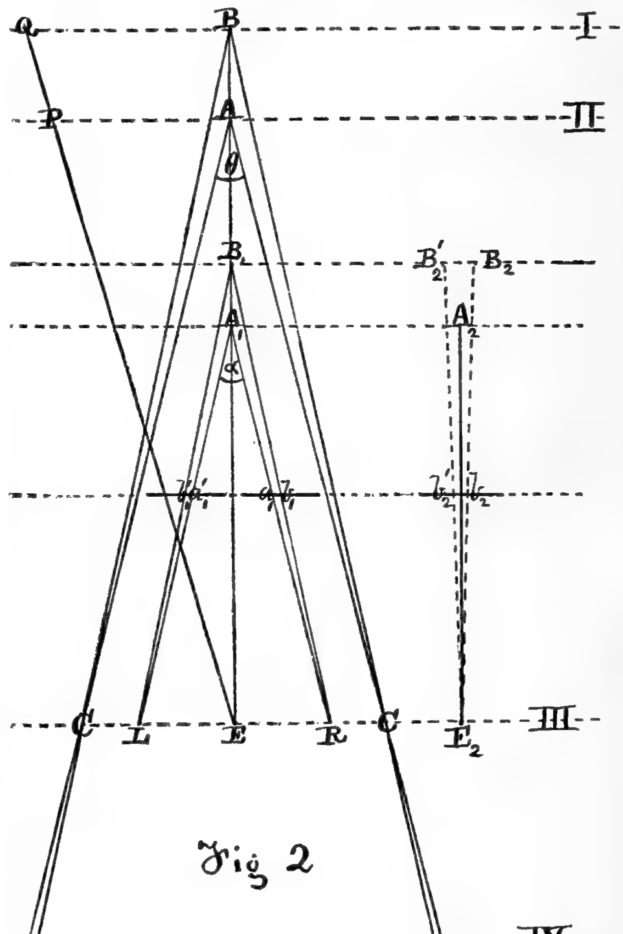


Fig 2

the total effect at once. Let Figures 3 and 4 be examined in the stereoscope, by resting the edge of the page on the cross-bar of the instrument at the proper distance in front of the semi-lenses. In each case, when the foreground circle is seen single, the background dot is seen double, and *vice versa*. When the background circles of one figure are combined binocularly, those of the other are seen separately by monocular vision. Axial convergence is necessary to combine the circles of Fig. 4. The combined image appears nearer, smaller, and less tall in proportion to its base than the combined image of Fig. 3, which requires axial divergence. The stereoscopic displacement is the same in both figures, and is measured by the distance between the centre of the large circle and that of the small one within it. The stereographic interval for the background is 90 mm. in

Fig. 3, and 50 mm. in Fig. 4. An average lenticular stereoscope will fail to produce axial convergence when Fig. 3 is viewed by a pair of eyes whose centres are 64 mm. apart. These variations in visual effect can be rendered still more striking by using the reflecting stereoscope, after cutting apart the right and left halves of the stereograph.

It must be remarked that although the perception of binocular relief is intensified by alternate examination of foreground, and background it is quite possible to attain it by momentary illumination with the electric spark, at least with convergence of visual axes, as has been done by Dove [11] and others. During such brief illumination, no variation of convergence is possible, and, if the foreground be distinctly focalized, the background must be slightly doubled homonymously. The position of the point of sight is found thus to be almost as nearly determinate as when the illumination is prolonged. Whether binocular relief, and the position of the binocular image, be perceptible with equal distinctness when the visual lines diverge, has not thus far been ascertained by experiment, so far as I am aware. I hope to test this, and to study certain other points of interest connected with it, at some future time.

Since the apparent distance of the point of sight continues to increase in a positive direction after axial parallelism passes into divergence, it becomes necessary to investigate the physiological conditions that interfere with the results of the mathematical theory hitherto generally applied.

In looking at any picture constructed in accordance with the rules of perspective applied by all artists, the illusion of distance is quickly attained by forming a conception of the reality in space to which the different parts of the picture are supposed to correspond, or of the object as the observer has been accustomed to view things of its kind. With the same degree of axial convergence a mountain and a piece of statuary will never be judged equally distant. It is unnecessary here to enumerate the elements that combine to produce the illusion. If these be excluded to the utmost, as in mere skeleton diagrams, there will still be left three to consider in judging the distance, size, and form of what is represented. These are—

I. The optic angle between the observer's visual axes.
II. The focal adjustment of the crystalline lens.
III. The visual angle subtended by the picture.

Of these the first is the only one usually considered as *distinctive* of binocular vision. It can never be dissociated entirely from the others, and its effect may be so overpowered by them, when distance is to be estimated, that calculations based upon its value, like those of Brewster, lose all claim even as approximations to the truth. Its true importance is dependent upon the extent to which the individual, in natural binocular vision, has been accustomed to associate the sensation of muscular contraction in the rectus muscles of the eyeballs with the true distance of objects as learned by other means. Doubtless this varies with different individuals. For distances of more than 240 m. the optic angle becomes inappreciable, and hence theoretically valueless; its importance is greatest near the lower limit of distinct vision. In every case its effect is appreciated mainly through the muscular sense and through the retinal perception of double images from objects farther or nearer than the point upon which attention is fixed. We are safe in disregarding the mere fact that a pair of imaginary lines *would* make a measurable angle with each other *if constructed*, though the use of this angle may be convenient in analyzing the phenomena of vision. It is well to remember, however, that its variations imply simply changes of muscular tension, and these constitute the most appreciable effects that influence the estimate of limited distance.

The judgment due to focal adjustment is also an interpretation, based upon personal experience, and suggested by the sense of contraction in the ciliary muscle, while adapting the crystalline lens to produce a distinct image. Variations in this are hence inappreciable for distances of more than 6 m., and are most noticeable near the lower limit of distinct vision. It is near this limit that the stereograph is held in most cases when regarded.

The visual angle is important as chiefly determining the size of the binocular retinal image. Since two eyes receive more of the light reflected from a given surface than either eye alone, the binocular image appears brighter than one that is monocular; and this is apt to produce the illusion of slight decrease of distance, if the focalization is perfect. But variation due to this cause is not important in comparison with that due to change in the visual angle.

The relative importance attached to the separate elements enumerated depends most frequently upon the unconscious experience of the individual. The results which they combine to produce cannot be referred to any one mathematical formula until the physiology of sensation is completely brought within the domain of mathematical law.

In testing the effects of these elements it will be best to apply a formula for the distance of the optic vertex from each eye, in terms of the interocular distance, i, and optic angle, α. Assuming the optic triangle to be isosceles, and calling D the required distance, we have, as the formula to be tested by experiment,

$$D = \tfrac{1}{2}\, i \ \text{cosec.}\ \tfrac{1}{2}\alpha.$$

Considering angles of convergence positive, the possible values of α, between which I find myself limited, are $+ 80°$

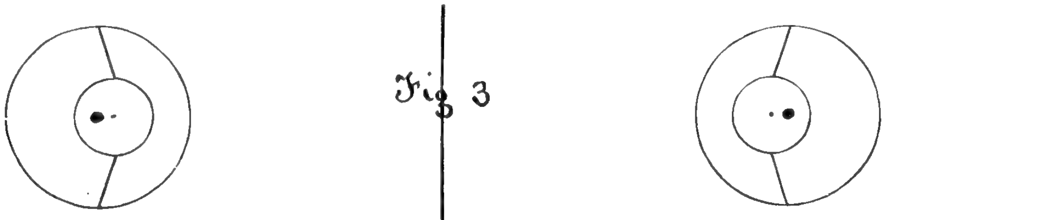


Fig 3

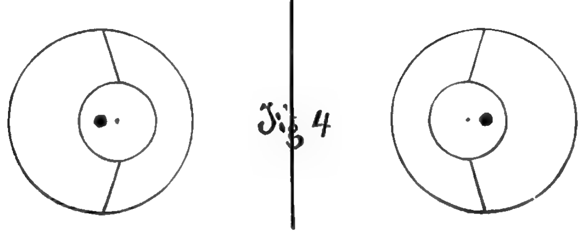


Fig 4

and $-7\frac{1}{2}°$, my eyes being perfectly healthy. If a curve be constructed from the formula, the values of a being taken as abscissas and those of D as ordinates, for parallelism of visual lines we have $a = 0$, $D = \infty$, and the axis of ordinates is hence an asymptote. A vanishing point should therefore be reached by the external binocular image. Its apparent distance, however, is still finite, and vision very easy. In passing to negative values of a by increasing the stereographic interval, the distance estimated continues to grow in a positive direction. This is undoubtedly due to the sense of increasing relaxation of the internal rectus muscles and contraction of the external rectus; but the rate of growth bears no recognizable relation to any succession of values given by the formula.

The explanation just given is based upon experiments, the description and discussion of which must be withheld for the present. It may be simply stated that with binocular fusion of images from the same pair of conjugate pictures, I have tested the visual effect of varying the optic angle from $-5°$ to $+45°$, vision becoming indistinct after the last named limit is passed. The value of the optic angle has been found to be largely, but by no means exclusively, effective in determining apparent distance in the stereoscope, especially for convergence of visual axes. Its effect is antagonized by the difficulty of focal adjustment and by the constancy of the visual angle, the latter element being particularly important when the axes diverge.

The variation in apparent magnitude of the combined image, dependent upon the value of the optic angle, has been noticed by Wheatstone ([12]), Helmholtz ([13]), and Meyer ([14]). Helmholtz constructed his telestereoscope ([15]) for producing exaggeration of perspective when distant objects are viewed, but no reference is made, in this connection, to divergence of visual lines. The possibility of fusion by optic divergence seems to have been first noticed about 1860, by Burckhardt ([16]); and Helmholtz notices the exaggeration of apparent distance thus produced, but explains it by saying ([13]), "Infinity does not, in our visual conceptions, present an impassable limit. When our eyes occupy a position which is never presented in the normal observation of real objects, all that we can do, conforming ourselves to the rule which we ordinarily follow for the interpretation of abnormal sensations, is to compare the sensation produced with that which resembles it most, and which is distinguished from it only by more feeble convergence, that is, with what is given us by real objects very remote." Vision in the stereoscope is always to some extent abnormal. The error into which Brewster fell, and in which he has been generally followed, was in supposing that under such conditions no modifications would be imposed upon the mathematical law found applicable to normal vision, in which there is perfect coincidence between the impressions traceable to the optic angle, focal adjustment, and visual angle respectively.

W. LE CONTE STEVENS.

THE German government is considering the participation of German men of science in the plan of International Polar Research. The Reichstag has been asked to grant the necessary funds $75,000.

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(16). Verhandl. d. Naturforsch. Ges. zu Basel. I, 145; or, Opt. Phys., p. 827.

FOREIGN NOTES ON THE SOURCE OF COMETS' LIGHT.

Numerous observations have been made abroad upon comet *b*, 1881, to settle the question as to the origin of the light of these bodies. Messrs. Thury and Mayer at Geneva compared the brightness of the comet's head, as ascertained by photometric measurements, with the brightness it would have had if its light had been derived solely from the sun, by reflection. It was found that the intensity of the light of the nucleus, as it withdrew from the sun, diminished at first faster, and after a time slower than would have been the case, had it shone solely by reflected light. The decrease in intensity took place, in fact, as if the nucleus, during its approach to perihelion, had acquired through the force of the sun's rays an intrinsic light, which accompanied violent action of some character; this violent action ceasing after the comet had measured some distance on its return track, its light decreased speedily in conformity therewith, but the nucleus continued to glow as if in a state of incandescence, and remained visible, according to the above observers, longer than could have been expected.

This method seems to be well adapted to an independent determination of this interesting question. In the data it is a question, not of absolute, but of relative quantities. Ignorance of the physical condition and nature of a comet's reflecting surface renders it impossible to compute the intensity of its light under reflection alone, with any degree of certainty. As not the absolute light, however, but the increase or decrease under the circumstances, is required, the necessity for such knowledge is eliminated.

Another conclusion as to the origin of the comet's light has been reached by Respighi, from spectroscopic evidence. According to him there is no doubt that the light in part is reflected, as is proved by the appearance of the Fraunhofer lines in the photographs of the comet's spectrum. As to the bright lines or bands, they also may be caused by reflected light, as will be seen when it is taken into consideration what changes this light must have undergone after it has passed through the gases and vapors which form the whole mass of the comet. "It is certain," he continues, "that the largest part of the light emitted by the comet comes from its interior, and that it has passed through thick strata of gases and vapors. It is there subject to the selective absorption which is peculiar to these vapors and their combinations. It is accordingly natural that dark lines and bands should thence arise, which are different from the Fraunhofer lines; and with the weak, but complete, spectrum of the light that is reflected from the exterior substance of the comet, another spectrum must appear, which is considerably modified through powerful absorption."

"The limits of a simple notice do not permit me to enter in detail into my numerous spectroscopic observations of the Comet *b*, 1881. But I can affirm that it does not require the supposition of an intrinsic light to explain the phenomena which they exhibit. For the discontinuity of the spectrum might arise from the same cause as the broad, dark bands in the spectrum of the sun near the horizon, or in those of the planets. In the case of comets, however, the phenomenon is greatly exaggerated by the immense thickness of the absorbing strata, the rich character of their chemical constitution and the weakness of the light which they reflect to us. One must therefore proceed as in the case of the spectrum of our atmosphere, and not consider so much the bright bands as those dark through absorption."

A COMMITTEE has been formed at Reggio, (Emilia) to collect funds for establishing a fitting monument to the memory of the Padre Secchi, in the form of a fine refractor, of which the objective is to have 70 centimetres diameter. Reggio thus follows the example of Arcetri, where a fitting scientific monument has been erected to the memory of Galileo.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

LOUDNESS *vs.* INTENSITY OF SOUND.

To the Editor of "SCIENCE."

Will it seem like firing a blank cartridge at Copenhagen to urge that writers on acoustics ought more carefully to distinguish between the words loudness and intensity as applied to sound? We think not, so long as statements like the following are found in elementary manuals of physics; or so long as the language of even distinguished lecturers on Sound is not wholly free from similar indefinite expressions.

For instance, the law of variation in intensity of a sound free to move in a homogeneous medium is often stated in substance thus: the intensity *or loudness* of sound decreases as the square of the distance. As an illustration it is sometimes added, a sound at the distance two will be only one fourth as loud as at the distance one. While as a triumphant proof or verification of this law, it is often said: a single bell at the distance of ten yards will sound as loud as four similar bells at twenty yards.

It is well known that the word sound and several of the terms used in describing sound have two meanings. The word loudness primarily refers to the sensation of hearing. In order to avoid confusion of thought, I esteem it important that the use of this word be restricted to the sensation, and that the word intensity (or volume) shall refer solely to the external vibrations which are the cause of the sensation. In other words, loudness ought always to be used in a subjective, intensity in an objective, sense.

What is meant by such expressions as those above quoted? Perhaps they are simply examples of a loose use of language, but it will certainly be natural for the unwary reader to infer from them that loudness and intensity vary according to the same laws, and also that we can by the ear verify those laws. I have no hesitation in affirming that this use either purposely or otherwise, of the word loudness as synonymous with the word intensity, has been the cause of great confusion of thought, and has often loaded down the undulatory theory of sound with that which is really foreign to it. The time has come when we ought to regard that treatise on sound as a failure in one important respect which does not leave the reader thoroughly imbued with the idea that the law of variation in the intensity of a sound refers to sound vibrations and not to the intensity of the sensation of hearing.

But if those who use such expressions as have been quoted, really mean to claim that loudness, i. e., relative intensity of sensation, varies according to the same laws as the energy of the moving molecules of the sound wave, or if it is claimed that by the ear we can accurately and validly verify the law, then it will be in order to demand the proof.

In order that such physiological laws may be proved true it must be shown, either, 1, that we can accurately know when one of our sensations is a multiple of another (as when one sound is four times as loud to the ear as another), or, 2, that we can recognize sensations of equal intensity; and, 3, it must also be proved that the intensity of the sensation is proportional to the energy of the blow causing the sensation. These assumptions cannot be proved.

1. It goes without saying that any one having normal senses can tell a heavy blow from a light one, and can recognize degrees of intensity among sounds, lights, heats, tastes, and smells. But if it is claimed that there are quantitative relations between sensations of different intensity, and that we can by consciousness recognize these ratios, we at once become committed to a remarkable system of mathematics. Since experience shows that the senses are easily deceived and that different persons disagree as to their estimates, who shall decide what are the true ratios? But we can only compare the relative intensities of two sensations by memory. Here is a fruitful source of uncertainty, for before we can be sure that one sound is to our ears four times as loud as another, we must be certain that we can by memory reproduce the first sensation and place it beside the second in exactly its true intensity. No one can be sure of this. This reasoning applies to those who have perfect senses, if there are such. When we consider the myriad degrees of nerve sensitiveness, partly congenital, partly the result of habit, and partly the result of disease, the problem becomes still more difficult, ludicrously so.

2. Can we recognize sensations of equal intensity? No doubt we can do so much more exactly than we can estimate the relation between sensations varying greatly in intensity. Yet here we meet the same cause of doubt as before,—the uncertainty of memory. The less is the time intervening between two distinct and independent sensations, the more nearly we can estimate their true intensities. In comparing sounds, somewhat more than one sixteenth of a second must elapse between them. In the photometer the lights or shadows are shown in contrast and are thrown side by side upon the screen, where we can see them simultaneously or pass from one to the other very quickly. Probably there is no way whereby we can compare two sensations more accurately than by the photometer, yet no one will claim that he can move the lights so that their intensities shall be exactly equal on the screen. All he can say is: to the eye they are equal. If then under the most favorable conditions, there is a residuum of doubt, the sense of hearing will be still more untrustworthy; I regard it, therefore, as a fallacious method of research to bring physical laws to be tested by the uncertainties of sensation. Can feeling demonstrate the accuracy of a thermometer, or can the laws of energy be verified by striking ourselves blows with moving bodies? All that we can say is that within certain limits the testimony of our senses approximately conforms to the laws which have been deduced from more accurate observations and reasoning.

3. Are sensations proportional to the energy of the impacts producing them? They must be, if loudness and intensity vary according to the same laws, or if equal sensations are caused by equal blows. The hypothesis is manifestly absurd as a general law, for we are unconscious of very weak blows, and very violent ones either destroy the nerves or paralyze them by what is known as shock. Even within the most favorable limits the rule can only be approximately true, and if it were true, could not be proved, for the nerves retain their impressions for a variable length of time, and this marks a limit to the intervals at which we can repeat impressions of normal intensity free from the residual effect of previous impressions. Hence if impressions be repeated too soon they will generally cause a progressive deadening of the nerve sensitiveness, or sometimes an increased sensitiveness, as in the case of the punishment of the bastinado. Even if there were nervous conditions such that the sensation was proportional to the energy of the impact, it would be difficult if not impossible to prove that the nerves were in the proper condition at any given time. Into such a tangled maze of uncertainties are we led when we try to pervert our senses, admirable in their proper sphere, into mechanisms for the quantitative estimation of energy!

If it be said that a single bell at the distance one will sound as loud as four bells at the distance two, it must be assumed that the ear is equally well adapted for receiving and transmitting all sounds, irrespective of the shape of their wave front. I will omit from the following discussion all the complications which spring from differences in the pitch and timbre of sounds and will premise a perfect ear and nerves.

According to the undulatory theory of sound, the wave

front in a homogeneous medium is a spherical surface, and the rays of sound proceed outwards in all directions and in straight lines ; hence the nearer is the source of sound, the more convex is the wave front and the more diverging are the rays. When the nearly parallel rays of sound proceeding from a distant point, strike the cup-shaped outer ear, a part is reflected toward the centre and thus reinforce the rays which directly enter the external opening of the ear. If a sound proceed from a point very near the ear the rays will be so diverging that all, except such as directly enter the opening, will be reflected outwards and will be lost. Hence it is evident that a far sound will seem louder than a near one, if their vibrations are of equal intensity as they come to the outer ear. This will at once upset the theory that loudness and intensity vary according to the same laws, unless in some way the far sound shall lose its advantage after entering the external meatus; but, as they enter the tube, the diverging rays of the near sound will strike obliquely outwards against the walls and will be reflected. Thus a part of their energy will be lost, a much larger proportionate loss than will come to the more parallel rays of the far sound. When at length after various reflections from the walls of the crooked meatus, the waves are wedged between one wall and the membrane of the tympanum which is placed obliquely across the inner end of the tube, the rays will fall upon the concave outer surface of the membrane, and a part will be converged. The more parallel rays of the distant sound will be more converged than those of the near sound, and hence will reinforce the impulse at the center of the membrane more than the other ; but the center is the point of greatest leverage against the hammer bone which is fastened to the back of the membrane ; hence nearly parallel rays of sound would more violently agitate the tympan of the inner ear than more diverging rays, even though both were of the same intensity before striking the concave membrane of the tympanum. The comparison by the ear of the intensities of two sounds would be still more untrustworthy if one of the sources of sound were within the outer tube of the ear.

Loudness, that is, the intensity of sound sensations, does not, then, depend upon the energy of the external sound vibrations, but upon the proportion of the energy which the mechanism of the ear is able to transmit to the auditory nerves, which amount is variable. The ear is so made as to relatively strengthen distant sounds and to weaken near ones, and it is so much the better an instrument because of this, for we are thereby saved from too violent shocks of the nerves, which are most likely to come from near sounds, while at the same time we retain a wide range of hearing. Such illustrations as that of the bells would not be chargeable with setting up a false test for the verification of physical laws, if it was not at the same time explained that the intensity of the sensation of hearing does not, and in consequence of the peculiar construction of the ear, cannot vary as the energy of the moving particles of the sound wave; also that at certain distances the testimony of the ear will approximately coincide, at other distances it will not coincide with the laws of intensity of sound which have been established by mathematical reasoning. The errors involved in the argument from the bells are very commonly held; it is not evident that all such arguments ought to be eliminated from treatises on sound, or at least that their true significance ought to be explained, and that the distinction should be more clearly defined between the subjective word loudness and objective word intensity.

GEORGE H. STONE.

COLORADO SPRINGS, *December 1st.*, 1881.

NEW YORK, *Dec. 19th.*

To the Editor of "SCIENCE."

In the official report of my paper read before the N. Y. Academy of Sciences, published in your last issue (Dec. 16th), I notice the cost of the balloon is given at about £12,000, whereas the amount should have been £4,000. The report also states, "the great body of warm water that flows northward by the peninsula of Norway and Sweden strikes the lighter currents near the Pole and goes on as a submarine current, sweeping around the Pole till it goes out again through Smith's Sound." I desire to say that it is obvious that only a part of the current passes through Smith's Sound.

Respectfully,

JOHN P. CHEYNE, R.N., F.R.G.S.

To the Editor of "SCIENCE."

Sir,—In No. 12 of this year's *American Naturalist* I notice a short paragraph on 'fossil organisms in meteorites.' The subject certainly is interesting and it seems perfectly proper that the '*A. N.*' should at last take notice of it.

The only objection that I may be allowed to raise on behalf of "SCIENCE" and perhaps of myself is that the *American Naturalist* did not duly give credit for what had been reprinted from your columns.* I cannot conceive any plausible reason—unless it be an oversight—why this simple duty of editorial courtesy should be neglected by an American contemporary, while every English scientific journal takes pains to give due credit to "SCIENCE" for all the various data and notes which are gleaned from its columns (*e. g.* Jour. Microsc. Soc., *Lancet, Crookes' Journal, Journal of Science.*)

As to the sceptical remarks with which the *A. N.'s* paragraph concludes, to the effect that "a great deal more evidence will be required by biologists before crediting these alleged discoveries," I may refer all sceptics to Mr. Darwin's opinion, as reported in No. 61 of your valuable journal and to any (silicious) meteorite on which they can lay their hands and grind transparent sections from. This will go far to supply the wanted evidence.

Very respectfully,

GEO. W. RACHEL, M. D.

To the Editor of "SCIENCE."

NASHVILLE, TENN., *Nov.* 30, 1811,

Dear Sir,—I have to-day received from Mr. H. H. Warner, of Rochester, N. Y., $200 (two hundred dollars), the "Warner Comet Prize" for the discovery of Comet E, 1881, on Sept. 17.

Respectfully,

E. E. BARNARD.

MUSICAL FENCES.

In the abstract of an interesting paper by Prof. S. W. Robinson, in a recent number of "SCIENCE," the author begins with the statement that "this sketch is mainly of a simple fact of observation." He gives then a clear exposition of the acoustic phenomena observed by him in walking past picket fences, and the mathematical formula expressing the law of retrogression of pitch.

The observation is by no means new. I am unable to say at what time it was first published, if at all, but am sure that it was made nearly as far back as twenty years ago. On the crisp, cold morning of December 31st, 1861, while taking a walk with Prof. Joseph Le Conte, myself being innocent of mathematics on account of my youth, we noticed the whistling sound returned by a picket fence past which we were moving, our feet striking sharply against the frozen earth. My fondness for music made me particularly appreciative of a musical fence, and I have noticed the phenomenon hundreds of times since that date, knowing its explanation qualitatively, though I did not deduce the formula. If the fence be long, and the distance between the wickets considerable, the returning whistle may be much longer in duration than a quarter of a second. The stroke of a hammer on a board

*S. my paper on the subject in SCIENCE No. 50.

is a convenient substitute for that of the foot against the ground. I recently had a beautiful illustration while riding slowly on the horse-cars in New York. A vehicle passed rapidly between me and a picket fence, every stroke of its wheels against each stone of the pavement being returned as a whistle from the opposite fence. The acoustic effect was much like that of the trilling of a canary bird.

I cheerfully accord to Prof. Robinson the credit of giving mathematical expression to this truth. His observation is none the less original even if others have preceded him, and I am by no means sure that any one has preceded him in giving it publication.

W. Le Conte Stevens.
40 W. 40th St., New York.

Dec. 17th, 1881.

NOTES FROM OUR FOREIGN EXCHANGES.

Phosphorescent Fungi—At the present day, several inferior species of fungi are known, which have the power of throwing out a phosphorescent light. M. Crié, Professor of the Faculty of Sciences at Caen, has noticed new species which spring up on old stumps or between the bark and the wood of the elder-tree.

Rectification of Inferior Alcohol.—Electricity is now employed in the rectification of inferior alcohol. The electricity generated by a voltaic battery and a dynamo-electric machine is passed through the alcohol so as to disengage the superfluous hydrogen. By this means, beet-root alcohol, which is usually very poor, can be made to yield eighty per cent. of spirits, equal to that obtained from the best malt.

A Japanese Antelope.—Several interesting acquisitions have recently been made by the Zoological Garden of London. Among other rare animals, it has obtained a Japanese antelope which has never before been in any collection in Europe. The antelope of Japan (*Capricornus Crispus*) is found only in the highest mountains of the Niphon and Shikoku islands. Very little is known of its habits and it has been but incompletely described by Siebold in his Fauna of Japan.

Phosphorescent Ice —Mr. J. Allen has written to "Nature" an interesting letter, in which he describes a curious phenomenon of phosphorescence of floating ice, observed in the Polar regions. Every time that the bow of the ship, where the observer placed himself, shattered the ice during the night, the ice suddenly shone with a very perceptible light. It is a light similar to that which is produced on the breaking of sugar, the cleavage of sheets of mica, or the striking together of pieces of flint in the dark.

Electric Fusion of Metals—M. Siemens, in the presence of the members of the Congress of Electricians, performed the following curious experiment: in a crucible conveniently arranged, furnished with a perforated cover, fragments of steel were placed; the two currents of an electro-motor apparatus entered the lower and the upper part of the crucible. In 14 minutes the metallic mass became hot, reddened and melted. The mass showed no inflation. The expense of the combustile consumed by the electric apparatus is much less than that which fusion by direct application of heat would necessitate.

Electricity produced by Light.—While traveling in Mexico, M. L'ur, mining engineer, was struck by the fact that the *amalgamation* of silver ore, by what is called the *American* method, only operates well under the influence of light. According to him, the action does not take place in the darkness. He sought the cause of this unexpected effect and his experiments seemed to him, to show that light, by acting upon the mixture of sulphide of silver, sulphate of copper, salt and mercury, develops electricity without which the amalgamation cannot take place.

M. Boussingault, however, expressed an objection to this conclusion, which appears decisive; that in Mexico, the operation is not confined to small quantities, but whole mountains of ore are acted upon. Now light is only able to act upon the periphery of the latter, and the largest part of their mass remains in permanent darkness.

The Telephone in a Storm.—A very curious experiment was made and announced by M. René Thury, of Geneva. He stretched a metal wire from one roof to another. One extremity of the wire was in connection with a telephone, the opposite extremity with the earth. During a storm, every time there was a lightning stroke, even at a distance of 20, 30, and even 40 kilometres, the telephone gave a very characteristic sound. This noise, according to M. Fleury, was due to the peculiar electric currents, called currents of induction, produced under the influence of the atmospheric electric discharge. It was a sort of return impact.

The Sulphate of Alumina of Commerce.—For a long time there has been a tendency to substitute sulphate of alumina for potash or ammonia alum, since it is richer in alumina. But the manufacture of pure sulphate of alumina, that is to say, free from iron, is not easy, at least in an economical point of view.

During the last twenty years, pure hydrated alumina has been prepared at a low cost, and by saturating this alumina by sulphuric acid, a warm liquid is obtained which congeals into a dry and easily transportable mass of sulphate of alumina containing about 15 per cent. of alumina.

The products obtained in this manner are relatively expensive, and it would be a great advantage to purify the ferruginous sulphate of alumina furnished by the action of sulphuric acid upon common clay, if this purification could be accomplished by an easy and less costly method.

Extraction of Magnesia from Sea Water.—*The Moniteur des produits chimiques* contains the following method of abstracting magnesia from sea water: "magnesia can be precipitated from sea water by means of calcium, just as from other more concentrated solutions. After precipitation and rest for a day, a cubic metre of sea water gives a precipitate of gelatinous magnesia, about 80 litres in volume. The treatment on a large scale of water whose magnesia is to be deposited in large basins, can easily be accomplished, speaking in an industrial point of view; the calcium will be the greatest expense.

If the magnesian sediment thus obtained is treated with phosphoric acid, a precipitate of tribassic phosphate is obtained, which, filtered, becomes an excellent chemical agent for the precipitation of ammonia from excrements in the form of ammoniac-magnesian sulphate, which is a powerful manure.

Spontaneous Combustion of Carbon.—Spontaneous combustion in colliers is a very important question, for, in 1874, 70 cases of this kind occurred. The recent investigations of M. Haedicke have thrown light upon this subject. These experiments were conclusive in proving that this combustion is due to the influence of iron pyrites. This substance becomes oxidized when submitted to moisture and is changed into ferrous sulphate. During this decomposition, the carbon bursts and offers a larger surface to the action of the air. The ferrous salt is then transformed into a ferric salt which yields up its oxygen to the carbon. In order to prevent spontaneous ignition, all currents of air should be excluded, unless they should be allowed to enter from the beginning in great quantities, so that the air acts as a cooling agent. As moisture prevents ignition and the accumulation of oxygen, the introduction of a jet of steam, where the temperature of the carbon has been raised to a high degree, will also act as a preventive.

Sea-sickness.—A correspondent of the "Paris Medical" has sent a communication to the editor, which will prove interesting to many persons who suffer sea-sickness in their travels.

"In a recent voyage from Algeria to France," he writes, "the sea being very rough, and almost all the passengers being sick, the officers of the ship could offer but insufficient means of relief. Among the passengers there was one, about thirty years old, who suffered cruelly. He vomited continually for thirty hours, and his sufferings became so severe that the surgeon had to be called. After hearing him prescribe lemon juice, I asked him if he had not morphine or chloral. To my astonishment he replied that he had none. I then offered him one centigram of morphine and my syringe. This was accepted. A solution was made in 20 drops of water, was injected into the epigastrium, and, a half hour afterwards the sickness was allayed. He ceased

vomiting and remained unmolested during the rest of the journey. This fact appeared remarkable to me. The benefit had been immediate." If this observation can be confirmed by other similar cases, it would be very fortunate, for then the surgeons of the maritime and transatlantic companies would be able to relieve passengers who suffer seasickness.

THE SEPARATION OF WOOL AND SILK IN WOVEN GOODS.—A. Rémont.—The following method is sufficiently exact for commercial purposes: the sample is kept for a quarter of an hour in boiling water containing 5 per cent. of hydrochloric acid, and is then washed and dried. The threads of the warp are then separated, if possible, from those of the weft, and examined separately as follows: a thread is burnt. There is given off a smell like burning horn, and a thread heated with a fragment of caustic soda evolves ammonia. In this case some threads are plunged in basic zinc chloride at a boil. If they dissolve completely the threads are *silk*. If on adding hydrochloric acid there is a plentiful flocculent precipitation, the threads are silk mixed with wool or with vegetal fibres. If nothing dissolves in zinc chloride, the threads are plunged in a boiling solution of soda, not too concentrated. If they dissolve completely, *wool*. If partially, *wool* and *cotton*. If no odor of burnt horn is given off, the threads consist entirely of vegetal fibres. These results may be confirmed by means of the microscope. For the quantative examination, if the preliminary tests show silk, wool, and cotton, four swatches weighing each 4 grms. are cut; one is laid aside and the three others are boiled.—*Journal de Pharmacie et de Chimie.*

THE REDUCTION of COPPER SOLUTION BY GLUCOSE* appears first to have been utilized by Trömmer. Frommherz suggested the employment of a citrate to keep the cupric oxide in solution. Modifications of the ordinary alkaline tartrate solution have been devised by Barreswil, Poggiale, Rosenthal, Chevalier, Boussingault, Reveil, Fehling Strohl, Viollette, Magneshahens, Lowenthal, Joulie, Pos soz, etc. Loewe employed glycerin instead of a tartrate. Various treatments of the precipitated cuprous oxide have been proposed by the following chemists: Mohr dissolves the oxide in hydrochloric acid, and titrates with permanganate. Brunner dissolves in an acid solution of ferric chloride, and estimates the reduced iron by bichromate or permanganate. Champion and Pellet dissolve the precipitate in hydrochloric acid and chlorate of potassium, boil off free chlorine, and titrate the liquid with stannous chloride. Girard and Soxhlet reduce the cuprous oxide in hydrogen, and weigh the metallic copper. Muter dries the cuprous oxide at 100° C., and weighs it as Cu_2O. O'Sullivan and other operators ignite the precipitate strongly, and weigh as CuO. Ferdinand-Jean dissolves the cuprous oxide in hydrochloric acid, and weighs the metallic silver precipitated on adding ammoniacal silver nitrate. Maumené uses an excess of copper solution, filters, adds ammonia to the filtrate, and estimates the residual copper by titration with sodium sulphide, for which Perrot substitutes potassium cyanide. Lastly, Pavy adds ammonia to the alkaline cupric solution, and runs in the sugar solution till the hot liquid is decolorized.

* From an Advance-Shee of Allen's "Commercial Organic Analysis," vol. ii.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING DEC. 17, 1881.

Latitude 0° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

DECEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun
Sunday, 11	30.432	30.476	12 p. m.	30.362	0 a. m	27.0	25.7	31	4 p. m.	29	4 p. m.	20	7 a. m.	20	7 a. m.	93.
Monday, 12	30.292	30.476	0 a. m.	30.110	12 p. m.	38.0	36.0	43	6 p. m.	43	6 p. m.	29	6 a. m.	29	6 a. m.	74.
Tuesday, 13	30.021	30.110	0 a. m.	29.938	12 p. m.	51.3	47.6	59	12 p. m.	54	12 p. m.	41	5 a. m.	40	5 a. m.	95.
Wednesday, 14	29.806	29.938	0 a. m.	29.688	4 p. m.	56.3	52.7	67	4 a. m.	60	4 p m.	40	12 p. m.	40	12 p. m.	69.
Thursday, 15	30.177	30.322	12 p. m.	29.900	0 a. m.	30.3	29.6	40	0 p. m.	40	0 a. m.	26	12 p. m.	26	12 p. m.	60.
Friday, 16	30.427	30.492	11 a. m.	30.322	0 a. m.	24.0	23.0	29	11 p. m.	28	11 p. m.	18	7 a. m.	18	7 a. m.	92.
Saturday, 17	30.232	30.396	0 a. m.	30.176	12 p. m.	33.7	31.3	40	0 a. m.	36	4 p. m.	25	4 a. m.	25	4 a. m.	88.

Mean for the week .... 30.198 inches.
Maximum for the week at 11 a. m., Dec. 16th .... 30.492 "
Minimum " at 4 p. m., Dec. 14th .... 29.688 "
Range .... .804 "

	Dry.		Wet.
Mean for the week	37.2 degrees		35.1 degrees.
Maximum for the week, at 4 pm., 14th	67. "	at 4 pm 14th,	60. "
Minimum " " 7 am., 16th	18. "	at 7 am 16th,	18. "
Range " "	49. "	....	42.

DECEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.
Sunday, 11	n. w.	w.	s. w.	125	1¼	4.30 pm
Monday, 12	e.	s.	s. w.	187	7	7.20 pm
Tuesday, 13	s. w.	w. s. w.	w. s. w.	274	6¾	3 50 pm
Wednesday, 14	s. w.	s. w.	n. n. e.	303	6	10.50 am
Thursday, 15	n. w.	n.	n. n. w.	238	4½	3.15 am
Friday, 16	n. n. e.	n. n. e.	n. w.	128	2	2.20 am
Saturday, 17	w. s. w.	w. s. w.	w.	263	3¼	3.00 pm

DECEMBER.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.
Sunday, 11	.108	.130	.137	100	78	79	0	4 cir. cu.	2 cir. cu.
Monday, 12	.149	.177	.244	89	66	91	9 cu.	10	10
Tuesday, 13	.244	.282	.336	91	67	70	10	4 cir. cu.	5 cu.
Wednesday, 14	.367	.420	.288	71	68	100	8 cu.	9 cu.	10
Thursday, 15	.181	.137	.153	100	79	100	10	8 cir. cu.	0
Friday, 16	.098	.106	.136	100	75	88	0	0	0
Saturday, 17	.141	.144	.157	100	63	71	1 cir.	2 cir.	0

DECEMBER.	RAIN AND *SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration h. m	Amount of water	OZONE.
Sunday, 11	-----	-----	-----	--	0
Monday, 12	7 pm	10 pm	3.00	.03	0
Tuesday, 13	-----	-----	-----	--	0
Wednesday, 14	9 am	12 pm	15.00	.40	10
Thursday, 15	0 am	8.30 am	8.30	.40	9
Friday, 16	-----	-----	-----	--	0
Saturday, 17	-----	-----	-----	--	0

Distance traveled during the week .... 1,518 miles.
Maximum force .... 7 lbs.

Total amount of water for the week .... .83 inch.
Duration of rain .... 1 day, 2 hours, 30 minutes.

* Thursday, 15th, 1¼.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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SATURDAY, DECEMBER 31, 1881.

As the present number of this journal concludes the second volume of "Science," the moment appears opportune for us to acknowledge our obligations to the many friends of the journal, who, by their contributions of valuable papers, have assisted in placing it in the distinguished position which it now occupies.

The expression of good will and satisfaction which we have received from so many of our subscribers, is encouraging for the future of the journal, for we must confess that our plans for the editorial management have been but partly developed; if we have deserved such recognition in the past, we look forward for a wide extension of our circulation in the future, when the improvements and additions which we contemplate shall be carried out. Arrangements are in progress to increase the number of pages of "Science" from twelve to sixteen, the four extra pages to be devoted to applied and practical science; in this division the most recent application of scientific principles to the arts and manufactures will find a place, and novel inventions of real scientific merit will be fully described.

Suggestions from our readers respecting any improvement or addition which will increase the efficiency of the journal, will be always welcome, and receive consideration, and we ask our subscribers to introduce "Science" to their friends, and to forward us names of those who in their opinion would desire to receive a sample copy.

We are gratified to find that heads of schools and other representatives of the intelligent classes are sending in their subscriptions, and recognizing "Science" as one of the highest educational journals in this country. The United States Commissioner of Education at Washington has expressed his high approval of the journal in this respect, and we hope to find it in the hands of all men interested in the education of others.

Our subscription list now includes Principals of Schools, Professors in Colleges and Universities, Pastors, Physicians, Manufacturing Houses; and we claim that no person of average intelligence should fail to read "Science," for he will find it a valuable and useful weekly journal, and when bound, a standard work of reference for all time.

An esteemed contemporary calls for a scientific journal, "such as the great body of intelligent people will admit with confidence to their homes." "Science" certainly fills this *role*. The editorial conduct of this journal has been based on a policy to admit the widest discussion of all current scientific subjects, but no editorial bias has been given to any particular set of views. The editor has not himself indulged in polemics, nor permitted the discussion of religious questions, believing that the ground covered by investigations of all branches of the sciences, is sufficient for one journal to cultivate, and that aimless attacks on religious belief are foreign to the purpose of a scientific journal, and inconsistent in a country where religious liberty is guaranteed to all.

At the moment of going to press, a copy of Mr. S. E. Cassino's International Scientists' Directory came to hand. We have, therefore, only time to take a glance at the book, which is a handsome work of 400 pages, containing the names, addresses, special department of study, etc., of those engaged in scientific investigations in America, Europe, Asia, Africa and Oceanica.

About 18,000 names and addresses are given, and the general arrangement of the work is all that can be desired. The Directory will be welcome to all engaged in scientific work, and we trust that it will be strongly patronized, and thus enable Mr. Cassino to continue his good work, which is still far from complete; although so many names are given in this directory, it clearly represents only a fraction of the whole list of scientists, as such well known names as Professor R. O. Doremus and Professor J. W. Draper, are omitted.

We have no doubt that Mr. Cassino has done his best to make his Directory complete, and we congratulate him upon his success, and it simply rests with scientific men to aid him in making future editions perfect.

On receipt of two dollars we will mail a copy of the International Scientists' Directory to any address in the United States or within the postal union.

THE DEVELOPMENT OF HEAT BY MUSCULAR ACTIVITY.

By Professor A. Fick, of Würzburg.

It is the object of physical science in the proper sense of the word, to perceive in all the phenomena of nature the operations of the *same* forces, with which any two material particles always act upon each other, when they come in contact with each other in the same relations. This object has never been so clearly seen by the majority of naturalists, as during the last decade. Since that time a law already proved in mechanics has been recognized as one applicable to all the events of nature. It is called by Helmholtz, who in a treatise which appeared thirty-one years ago, first demonstrated its universal importance, the "law of the preservation of power"; recently the designation "law of the preservation of energy" has also been brought into use by English men of science. The amazing productiveness of this fundamental law of the operation of all natural forces essentially consists in the fact that from it may easily be derived experiments for testing results even in natural phenomena, in which in detail the nature of the acting forces is wholly concealed.

Therefore it could not fail to happen, that since this time individual investigations in the most varied departments of physical science have principally turned upon this fundamental principle. It now seems to me that the results of such individual investigations, which are connected with the most universal points of view, might be best adapted to secure interest even outside the circle of the scientists. In this opinion I will venture to claim the attention of the readers of this publication for some general observations connected with an experiment made by me a short time ago, and elsewhere communicated to persons familiar with such matters.

Each individual can experience in his own body at any moment, that with the aid of his muscles he can conquer opposing forces and set masses in motion. The former happens, for instance, when we lift a burden or throw the whole weight of the person upward in climbing a mountain; the latter occurs when we hurl a stone or swing a hammer. The principle of the preservation of power now demands that, where we see forces conquered or masses moved, necessarily powers on the other side have "acted" or performed labor, that is, that the points of assault of forces have been displaced. This, for instance, is clearly apparent in the voluntary fall of a heavy body. It is the point of attack of a power directed downward, namely weight, and as under the influence of this power it moves downward, its velocity increases; or when in a wavering balance one scale with its burden ascends—its weight is conquered—but the other sinks and its weight performs a certain amount of work. So if by the mediation of muscular action we see forces conquered or masses moved, it must be asked: what powers have acted or performed the labor here, that is, have changed their points of attack in their action.

Forces which, for instance, like weight, act upon larger bodies in a similar manner, will not of course be alluded to here. The point in question can only concern powers that operate even among the smallest particles of muscular substance, that is chemical powers of attraction. Something must take place in the muscle similar to what occurs in the steam-engine, when in the act of combustion under the boiler the particles of carbon and oxygen, obeying their strong reciprocal power of attraction, rush towards each other, makng violent little movements, and a portion of this energy, by means of a series of shocks, is applied to the conquest of opposing forces, or to accelerating the speed of bodies. So in the muscle, during its activity, chemical processes evidently take place, with which powerful kindred forces come into action. That this is really the case can be shown by experiments. Singularly enough, it is not only an analogous, but for the most part at any rate precisely the same chemical power of attraction which performs the work in the active muscle and in the steam-engine, namely the power of attraction between the particles of carbon and the particles of oxygen. The product of the operation of this power of attraction, carbonic acid, appears in a certain quantity at every act of muscular motion.

In all the examples, in which, by the mediation of any arrangements, through whose operation the action of chemical powers of attraction, taking place even in extraordinarily small distances, accelerates the movement of bodies, or overcomes mechanical forces, like weight, a general remark may be made, which has hitherto been everywhere confirmed by experience. The lines of communication between the particles undergoing a change by means of a chemical process are usually irregularly distributed in every part of the space. The movements arising from the individual processes of change are, therefore, also irregularly driven in all directions, and thus can never be applied in their full strength to overcome an opposing force acting in a fixed direction, or to accelerate the speed of a body, whose particles are all moving in the same direction. Only a portion of this collected energy of motion can appear in such a form. A fraction, greater or less, according to circumstances, of the sum of the individual processes of change must retain its original form of the irregularly whirling movement of the tiniest particles. This conclusion may, therefore, be briefly expressed thus: wherever in a chemical process the power of attraction of the smallest particles of different substances performs labor—no matter under what circumstances this may occur—a portion of the labor will always be employed in the development of *heat*.

The *heat* contained in a body is, therefore, nothing else than the energy of slight invisible irregular whirling movements, in which the tiniest particles of the body are included. To increase the temperature of a body, therefore, is merely to increase the energy of these irregular molecular movements of the smallest portions. This view instantly finds support in the common phenomenon, that at the increase of the temperature of a body above a certain degree its particles in consequence of the colossal energy of motion really pulverize each other—"the body evaporates."

If this view of heat is correct, a certain degree of heat can be produced by a certain amount of work. The proportion of work, or the operation of a power is, as is well known, the product of the intensity of the power and the distance through which it has acted. Therefore the product of the unit of the intensity of the power, the *kilogram*, and the unit of the distance, the *meter*, is chosen as the unit of this power. This unit of the value of the work is called the *kilogrammeter*. As the unit of the quantity of heat the same degree has been fixed that is required to be supplied to a kilogram of water, when its temperature is to be raised from 0° to 1° of the Centigrade.

Natural philosophy has now succeeded—and it is one of its most important achievements—in showing, that for the production of a unit of heat an expenditure of work of 425 kilogrammeters is requisite. This number is called the *mechanical equivalent* of *heat*, because it is thereby possible to calculate each quantity of heat in a certain number of mechanical units of work, which is requisite for its production.

The knowledge of the mechanical equivalent of heat enables us to measure exactly the work performed by any chemical process of kindred forces operating even at immeasurably little distances, although we know nothing at all of the laws of action of these forces in detail. In fact, we need only direct the process, so that no effect is produced except the development of heat. If we then measure the heat developed and multiply the number of units found by 425, we shall have the labor which the chemical powers of attraction have performed in the process, expressed in kilogrammeters, since according to the

supposition the whole operation of this labor consisted exclusively in the development of heat. The burning of one kg. of coal may serve as an example; if no other effects are accomplished, about 8000 units of heat will be released. The work which the kindred powers between the atoms in one kg. of coal and the two-fold number of atoms of oxygen accomplish in their union into carbonic acid, thus amounts to 8000 x 425 or 3,400,000 kilogram-meters. From this an idea may be formed of the prodigious intensity of the chemical power of attraction between an atom of carbon and an atom of oxygen. The force with which the particles of carbon, amounting only to one kg., rush from a very little distance to the corresponding particles of oxygen in burning, is precisely as great as when a body weighing 3,400,000 kilograms falls from a height of 1 m.

Let us go back with these axioms from natural philosophy in general to muscular action. If, as was shown, there are chemical powers of attraction, whose operation or performance of labor produces mechanical effects which are externally perceptible, besides these, heat must also proceed from every muscular action. This proposition, which we here bring forward as a conclusion from the most universal lessons of the action of powers, has already long been acknowledged as a principle derived from experience.

It was by no means easy to prove this proposition. To be sure, it is rendered extremely probable by the daily experience, that our bodies are perceptibly heated by great muscular exertion, and give off more heat than during the same time with the muscles at rest. But this does not afford an accurate proof. It might be represented that the excessive activity of the muscles only afforded increased opportunity for heat-producing combustion in other constituent parts of the body, for instance in the blood. An exact proof can, therefore, only be given by putting in action a muscle severed from connection with the rest of the body, and proving that heat is developed therein. Such experiments can, of course, only be made on the muscles of cold-blooded animals, because those of the warm-blooded, when separated from the body, lose their vital properties too quickly.

The first person who made such experiments and has shown an increase of temperature, that is a development of heat in *isolated* muscles by action, was *Helmholtz*. This fundamental fact could not fail to attract great attention, and make people endeavor to ascertain what circumstances had an influence on the greater or less development of heat by muscular action. The most important labors in this direction proceeded from *Heidenhain's* laboratory. He has especially much improved the thermo-electric system, which alone can be used to ascertain the increase of temperature of the muscles. With the aid of this system one can distinctly perceive even the extraordinarily slight increase of temperature, which a little frog muscle undergoes at a single, by no means energetic, movement, that scarcely amounts to $\frac{1}{1000}$ of a Centigrade. In successive experiments it can even be determined, in which *more*, and in which *less*, heat was developed, but until now the system has not been thoroughly adapted to fix the absolute value of the increase of temperature.

Some time ago I succeeded in so changing the thermo-electric apparatus, that it is possible, by its means, to fix with some degree of accuracy, the increase of temperature a muscle experiences in its action. Thereby the possibility was instantly afforded, of stating in the usual units the quantity of heat developed by the muscular action. This quantity of heat is namely, evidently, the increase of temperature multiplied by the capacity for heat of the proportion of muscle used, which latter is assumed to be about equal to $\frac{9}{10}$ of the capacity for heat of a body of water of equal size.

According to a general observation previously made, the whole labor performed in the muscular act by chemical powers of attraction can now be definitely determined. For this purpose it is only necessary to allow the muscular action to pass away, so that finally no sort of mechanical effect remains; then, since every labor of forces must leave an effect, a quota of heat will exist that will be the exact equivalent of the work performed by the chemical powers. The condition just expressed may be fulfilled by simply letting the muscle, in its action, raise a weight; but allowing this to fall again, so that it pulls the muscle which meantime has relapsed into a state of rest. In so doing the work performed by the weight of the falling body will evidently be used for the development of heat in the apparatus. To be sure, it might now be asked, in what portions of the whole machinery used, this amount of heat is developed. Theoretically it is beyond doubt, that a portion of it is set free in the intermediate pieces, which connect the weight with the muscle, especially by the friction at the points of union, but since these intermediate pieces are practically non-ductile, and the friction at their points of union can only be very slight, it may be assumed from the beginning, that the quota of heat in question is almost entirely released in the body of the muscle itself, which, by its extreme ductility, receives, so to speak, almost entirely the shock of the falling burden. This supposition is so probable, that in the exact scientific publication of the result of my experiments, I have pre-supposed it as a matter of course. Meantime I have made experiments in my laboratory, which render this supposition one empirically shown.

The experiments have been made in the following manner. A body of known weight fastened to the muscle was raised, not by its own action, but by other labor to a measured height and then allowed to fall. The increase of temperature experienced by the muscle in consequence of the jerk was now measured, and by multiplication with the capacity for heat of the muscle, the quantity of heat developed in the muscle was ascertained. It usually corresponded in a really surprising manner with the thermic equivalent of the mechanical labor, which was applied to raise the appended burden. This affords the proof, that the heat produced by such a jerk is liberated almost entirely in the *muscle*, and only very inconsiderable fractions are developed in the other portions of the machinery used. Every such experiment can thus be looked upon as fixing the mechanical equivalent of heat, which, of course, in point of accuracy, falls far behind the purely physical tests, but is worthy of notice because a living tissue is the means of ascertaining it. To us, however, the interest of these experiments consists in the fact that they prove the reliability of the system used to fix the heat of the muscles.

Let us now return to the development of heat by *active* muscular action, and consider more closely the numerical product of an accurate experiment. That in the estimates of the quantities of heat, and afterwards the value of labor too, many ciphers may not appear immediately behind the comma, we will base them upon units a million times smaller. So, for the unit of heat, we will take the quantity of heat necessary to raise the temperature of 1 mgr. of water from 0° to 1°. As the unit of labor we will choose instead of the kilogrammeter the grammillimeter. The equivalent proportion, therefore, remains unchanged—425. For an experiment a body of muscle weighing 3114 mgr. had lifted in ten pulls, rapidly succeeding each other, a burden of 500 gr. 10 times, and the latter had fallen again as many times, so that at last it hung no higher than at first. The temperature of the mass of muscle was increased 0,0195° by this act. Now, since 3114 mgr. of muscular substance possesses exactly as much capacity for heat as 2803 mgr. of water, the increase of temperature which followed, required $2803 \times 0{,}0195 = 54{,}6$ units of heat. But in our experiment the production of this quantity of heat is the *only* effect of the work accomplished by the chemical powers of attraction in the muscular action. It must, therefore, ex-

pressed in the measure of labor, have amounted to 54.6×425, that is 23205 grammillimeters.

The chemical process, which takes place in muscular action is, it is true, by no means accurately known in the individual stages of its course; but as a whole, it undoubtedly consists in the combustion of a body free from nitrogen, whether fatty or saccharine, to carbonic acid and water. The numbers obtained, therefore, afford us a point, by which to determine what quantities of the above mentioned materials must be consumed in a muscular contraction. We know, through *Frankland's* researches, that in the consumption of 1 mgr. of sugar the chemical powers of attraction perform as much work a is necessary to produce 3800 units of heat. Now, since in the ten contractions of our experiment, 54,6 units of heat were produced, an expenditure of material of $5{,}46 \div 3800 = 0{,}014$ mgr. would have been necessary, under the supposition, that the combustible material was a saccharine body. Let us suppose that the combustible material is a fatty body, then a still smaller expenditure would be sufficient, to produce the effect observed, namely, $54{,}6 \div 9000 = 0{,}0067$ mgr., because 1 mgr. of fat, according to the estimates of the investigator just mentioned, supplied in its combustion, 9000 units of heat. So, for one contraction the combustion of 0,0014 mgr. of sugar, or of 0,00067 mgr. of fat, would have been requisite. If we divide this number by 3.1 (the weight of the quantity of muscle used in grams) the result will show how much material must be consumed at *one* energetic contraction in a gram of muscular substance, that is 0,00045 mgr. of a saccharine, or 0,00022 of a fatty combination. So it appears, that for 1000 energetic contractions not quite 1 mgr. of combustible material in each gram of muscle is requisite, and, therefore, it can no longer surprise us, that only very small quantities of the actual combustible material are ever found in the muscular substance, the greater portion of which, as is well known, really consists of very different materials, principally of substances like the white of an egg.

The results obtained with the new systems can be applied to the decision of the question, what portion of the work performed by chemical powers in the active muscle, can, in the most favorable cases, produce mechanical outward effects. The closest interest in this question might be designated as an "economical" one. In fact, the real object of the animal subject in muscular activity is the production of mechanical effects in the surrounding universe, and one might denote the portion of the work accomplished by chemical powers, which is applied to the mere production of heat, as an inevitable loss from the point of view of animal economy. At any rate, one will have the more reason to admire the judicious arrangement of the muscular substance, which can apply a larger portion of the chemical labor performed in it to external mechanical results.

It is precisely the same as in the steam-engine, whose construction we also call the more perfect, according to the larger portion of the work performed by the chemical powers of attraction in the burning of the coal it allows to be used to produce mechanical effects. In spite of the most eager efforts of technics hitherto no attempt has been successful in making more than $\frac{1}{10}$ of this labor mechanically effectual. Fully $\frac{9}{10}$ are lost to the objects of the machine, by being inevitably employed in the production of heat, which at the utmost can only be used for minor purposes, such as the heating of rooms and similar objects.

If it must now be ascertained, how the muscle is situated in this respect, it is only necessary to fix, by experiment like the one above described, what mechanical effect has been accomplished in a given time, and compare this measured in the proportion of work, with the chemical labor calculated by the heat produced. It will be advisable to pay special attention to the fact, that the heat finally developed would be less by a corresponding amount, if the experiment had been so arranged, that the mechanical effect, that is the raising of the weight, had been maintained. The quantity of heat corresponding with this effect was first released in the muscle by the falling of the burden again.

By the 10 contractions of the foregoing experiment 500 gr. were raised on an average about 1.3 mm. high. Thus the mechanical result amounted in the whole to 6,670 grammillimeters. The work performed by chemical powers of attraction in the 10 contractions we have found above—23,205 grammillimeters. This number is about $3\frac{1}{2}$ times 6,670. Thus, by these contractions, somewhat over $\frac{1}{4}$ of the whole chemical labor was applied externally and not quite $\frac{3}{4}$ to the direct production of heat. That in the actual experiment this quarter was also finally converted into heat, depended merely on the external arrangements, which permitted the burden raised to fall again each time.

We see by this, that—as was to be expected—the muscle machine is very superior to even the most perfect steam-engine, in so far that it *can* employ the combustible material twice as frugally for the same main object.

Besides, this relation between mechanical action and development of heat is by no means obtained at *every* muscular contraction. I have intentionally selected from my experiments as an example, the one in which the mechanical labor amounts to the largest fraction of the whole chemical labor. To obtain this most favorable proportion, the burden must stand in a certain relation to the thickness of the muscle. If the burden is larger or smaller, a smaller portion of the chemical work will be used for mechanical action, or—as it might be expressed—the combustible material will be less economically used. This proposition may be demonstrated *a priori*, for it is easily seen, that in the two extreme cases, where the burden is a cypher or infinitely great, chemical work is performed and heat developed, but no external mechanical action is obtained.

The solution of the question, in what relation the mechanical action for the development of heat can stand, under the most favorable circumstances, towards the muscular contraction, enables an observation to be made which throws new light upon the change of substance in animal bodies. As is well known, the change of substance in animal forms may be designated in general as a process of combustion. In reality, a certain quantity of combustible nutritious matter daily enters into the fluids, and a corresponding quantity of oxygen is taken in with the breath. On the other hand, every day on an average, a precisely similar quantity of substances is withdrawn, whose combination is to be regarded as the product of an almost total combustion of the nutritious matter. The condition of the body with this equal balance between receipts and expenditures, remains for a long time apparently unchanged.

With the formation of the product of combustion from the assimilated nutritious matter and the inhaled oxygen, the colossal power of attraction of this element for the elements of the nutritious matter, especially for the carbon and hydrogen gas, now performs a fixed amount of labor, which is independent of where the combustion takes place, and whether it occurs at once or in various stages at various places.

People were formerly inclined to suppose, that the greater portion of the combustion in question occurs either in the fluids themselves or in special organs, such as the liver, the kidneys, etc.

Since the changes occurring in animal bodies have begun to be viewed from the standpoint of the principle of the preservation of power, it must be looked upon as a self-evident truth, that at least a certain portion of the assimilated nutritious matter passes into the muscles, to be first consumed here, since from the point of view of that principle, the mechanical performance of the muscles can only be understood as the action of the labor of the

chemical powers of attraction, as we have done in the preceding discussions. The question may now be raised, how large a fraction of the whole combustion takes place in the muscles, and how large a fraction in the other parts of the body? The distribution of the process of combustion in the different places might, therefore, be accomplished in two ways. One part of the material might be consumed entirely in the muscles, the other entirely elsewhere, or certain stages of the combustion of the whole material might take place in the muscles, and other stages in other places. However this may be, as it is supposed that a considerable portion of the combustion takes place outside of the muscular substance, it must be expected that, under all the circumstances, far more than $\frac{3}{4}$ of the whole heat of the combustion of the assimilated nutritious matter in animal bodies appears as heat, and only the equivalent of far less than $\frac{1}{4}$ is available for mechanical labor. For, as we saw, even under the *most favorable* circumstances, about $\frac{3}{4}$ of the chemical work performed in the muscle itself is inevitably used for the production of heat. But under these most favorable circumstances, however, probably *all* the muscles do not take part in the labor of the whole body. Therefore, in the acts of living beings we must assume that more than $\frac{3}{4}$, probably $\frac{4}{5}$ of the result of the work performed in the muscles by chemical powers, finally appears as heat. Now, if the material coming into the muscles for combustion should be even a moderate portion, for instance $\frac{1}{3}$ of the whole assimilated nutritious matter, while $\frac{2}{3}$ was consumed elsewhere, then $\frac{14}{15}$ of the chemical work performed by the whole combustion is used for the mere production of heat, since $\frac{2}{3}$ of the labor accomplished outside of the muscles can have only the result of producing heat, and of the third coming from the muscle, $\frac{4}{5}$ will also produce mere heat. So, under this supposition, it must be expected, that at the utmost the equivalent of $\frac{1}{15}$ of the heat proceeding from the combustion of the nutritious matter would be available for the mechanical effects of the organism, externally.

It is already more than twenty years since Hemholtz, by very convincing arguments, proved from facts known at that time, that in seasons of extreme muscular labor, for instance, climbing a mountain, the measureable mechanical performances of the whole organism are proportionally considerably greater. They are equal to the equivalent of about $\frac{1}{5}$ of the heat of the consumption of the material that burns during the time of these performances, in the whole body. Unless the supposition is now made, that the muscles of mammalia can work incomparably more economically than the muscles of the frog—a supposition wholly unjustified by our knowledge of the properties of the muscular substance in the different bodies of animals—we must conclude that, in times of extreme muscular activity, the whole process of combustion takes place in the muscles and the chemical processes going on in other parts can only be those in which the chemical powers of attraction accomplish no considerable labor. In fact, from the results of our experiments concerning the heat of the muscles we have inferred, that by the chemical labor performed in the active muscles themselves in the movements of living beings, fully $\frac{4}{5}$ is employed in the production of heat; but if chemical labor was performed in other parts of the body, whose whole result could be only a purely thermal one, more than $\frac{4}{5}$ of the chemical labor performed in the whole body must go to the production of heat, and less than $\frac{1}{5}$ would remain for mechanical external actions.

If it is once proved, that in times of extreme muscular action the processes, by which the chemical powers of attraction perform labor, take place almost exclusively in the muscles, a similar performance will occur even in times of comparative muscular rest; for otherwise it must be supposed, that the change of substance during the period of rest takes a totally different direction from that during the time of muscular activity, which is scarcely conceivable. Yet it must be supposed, that in animal bodies a certain kind of combustible material is prepared for the machinery of the muscles, for which in other portions the conditions of combustion do not exist, as coke cannot be burned in a stove arranged for wood. We shall, therefore, be compelled to suppose, that the process of combustion, which renders muscular labor possible, glimmers continually in this texture even in times of rest, only with so little strength, that there is no mechanical action, and only heat is produced.

There is a very note-worthy harmony between this inference and an assertion made by Pflüger and some of his pupils on the basis of very different facts, which is, that in the muscles, even during periods of rest, processes of combustion occur, which are under the influence of the nervous system; they can be kindled to considerably higher degrees of intensity, before attaining the point requisite for the purpose of a visible mechanical action of the muscle. The increase in the department of these lower degrees of power would, therefore, lead only to an increase of the production of heat, and according to the well-founded hypothesis in question ought to explain the fact, that the development of heat in animal bodies can exist under conditions of the loss of heat externally.

From all this one would form the following idea of the course of the chemical processes, by which the assimilated nutritious matter is transformed into the rejected matter. The nutritious matter enters into the blood, the liver, and other places only during the chemical processes in which the chemical powers of attraction either perform no considerable work, or in which as many chemical powers of attraction are conquered as come into positive action. These may be partly synthetic performances, partly disunions. Above all, it must be supposed that the greater portion of the nutritious albumen undergoes, directly after its reception into the fluids, a process of this nature, in which a body containing nitrogen is separated, that soon leaves the body under the form of urine. The remnant of the nutritious albumen, free from nitrogen and the other nutritious matter rich in carbon and hydrogen, is then supplied to the muscles as combustible material, perhaps loosely united with the oxygen received by the breath. In action, however, the vast powers of attraction between the atoms of oxygen on one side and the atoms of carbon and hydrogen on the other, first enter into the muscular tissue, whereby in the formation of carbonic acid and water, partly heat and partly mechanical effects proceed.

I should consider the object of these lines attained if I succeeded in showing how a few insignificant thermometrical experiments in frogs' muscles are capable, from the point of sight of the principle of the preservation of power, of casting a new light on all the particulars of the nourishment of the human body.—*Translated from "Deutsche Rundschau," by M. J. S.*

DR. T. S. COBBOLD exhibited (at the Linnean Society's meeting, November 5) under the microscope about a hundred eggs of *Bilharzia hæmatobia*. They were taken from a gentleman who had just arrived from Egypt, and who was the victim of hæmaturia, supposed to have been contracted during a shooting expedition. By adding water nearly all the eggs were hatched during the meeting of the society, and a rare opportunity was thus afforded of witnessing the behavior of the newly-born ciliated animalcules.

DONATION TO AID SCIENCE. Mr. Charles Crocker has made the very handsome donation of $20,000 to the California Academy of Sciences, the income of which is to be devoted to aid worthy and studious investigators in any branch of science, who, by their scientific work, have excluded themselves from acquiring support through the ordinary avocations of current industrial life.

M. PAUL BERT, the new French Minister for Public Instruction, is said to be a candidate, in the section of Medicine, to fill the place vacated in the Academy of Sciences by the recent death of Dr. Bouillaud.

EXPERIMENTS OF M. BJERKNES.

"INVERSE" IMITATION OF ELECTRIC AND MAGNETIC BY HYDRO-DYNAMIC PHENOMENA.

It must be confessed that absolutely nothing is known of the real nature of electricity. The principle of the conservation of energy and that of the unity of physical forces, which tends to simplify the phenomena and embrace them under one common term, for want of a clear and precise definition lead us to consider electricity or rather electric phenomena, as a *mode of movement*. This conception removes the difficulty, simplifies the question, but does not resolve it. The mode of movement, once admitted, forcibly compels the abandonment of the idea of *fluid*, which always accompanied electric phenomena at the outset of their study.

If we are agreed to-day upon the immateriality of electricity, we are, on the other hand, far from understanding the nature of the special mode of movement which characterises electricity, the word being taken in its most general acceptation.

In his *Recherches sur l'électricité*, M. Gaston Planté expresses his ideas on this point in the following words: "Electricity can be regarded as a movement of *ponderable matter*—movement of *transportation* of a very small mass of matter incited with very great velocity, if the question is of the electric discharge, and a very rapid *vibratory* movement of the molecules of the matter, if the question is of its transmission to a distance under the dynamical form, or of its manifestation under the statical form at the surface of the body."

For some, who are much less precise in their definitions, electricity is produced by *molecular movements*, without otherwise determining its nature, characterised by form, direction, velocity, periodicity, &c.

In the experiments about to be described, M. Bjerknes proposed to throw light upon the question of the nature of molecular movements, by reproducing *mechanically*, but INVERSELY, simple and fundamental electric phenom ena

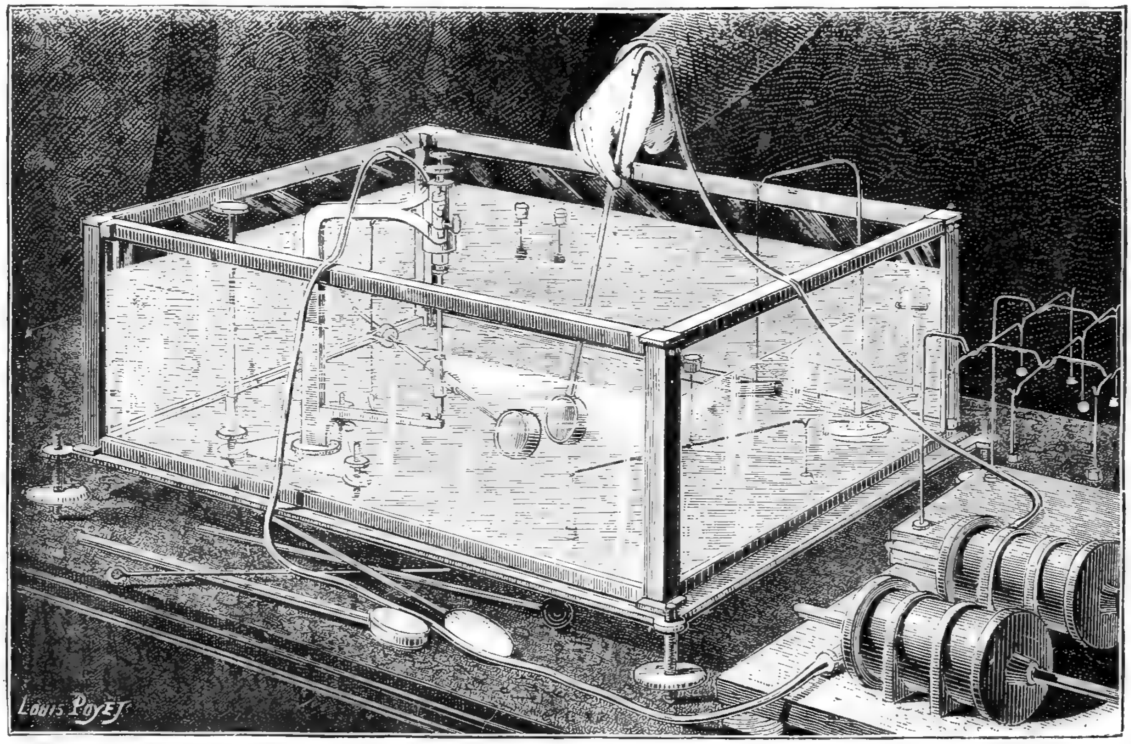

Fig. 1. Apparatus of M. Bjerknes at the Electrical Exposition.

M. Bjerknes designates under the general name of *vibration*, the movements which take, according to their nature, the name of *pulsation, oscillation*, etc.

Pulsation has reference to the change in volume. It includes two phases, one, in which the body swells, the other in which it shrinks. Pulsations are *synchronous* when the phases commence simultaneously.

Oscillation has reference to the change in place, it is an alternative displacement to the right and to the left.

M. Bjerknes mechanically obtains pulsations in water by the aid of a very ingenious apparatus.

The *pulsations* are produced by small cylinders stopped at their ends by flexible walls. A small hand pump which is partly shown on the right in fig. 1, is employed to exhaust and compress alternatively the air in the cylinders provided with flexible walls, at a great velocity.

In the simplest pulsator, the two walls dilate and contract at the same time under the action of forcing the air from the pumps, the phrases are synchronous (fig, 2. No. 1). In another arrangement, the two drums are separated by a rigid wall, which forms two chambers each in connection with a separate pipe conducting the air (fig. 2, No. 2). We have thus a most complete system, for by adjusting conveniently the tubes of the air-pump for exhausting and filling, synchronous pulsation can be produced at will, as in the first case, or pulsation in which the phases are alternate.

Oscillations are produced by means of small metallic spheres bound to supports, upon which they oscillate, under the action of compressed air, in a plane which varies with the position of the sphere.

Fig. 2 (No. 3) represents two of these oscillators, the sphere on the right oscillating vertically up and down; that on the left, on the other hand, oscillates horizontally from right to left.

This is the very simple and well constructed apparatus which M. Bjerknes employs. Now we come to the phenomena.

First two pulsators are taken and made to vibrate. The phase of dilation, according to M. Bjerknes, correspo ds

to the north pole, the phase of compressing to the south pole. Now bring to one of these pulsators which can turn freely around a vertical axis which acts as its support, while allowing the vibration to continue, a second pulsator held in the hand.

If we put in juxtaposition, in the liquid, the two pulsators whose phases are of the same kind, synchronous, poles of the same name will always be in juxtaposition, there will be *attraction*, the movable pulsator turning on its axis, will tend to approach the pulsator held in the hand by the experimenter, and it will follow it if it is moved. If the phases are changed, so that they are inverse, opposite poles will be together, and there will be *repulsion*. In the one case as in the other, the attractive or repulsive force is proportional to the intensity of the pulsations and inversely proportional to the square of the distances. In both cases, the hydro-dynamic effect is the reverse of the magnetic effect: similar phases attract (poles of the same name repel each other), different phases repel (opposite poles attract).

The same experiment is repeated with the oscillators (Fig, 2, No. 3); by presenting to a vibrating sphere, movable on an axis, a second vibrating sphere, attraction or repulsion is produced according to the synchronism or the discordance of vibration, which the parts of the sphere in juxtaposition present at each instant.

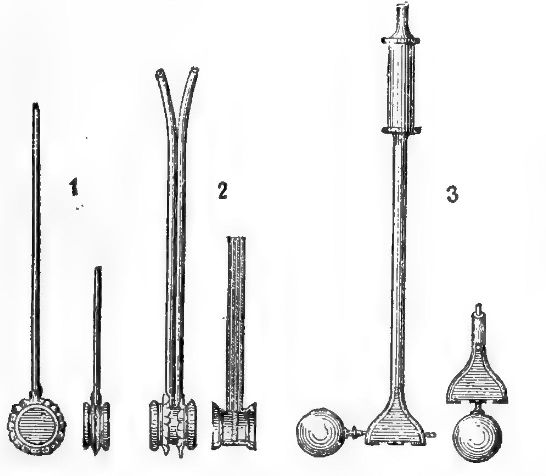

Fig. 2. Apparatus for reproducing pulsations and oscillations in a liquid.
1. Simple drum. 2. Double drum. 3. Sphe es.

For these phenomena, the arrangements for which can be varied, M. Bjerknes has a collection of apparatus almost complete, representing inverse analogies to the phenomena of the reciprocal action of two permanent magnets. A result similar to the action of a magnet on a piece of soft iron can be obtained. By presenting, in the water, a small metallic sphere to a pulsator, or to an oscillator, the small sphere will be attracted.

The effects of *diamagnetism* are shown by means of a small sphere lighter than the water, maintained at the middle of the liquid by a thread attached to a weight which ballasts it. By bringing a pulsator or an oscillator near this sphere, the latter will be repulsed.

From these experiments, and from others the details of which cannot be given, M. Bjerknes concludes that the motion in water of a vibrator (pulsator or oscillator) produces in this fluid a real magnetic field with its lines of force, presenting, *but always inversely*, phenomena similar to those of diamagnetism, paramagnetism, magnetic interference, etc.

M. Bjerknes has even succeeded in tracing the directions of the lines of force produced in the liquid, by means of the arrangement shown in fig. 3. For this a light bowl sustained by an elastic rod is placed in the middle of the liquid; this bowl having no motion of its own will take exactly the direction of the oscillation of the ambient medium. If it is surmounted by a small brush, the latter will paint faithfully and automatically on a sheet of glass the lines of force of the field under the influence of which it oscillates.

M. Bjerknes commenced by submitting all these questions to analysis, and the results of his experiments are only the rigorous confirmation of his calculations. In that which concerns the analogy between the electric currents and hydrodynamic action, M. Bjerknes recognized that the question is not as advanced as in magnetic phenomena.

In order to produce more complex movements, the vibrators are no longer suitable. M. Bjerknes attempted to realize them in a viscous liquid, and striking analogies were found between the lines produced by hydro-dynamic phenomena under these conditions and the lines obtained by real currents under corresponding conditions, but the results obtained are not accurate enough to enable one to form an opinion.

What now can be concluded from the experiments of M. Bjerknes? The fact indisputably established is as follows:

Mechanical vibrations produced in a liquid medium cause phenomena analogous, but *inverse*, to the magnetic

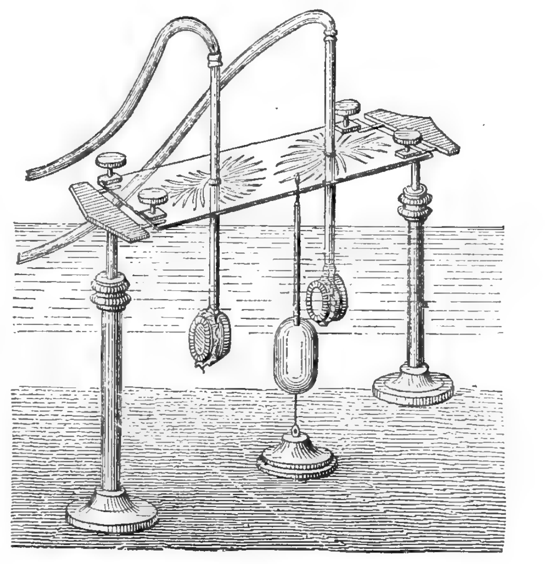

Fig. 3. Apparatus of M. Bjerknes for tracing automatically the lines of hydro-dynamic force.

phenomena produced by magnets. From this can be concluded, by *analogy*, but not *absolutely*, that molecular vibrations of a different nature can produce *direct* phenomena.

If this is not a new *proof*, in the exact meaning of the word, of the *vibratory nature* of magnetic and electric effects, it is at least a powerful argument applied to this view, accepted at the present time by most physicists.—*Translated from La Nature.*

M. PLATEAU describes as "*un petit amusement*" the following experiment:—a flower like a lily, with six petals each about an inch long, was constructed in outline in thin iron wire, the wire being first slightly peroxidised by dipping for an instant into nitric acid. This wire frame was then dipped into a glyceric-soap-solution, which, when it was withdrawn, left soap-films over the petals. The stalk was then set upright in a support, and it was covered by a bell-glass to protect it from air-currents. In a few moments the most beautiful colors made their appearance. If the solution is in good condition the films will last for hours, giving a perpetual play of color over the flower.

SHALER AND DAVIS' "GLACIERS."

By W. J. McGee.

(*Continued from page* 584).

VII. *Ancient glacial periods.*—Since the records of glacial phenomena are mainly such as are likely to be obliterated by succeeding geological mutations, it is needless to look for as unequivocal testimony of glacial periods as that attesting the occurrence of the last ice age. The principal evidences we can hope to obtain are (1) tide-washed boulders, gravel, and sand, (2) erratics dropped from icebergs in the deeper sea deposits, and (3) paucity or absence of organic remains, or possibly fossil forms suggesting low temperature; and it is to be expected that such evidence will become constantly less explicit as the geological record is traced backward.

Anterior to the Quaternary ice period, we first come upon evidence of a Miocene glacier, well marked in the hill of Superga, near Turin; from which evidence it may be inferred that the European continent underwent a very severe glaciation in Miocene times. Next follows the Eocene, in which the Flysch of Eastern Switzerland, formed of conglomerates containing immense boulders, supposed to be derived from worn-down mountains of the Vosges or Black Forest, eloquently attests vigorous ice-work; while "in North America we can almost mark the line of the ice by the limit of the destruction of the Tertiary beds" (p 95), and are hence without so characteristic ice deposits. The Cretaceous affords no evidence of glacial conditions save occasional iceberg-dropped boulders; but in the Jurassic are found the conglomerates of Northern Scotland and of the Connecticut Valley, which appear to be of glacial origin. The traces of Permian glaciers are unmistakable, and of almost world-wide extent, being found in Central England, in Scotland, in the Isle of Arron, in Ireland, in South Africa, and elsewhere. The rocks of Carboniferous age include conglomerates, probably of glacial origin, from Southern France to Scotland, from Alabama to New Brunswick, in India, and in other countries; from which it appears "that it extended ice action over a wider meridional range than any [periods] that have succeeded it" (p. 98). There are then no beds certainly attesting ice-work until the base of the Cambrian is reached, when the extensive Ocoee conglomerates and Chilhowee sandstones, and lesser deposits of like character at Roxbury, Mass., and elsewhere, are found. In many of the foregoing cases the associated beds indicate warm or subtropical climate, as should happen according to Croll's theory.

VIII. *The climatal conditions of the glacial periods.*—There is no reason to believe that ice periods were ever of particularly low temperature. On the contrary, the Quaternary fauna was characterized by the great size of its individuals, and clearly proves the contemporaneity of a luxuriant flora, such as could not exist in arctic cold. Moreover, there are abundant proofs, that this glacial period was a time of much greater rainfall than the present. Accordingly several glacial hypotheses may be summarily dismissed. That of Poisson is qute untenable. It is questionable whether that attributing climatal oscillation to variation in the constitution of the atmosphere should be admitted to have weight; though effects resulting from such a cause would be cumulative. Croll's theory is found to harmonize strikingly with the observed facts; There is the last period of high eccentricity occurring at the proper date for the Quaternary ice age; there are the numerous successive inter-glacial periods corresponding with the alternate advances and retreats of the ice; and there are the brief epochs of warm climate, represented by luxuriant floras and vigorous faunas. On the other hand, however, there are the objections that some minor factors may possibly have been overlooked in framing the theory; that there are vast ages without evidence of glacial action, as should not occur, according to the theory, since "the eccentricity of the earth's orbit is such a constantly recurring phenomena" (p. 107); that the Antarctic glaciers are not advancing, but that all observations "lead us to the conclusion that the ice there is as much in process of retreat as it is in the Northern Hemisphere" (p. 107); and that, the hypothesis assumes essentially the same outline for Cape St. Roque during the glacial periods as at present. But none of these objections are fatal to the theory; the only question is as to its being a sufficient cause of anything so wide-spread as the continental ice periods. There are also elements of probability in the hypothesis of Le Coq, that variations in solar emissivity might produce glaciation; for augmented temperature would increase precipitation and lead "to an extension of the fog envelope which in all glaciated regions does so much to protect the ice from the sun" (p. 108); but on the whole, though this hypothesis has the advantage of ndefiniteness, it is entitled to less weight than that of Croll. Neither must the hypothesis of minor geographical alterations be overlooked, since it is perhaps possible that such changes may have occurred in such manner as to facilitate glaciation. The question, however, remains an open one, "and it is unsafe for the geologist to commit himself definitely to any of the hypotheses that have been suggested" (p. 110). There are half a dozen distinct and powerful causes, together with a number of minor factors, which co-operate to produce the singular uniformity of terrestrial temperature; and the only safe conclusion is that the earth's secular winters may be due partly or wholly to any or all these agencies.

IX. *Effect of glaciers on the altitude of the lands.*—Throughout northern regions there are evidences of considerable depression of the land during the glacial period; which depression in Europe was variable, and mainly confined to the severely glaciated area. "In America this depression has not been studied except along the Atlantic shore" (p. 113), where it increases from twenty feet near the southern limit of glaciation, to over two thousand feet in Greenland. The land appears to have remained below the normal level until after the withdrawal of the ice. Two hypotheses have been framed to account for this depression:—1st. That of Adhemer, which attributes the phenomenon to the dislocation of the earth's centre of gravity by a polar ice-cap, and which is based on the assumption of a rigid terrestrial crust. The depressions at various latitudes were not, however, of the relative value demanded by this hypothesis. 2nd. That of local deformation of a flexible terrestrial crust beneath the weight of the ice. This hypothesis is supported by the greater part of the evidence thus far collected; though it is likely that both classes of agencies co-operated in producing the effect. There is reason to believe, also, that a temporary upheaval of lands south of the ice-sheet occurred during the Quaternary; which upheaval was doubtless a concomitant of the local depression beneath the ice.

X. *The effect of glaciation on the life of the earth.*—During the growth of the ice-sheets there must have been a widely extending southerly migration of animals and plants, giving rise to individual and specific variation in consequence, not only of the change of habitat, but also of the crowding of individuals over the contracted habitable area; and the converse movements following the withdrawal of the ice must have been nearly as important biologically. The development and extinction of the hairy mammoth affords an illustration of the effects of secular winter on animal life. "In the closing stages of the glacial period we find him the most widely disseminated of all the large mammals that are known to us" (p. 119); his remains occurring alike over Europe, Asia, and America. "As we go back into the glacial time, we have fewer and fewer indications of the existence of this noble beast, yet we have remains enough to make out that he or his immediate ancestors existed at the beginning of that epoch, and that in all its stages he was feeding

in the rich forests that seemed to have flourished close to the walls of ice" (p. 119). In Europe, and perhaps in Asia and America he was a contemporary of man. In Asia, he dwelt in vast numbers on the plains of Siberia. "When he abounded there, the climate was * * * as cold as it is at present. The rivers in that country have their sources farther to the south than their main streams, so that the springtime sends down a torrrent of water before the more northern channels are released from their wintry bonds; the elephants seem to have herded together along these streams for winter quarters, * * * and to have been swept away to the north by the inundations. These freshets carried their bodies to latitudes where the cold was so great that they were frozen in the mud that wrapped them round and covered them to such a depth that the brief summer-times never melted their icy casing" (pp. 119-20). Some of these bodies remained undecomposed up to the time of their discovery in recent years; and their tusks yet occur in such numbers as to be of commercial importance. The low temperature under which the mammoth existed is attested not only by his hairy covering, but by the coniferous vegetation upon which he, with his congeners, appears to have subsisted. Whether his final extinction was accomplished by human agency is a question; but it is little less than certain that he entered the glacial era with man, was hunted in Europe, Asia, and America by paleolithic savages, and survived until the amelioration of the glacial climate.

XI. *Relation of glaciation to the history of man.*—The evidence concerning man's relation to the glacial period is divisible into two categories:—(1) that which connects him with the closing stages of that epoch; (2) that which establishes his existence previous to the advent of the ice. The evidence belonging to the first category is overwhelming in quantity; it mainly consists in the finding of human bones and products of art associated with the remains of glacial animals or imbedded in later glacial deposits. That belonging to the second class is much more meagre, and has been obtained satisfactorily in only three localities, viz.; central France, California, and New Jersey. In the first of these localities a human cranium was discovered in volcanic tufa beneath a sheet of lava, associated with a fauna whose general facies is ancient, though not sufficiently definite to establish the pre-glacial existence of man paleontologically. The pre-glacial age of all these remains may be, however, inferred from the evidence furnished by the sub-ærial erosion of the valley of Le Puy and the glacial erosion of the adjacent mountains of Coutal; though the testimony can hardly be regarded as conclusive. The finding of a human cranium in auriferous gravels overlain by extensive lava-beds probably of pre-glacial age in California, associated with organic remains of rather more southern type than those of Le Puy, as attested by Whitney, affords more satisfactory evidence of the pre-glacial existence of man. Along the Delaware river in New Jersey numerous rough-stone implements have been collected by Abbott from a table drift, or mass of re-arranged glacial matter, which is destitute of organic remains. "From a rather incomplete study of the ground, the only view I could take of these remains was that they were scattered on the surface of the earth to the northward before the last glacial period; that they were thrust before the glacier during its period of greatest extension, and deposited in the beds where they now lie by the action of water, while the above underwent a slight submergence" (p. 134). These chipped flints of the Delaware, no less than the Le Puy and Calaveras eronia, indicate that even at this early day man had attained a social condition similar to that of the European stone age; and hence that during the vast intervening period, the duration of which was probably not less than 200,000 years, or forty times the term of recorded history, there was almost no progress in the latterly rapid process of intellectual development.

XII. *The movement of glaciers.*—"It is to DE SAUSSURE that we owe the first hypothesis concerning glacial motion" (p. 140); his view being embraced in the suggestion that the ice slid bodily downward in a solid mass, the sliding being facilitated by melting of the basal portion of the ice through the influence of proper terrestrial heat. The utter inadequacy of this hypothesis must, however, have been apparent to its talented author;—indeed—"we are forced to believe that this statement does not represent his conclusions" (p. 140). Charpentier subsequently suggested that the motion is due to the nightly freezing and expansion of the water taken into the interstices of the ice during the day; but this hypothesis is also inadequate. Still later the solution of the problem was undertaken by AGASSIZ, who devised a critical series of experiments to determine the empirical laws of motion of the glaciers. The plan was to plant a line of stakes across the ice-stream, and to measure their absolute and relative movement at the end of a year. During the first season the entire series was overthrown by the superficial melting of the ice; but the stakes were again more firmly planted. Among the naturalists who visited his camp on the glacier was J. D. FORBES, whose contributions to glacial physics are well known. "While the guest of Agassiz, Professor Forbes made his first acquaintance with existing glaciers. Owing to his superior training in the branches of learning that this peculiar problem called for, he soon saw that the method that Agassiz was using was, by a slight modification, capable of a more speedy solution than his Swiss host could obtain under the conditions of his experiment. Agassiz planted a row of stakes across the glacier, but proposed to wait, with the patience that characterized his mind, until after a winter, to read the answer he sought. Mr. Forbes saw that with a transit or theodolite he could, in a few days at most, see how the stakes were moved, and so anticipate the results his host was seeking. With this plan in mind he went to the Mer de Glace, set up a line of stakes in the precise position devised by Agassiz, and within a month proved that the ice moves most rapidly in its middle parts, and not, as had been supposed, more quickly upon the sides of the stream; this result he hastened to make public" (p. 142). These, as well as later observations, show the laws of motion of glaciers correspond to those of moving liquids. Somewhat previously RENDA had reached a similar conclusion. FORBES soon after enunciated the viscous theory of ice-motion, illustrating "his conception of glacial movement by frequent reference to other substances, the the viscosity of which we recognize in ordinary experience, such as tar, wax, or molasses" (pp. 143-4). "In the hands of his followers this theory has sometimes assumed a different shape * * *; it is then made to mean that the ultimate tangible elements of the glacier, the bits of ice into which it is divided, slide over each other, as, for instance a heap of peas when poured on a sloping surface" (p. 144). This captivating hypothesis has not, however, been widely adopted. Next followed the fracture-and-vegetation hypothesis of Tyndall; but neither has this view commanded general assent. Still later Croll enunciated the hypothesis of successive melting and freezing of the molecules of the ice; "each molecule, as it is melted, parting with its heat to its neighbor on the inward side of the ice, and returning to the solid state, shortly to be remelted by the heat transmitted by its outer neighbor" (p. 145). There are certain phenomena, however, which this inherently probable hypothesis fails to explain. There is then the view in which the motion is referred to momentary melting, through the influence of pressure, of the particles of ice from time to time subjected to unusual strain; but, like the last, this hypothesis is alone incompetent to explain all the phenomena of ice-motion. Finally there is the sliding theory of Hopkins, which is in conflict with all we know of glacial movement. Examples of glacier motion are furnished by the annual snows of New England hill-sides; and the energy of this motion is well illustrated by a phenomenon observed

on a terrace in a cemetery at Augusta, Me. "On this terrace snow accumulated one winter so as to fill up the re-entrant angle it formed with the hill-side. When in the spring this snow melted away, it was found that the upright tombstones and the iron fence that surrounded the graves were broken off near the surface of the ground, and moved in the direction of the general slope of the hill" (p. 148). Summing up the foregoing hypotheses in their application to glaciers of the alpine type, and viewing them in the light of the various phenomena recorded, it appears that all except the sliding and viscous theories are "true causes" of ice-motion; and though neither is alone competent to explain the various phases of the movement, all must be recognized in a satisfactory and consistent theory. As in the theory framed to account for glacial periods, so in this case also, our theory is sufficiently flexible and indefinite to be sure of coinciding with the truth in some of its aspects.

"The problem with which we have to deal when we come to the task of explaining continental glaciers is of quite a different nature" (p. 151), since in this case "we have to leave gravity, as it works in Swiss glaciers, almost out of account" (p. 154). The ice might move freely for a score of miles from its southern border; but in the interior little if any motion would probably occur, except such as might result from pressure-melting; the excess of water formed in this manner escaping in sub-glacial streams. "In this way we may conceive that the ice of British America may have been carried out, from centre to periphery, in the form of water, and the waste of its grinding borne along by the streams that were formed by the pressure-melted water" (p. 159). In other words, continental glaciers appear not to move as ice, but only as water, except along their extreme peripheries.

XIII. *Certain effects of glaciers.*—Outside of glaciated regions the soils and sub-soils are the products of simple weathering conjoined with vegetal action, and may be denominated *soils of immediate derivation*: while within such regions the superficial accumulations are made up of mechanically comminuted materials brought thither from numberless localities, some perhaps hundreds of miles distant, and may be termed *soils of remote derivation*. Since the soils of the first class vary with the character of the underlying rocks, it follows that the latter are more uniform in constitution over considerable areas; and they are at the same time more durable, for not only are the materials essentially identical in all parts of the thickness of the deposit, but they also contain desirable mineral constituents locked up in the included pebbles to be gradually liberated by atmospheric and chemico-vegetal action. The glacial clays were "laid down in a very unoxidized state. Generally they are of a bluish hue, and only attain the ordinary yellowish or reddish color of decomposed clays as the waters acidulated by vegetation slowly penetrate into them." "In North America this penetration of atmospheric decay is distinctly proportionate to the nearness of the clays to the old glacial front" (p. 165); and hence affords a rough measure of the length of post-glacial time.

The coarser moraine debris constitutes, however, but a small part of the glacial waste;—the impalpable glacial mud must have been formed in scores if not hundreds of times its volume, and swept for the most part into the sea to build up azoic shales and clay slates, perhaps intercolated with conglomerates, as in the Roxbury conglomerate series near Boston. This fine glacial detritus, whether accumulated in river-beds, lake-bottoms, or unfossiliferous marine formations of any age, may be distinguished from river salt by its unoxidized state and blue color. The distribution of both the coarser and the finer glacial products has been largely accomplished by marine forces, after they were thrust by the ice into the sea; as in the case of the Tertiary sands of the southern states, which were probably originally brought to the ocean by glaciation along the more northerly Atlantic coast.

The formation of auriferous gravels and the general accumulation of gold in drift by glacial action, was accomplished by the simple concentration of heavier materials in depressions or gentle slopes, just as occurs in miniature in a miner's pan or rocker; and the result may be brought about by local as well as general glaciation, as is well shown in the valley of the Arkansas River at Twin Lakes.

"It is a very important fact that no pre-glacial caverns have ever been discovered" (p. 170)—a fact which leads to the inference that the extent of glacial erosion was so great as to totally remove pre-existing cavern-bearing limestone strata. The excavation of fiords and lake-basins, already adverted to, is a farther illustration of the enormous extent of this erosion. A most interesting, though indirect, result of this property of glacier ice is the influence which it has exercised on the social condition of mankind; for it is only shores indented by bays, fiords, and inlets, and fringed with islands, that afford the incentives to and facilities for the development of the maritime industries which occupy so important a place in human progress. The far-seeing geologist cannot, however, avoid speculating on the possibility that a contrary effect may, in the distant future, be exerted on mankind, by the return of glacial conditions to our globe. There is every probability, indeed, that the earth will again be enfolded in an icy mouth such as crept over it during the Quaternary; though there is no reason to fear that such an untoward vicissitude is imminent.

A glossary of some fifty terms, a bibliography of nearly seven hundred entries, and a four-page index with the plates and descriptions follow.

It has been the aim to present, in the foregoing paragraphs, a full synoptical *resumé* of the work considered, without approval or comment. Several passages which are regarded as either erroneous or misleading, or open to serious objection on well-established theoretical grounds, have, however, been quoted in the words of the author. A portion of these may be noticed in their order; reference being made to the pages on which they occur.

P. 28.—Geikie[1] regards ice-bergs as the terminal portions of glaciers, broken off by their buoyancy on entering the sea; Tyndall,[2] however, supposes that the masses break downward by their own weight; while Schwatka[3] has shown that they are formed in either mode according to the temperature of the sea—and, he might have added, other circumstances.

P. 31.—"From information derived from all sources up to the present time, it may be gathered that the unpenetrated area of about 4,700,000 square miles surrounding the South Pole is by no means certainly a continuous 'Antarctic Continent,' but that it consists much more probably partly of comparatively low continental land, and partly of a congeries of continental (not oceanic) islands, bridged between and combined, and covered to a depth of about 1,400 feet, by a continuous ice-cap; with here and there somewhat elevated continental chains, such as the groups of land between 55° and 95° W., including Peter the Great Island and Alexander Land, discovered by Billingshausen in 1821, Graham Land and Adelaide Island, discovered by Biscoe in 1832, and Louis Philippe Land by D'Urville in 1838, and at least one majestic modern volcanic range discovered by Ross in 1841 and 1842, stretching from Balleny Island to a latitude of 78° S., and rising to a height of 15,000 feet."[4]

Pp. 38, 98.—Geologists generally do not consider that the correctness of the views of Ramsey and others as to the repeated recurrence of glacial periods throughout ge-

[1] "Great Ice Age," Am. ed., 1877, p. 55.

[2] "Forms of Water," 1877, p. 134.

[3] *Science*, vol. ii, No. 30, 1881, p. 31.

[4] Sir Wyville Thomson, in addresses before the Geographical Section of the British Association, Dublin meeting. Brit. Assoc. Rep., 1878, p. 619; *Nature*, Aug. 22, 1878; *Am. Jour. Sci.*, vol. xvi., 1878, pp. 355–6.

ological time, is fully established; and they certainly do not regard the covering by ice of "a very large part of land and sea" as a "fact," or indeed as more than an extremely vague hypothesis.

P. 40.—Croll long since suggested[5] that Caithness and the Orkneys were glaciated by the Scandinavian ice-sheet, and Peach and Horne have recently urged[6] that during a part of the glacial period the Shetlands were overspread by the Scandinavian *mer de glace*, while during another part they gave origin to a system of local glaciers, though Milne-Home[7] seriously questions these several conclusions; and it has also been suggested by Reid,[8] though it can hardly be regarded as established, that the contortion of the drift along the Norwich coast was effected by Scandinavian land-ice. As shown by Croll's[9] and Geikie's,[10] as well as all other reliable maps, however, all the British highlands were unquestionably independent centers of glaciation, quite distinct from the Scandinavian ice-sheet;—indeed, when Milne-Home characterized the statement that "the land-ice which glaciated Scotland could only have come from Scandinavia" as an "astounding declaration,"[11] Peach and Horne hastened to explain that the word *Scotland* was a mis-print for *Shetland*.[12] There are also grave reasons to question whether a polar ice-cap ever existed in the northern hemisphere, as the writer has endeavored to prove,[13] and it is quite certain that if such an ice-cap did exist it did not extend to the British Isles by way of Scandinavia. So long ago as 1845 Murchison showed by means of a map in his "Geology of Russia and the Ural Mountains," that the Scandinavian drift "proceeded eccentrically from a common centre;"[14] and Geikie (illustrating his remarks by a map) says: "the direction of the glaciation in the extreme north of Scandinavia, the peninsula of Kola, and north-eastern Finland, demonstrates that the great *mer de glace* radiated outwards from the high grounds of Norway and Sweden, flowing north and northeast into the Arctic Ocean and east into the White Sea, and thus clearly proving [proves?] that northern Europe was not overflowed by a vast ice-cap creeping outwards from the North Pole, as some geologists have supposed."[15]

Pp. 41,44.—It is not known that a continuous ice-front ever stretched across the American continent, since a portion of the region eastward of the Cordilleras remains unexplored. Dana remarks, "since evidence of the great southward moving glacier fail over the region west of a line passing from a few degrees west of Winnipeg, southeastward through Western Minnesota and Iowa, near the meridians of 98°–100°, and all the way westward to the borders of California and Oregon if not to the Pacific coast, the ice thinned out toward the interior of the continent and was mostly absent except about the higher parts of the Rocky Mountains."[16] There is moreover no sufficient reason for believing that the American ice-sheet swept down from polar regions. Hitchcock's map of directions of ice-flow, which is reproduced in the work under consideration (following pl. XX.), indicates that the principal American center of dispersion was probably in the northern Laurentian Highlands;—a view which is corroborated by more recent observations of the Canadian Geological Survey.[17] Houghton has also shown that the boulders of the Arctic archipelago were carried northward.[18]

P. 42.—The quoted remarks describing the southern limit of ice-action in the Mississippi valley display unpardonable disregard of the work of Cox and Collett in Indiana, of Worthen and his associates in Illinois, of the several Missouri geologists, of White and St. John in Iowa, of Aughey in Nebraska, of N. H. Winchell and Upham in Minnesota, and even of Hitchcock's map appearing in the volume. The southern limit of the drift does not pass through either Iowa or Minnesota, but through southern Indiana and Illinois, northern-central Missouri, and Nebraska. The statement possibly originated in confounding the great Kettle Moraine with the southern drift-line, though as specifically pointed out by Upham,[19] and hardly less distinctly by Chamberlin,[20] these lines are 300 miles apart.

The glacial phenomena of south-western British America have been carefully and tolerably fully studied by G. M. Dawson.[21]

Pp. 66, 68.—An explanation of the origin of kames and aasor which is satisfactory to students of glacial phenomena was independently offered by N. H. Winchell,[22] Holst,[23] and Upham.[24] The hypothesis is as definite and probable as almost any other portion of the glacial theory.

Pp. 70, 90.—Tyndall has shown[25] that a diminution of terrestrial temperature could never inaugurate a glacial epoch; it has never been demonstrated that augmentation of temperature would be in any degree likely to produce a similar effect; and, accordingly, Poisson's and Le Coq's hypotheses can hardly be regarded as "reasonable" or "likely" in the present state of knowledge. As Croll has urged[26], there are grave reasons for questioning whether anything like half of the heat reaching the earth comes from the stars.

P. 75.—Loomis, describing the orbital motion of planets undisturbed by exterior forces, says: "the curve cannot be a circle unless the body be projected in a [particular] direction * * *, and, moreover, unless the velocity * * * is neither greater or less than one particular velocity;"[27] and Stockwell, speaking of the solar system in its actual condition, says: "the eccentricity of the earth's orbit will always be included within the limits of 0 and 0.0693888."[28]

P. 95.—Sound bases for the assertion do not sppear. Hilgard says[29]: "the relations mentioned by Tuomey (Second Report on the Geology of Alabama, p. 146) as existing between the shore of the Tertiary sea and the region of occurrence of the southern [northern?] drift on the Atlantic slope, are not so clearly recognizable in Mississippi and Alabama;" and further westward no relation whatever is apparent. Furthermore the "northern drift" of Hilgard and Tuomey was deposited far beyond the southern limit of the ice.

P. 107.—Le Conte shows graphically[30] that, admitting the influence of eccentricity, glaciation would only be possible after protracted secular refrigeration. The period in which this refrigeration reached such a degree as to permit of glaciation has not been determined. If in the Cambrian, Le Conte's diagram is, of course, incorrect; but it is not seriously maintained by working geologists in America that such was the case. Speaking of the Quaternary Le Conte, referring to Nordenskjold's observations,[31] declares[32]: "that glacial conditions were

[5] *Geol. Mag.* May and June, 1870; "Climate and Time," chap. XXVII.

[6] *Quar. Journ. Geol. Soc.*, vol. XXXV, p. 778; *Geol. Mag.*, II., vol. VIII, p. 65; *ibid*, p. 364.

[7] *Trans. Edin. Geol. Soc.* vol. III. pt. 3, p. 357; *Geol. Mag.*, II., vol. VIII., p. 205; *ibid*, p. 449.

[8] *Geol. Mag.*, II, vol. VII, p.——(A number of the writer's volumes are in the bindery at this writing, and a few references accordingly cannot be given in full.)

[9] "Climate and Time," p. 449.

[10] Accompanying "Great Ice Age."

[11] *Q. J. G. S.*, XXXV, p. 809.

[12] *Geol. Mag.*, VIII, p. 69.

[13] *Proc. A A. A. S.*, vol. XXIX, p——.

[14] Cited by Lyell, "*Antiquity of Man*," 4th revised ed., 1873, p. 275, note.

[15] "Great Ice Age," p. 354.

[16] "Manual of Geology," 3d. ed. 1880, p. 537.

[17] Geol. Surv. Con. Rep. Prog. for——, p——; *ibid.* for——, p——.

[18] McClintock's "In the Arctic Seas," author's ed., appendix, p. 368.

[19] 8th Ann. Rep. Geol. and Nat. Hist. Surv. Minn., p. 73.

[20] Geol. Wis., vol. ii., 1877, pp. 205–15; *Trans. Wis. Acad.* of Arts, Sciences, and Letters, vol. —, p. —.

[21] *Quar. Jour. Geol. Soc.*, vol. xxxiv., pt. 1; Geol. Surv. Con., Rep. Prog. for—, p. —; *ibid.* for —, p. —.

[22] *Proc. A. A. A. S.*, vol. —, p. —.

ever before reached, even in polar regions, seems more than doubtful."

The foundation for the second statement quoted does not appear.

P. 108.—It has not been empirically established that the effect of fog-banks is to diminish temperature; and analogy with the known observation of finely disseminated water suggests that a directly opposite result ought to be produced. Croll's argument has been considered by Newcomb [33] and the writer.[34]

P. 113.—G. M. Dawson[35] has investigated the late-Quaternary depression of Vancouver's Island and British Columbia, and finds it to have been practically commensurate with that of the Atlantic coast.

P. 119.—Geologists are not united as to the age of the great pachyderms. Thus, Collett[36] records observations indicating that they were recent; while Phillips[37] and Godwin- Austen[38] regarded them as wholly pre-glacial, and Hall[39] and Belt[40] have shown that at least some individuals existed before the advent of the ice.

PP. 119-20.—Howorth has recently (mainly since the publication of "Glaciers") examined the evidence relating to the former existence of the mammoth in Siberia, and reaches the conclusion (among others). 1st, that the animals lived where their remains now lie; and 2nd, that the climate was comparatively mild at that period[41].

P. 134 A subsequent and apparently complete study of the locality by Lewis leads to the conclusions; 1st, that the implements are confined to the Trenton River gravel: and 2nd, that this gravel was deposited "at a period immediately following the last glacial epoch." [42]. If, as suggested by F. W. Putnam, [43] the flints were dropped into this gravel while in process of formation by the paleolithic men who hunted and fished along the old river-bluffs of New Jersey, it follows that these men were post-glacial; and even if the correctness of Lewis's views are not fully established, as intimated by Dana,[44] this instance does not demonstrate, or even indicate, man's pre-glacial existence.

P. 140.—Tyndall mentions[45] that Scheuchter first propounded the dilatation theory in 1705, that in 1760, or nearly forty years in advance of De Saussure, Altman and Grüner enunciated the sliding theory, and that in 1773, or thirty years before the publication of De Saussure's "Voyages dans les Alpes," the plastic theory was put forth by Bordier.

P. 142.—The re-opening, not incidentally or even judicially, but in a ludicrously partisan tone, of this now almost forgotten though erstwhile bitter controversy, would be quite unjustifiable even if the statements where not erroneous. Forbes' first visit to the alpine glaciers (as published by himself in that year), was on the 9th. of August, 1841; [46] during which visit he was in the company of Agassiz. Throughout this season his observations, as indicated by his published results, were confined to superficial phenomena; chiefly "ribboned structure" and "slaty cleavage." On the 24th of June, 1842, he again reached Montannent[47] with a set of instruments of precision, avowedly and obviously carried thither for the express purpose of instituting a series of measurements of the motion of the ice—the necessity for such measurements having been pointed out in lectures in December, 1841, and January, 1842,[48] and also in the *Edinburg Review* for April, 1842[49];—and the "First Letter on Glaciers," containing the "account of the first experiments, undertaken in June, 1842, to determine the laws of motion of the Mer de Glace of Chamouni" (published in October as already noted), was dated "4th July, 1842"; on which very day as stated by Dana on Tyndall's authority,[50] Agassiz' measurements proving the more rapid flow of the medial portion of the glacier, were published in the *Comptes Rendus*. Lyell, speaking of the more rapid medial than lateral motion of glaciers, says [51]; Mr. Agassiz, at p. 462 [of the "Systeme Glaciere"], states that he published in the *Deutche Vierteljahrschrift* for 1841, this result as to the central motion being greater than that of the sides, and was, therefore, the first to correct his own previous mistake." Comment is unnecessary.

Pp. 143-4.—Substances not previously regarded as viscous, as for instance Stockholm pitch "so hard as to be fragile throughout, and present angular fragments with a conchoidal fracture" and a glassy lustre[52], are also referred to by Forbes.

The "followers" who hold the view indicated are not advocates of the viscous theory proper, which is essentially molecular. The motion of viscous, as of fluid bodies, may, however, be very imperfectly *illustrated* by the movement of a heap of independent spherical masses.

P. 145.—The re-statement of this view, which it is painful for admirers of Croll's important labors in other directions to discuss, is hardly excusable. Readers may satisfy themselves as to its validity by referring to the criticisms of Blakie[53] and Teal[54]. The authors should have pointed out the differences between solid, liquid, and gaseous molecules of H_2O.

P. 148.—The motion of the alternately freezing and thawing snow unquestionably occurred in the manner assumed in the dilatation theory as advocated by Scheuchzer, Charpeutier, and, especially Mosely; but it is just as unquestionably distinct from the true flow of glacier ice. The recognition of miniature glaciers in the New England snows, far transcends the peculiar ideas of Muir, which are so strongly deprecated by King[55].

Pp. 151, 154, 159.—The extraordinary conclusions reached are perhaps to be attributed to the inadequacy of the theory of glacier motion adopted. The statements may be looked upon as representing unduly emphasized ideas of a purely speculative nature.

P. 165.—So long as a majority of leading students of Quaternary phenomena classify the upper and generally yellowish portion and the lower and generally bluish portion of the drift respectively as Upper Till and Lower Till, and look upon them as distinct in either time or mode of formation, as do Newberry, Upham, Hitchcock, Aughey, N. H. Winchell, Stone, and many others in the United States, and so long as these deposits are distinctly separated by a characteristic vegetal stratum, as they are at least in southern Ohio,[56] northeastern Iowa,[57] and Nebraska,[58] the first quoted statement must be regarded as unsupported by facts, notwithstanding the possibility that atmospheric and vegetal action might, as urged by Hawes,[59] Julien,[60] and Van den Bröck,[61] produce a similar discoloration of a single homogeneous formation having the constitution of the Lower Till; and since the bluish clays quite frequently (and indeed over some considerable

[23] *Geol. Fören. Stockholm Förh.*, Bd. iii., No. 3, pp. 97-112.

[24] *Am. Jour. Sci.*, Lec., 1877; *Proc. A. A. A. S.*, vol. xxv., *p.* 216, *et req.;* and vol. iii. of the late New Hampshire reports.

[25] Heat as a Mode of Motion," Am. ed., p. 176; "Forms of Water,' p. 154.

[26] "Climate and Time," p. 39. Newcomb says (*Am. Journ, Sci.*, Vol. XI., 1876, p. 263)—"Practically there is but one source from which the surface of the earth receives heat, the sun, since the quantity received from all other sources is quite insignificant in comparison."

[27] "Treatise on Astronomy," 1876, p. 138.

[28] "Secular Variations of the Orbits of the Eight Principal Planets." *Smithsonian Contributions*, No. 232, 1872, p. XI.

[29] Geol. and Agricult. Miss., 1860, p. 28.

[30] "Elements of Geolo " 1879, p. 550.

[31] *Geol. Mag*,, Nov., 1875, p. 525.

[32] "Elements," p. 549.

33 *Am. Jour. Sci.*, vol. xi, 1876, p. 272.

34 *Popular Science Monthly*, vol. xvi, 1880, p. 816.

[35] *Vide*, note 21.

[36] Ind. Rep. of the Bureau of Statistics and Geol., 1880, pp. 384-6.

[37] "Geology of Yorkshire," 1829, vol. I, pp. 18, 52. cited by Belt, *infra*.

[38] *Reports British Assn.*, 1863, p. 68.

[39] 21st. Regent's Rep. on N. Y. State Cabinet, 1871, p. 103, *et req.*

[40] *Popular Science Monthly*, vcl. XII, 1878, p. 62.

[41] In an unfinished series of papers in vols. VII. and VIII, of the *Geol. Mag.*

areas generally) approach or even reach the surface toward the southern limit of ice-action, as in southern Indiana,[62] Illinois,[63] Iowa,[64] and Nebraska,[65] and in northern Missouri,[66] the second statement must be viewed in a similar light.

P. 170.— The cave-fauna, especially in Europe, is essentially identical with that commonly regarded as Quaternary or Champlain; and as already intimated, there is good reason to suspect that this fauna was at least partially pre-glacial. Moreover, the same facts relied on by the elder Buckland to establish the anti-diluvian age of the ossiferous cave-deposits,[67] must today be considered equally conclusive of the pre-glacial existence of these remains.

On passing to a more general survey of the work there is found to be less occasion for criticism, and indeed some grounds for high commendation. Thus, the influence of aqueous vapor and other substances diffused in the atmosphere upon terrestrial temperature is rarely lost sight of, the existing glaciers are faithfully and accurately described, and the general character and effects of Quaternary glaciation are fully and clearly dealt with. The teachings as regards the recurrence of glacial epochs and their influence upon the successive geological formations of the globe are, however intensely radical. The evidence relied on to demonstrate the glacial origin of sandstones and conglomerates, azoic shales and slates, and unoxidized argillaceous rocks generally, is quite inadequate. For instance, the idea that the iron of glacial mud alone is normally unoxidized while that of river silts is normally oxidized, appears to be expressly contradicted by the facts that the iron of the Lörs (which is almost certainly formed of impalpable glacial debris), is nearly universally peroxide, even when the deposit is one or two hundred feet thick, while that of the post Lörs alluvium of the lower Mississippi is invariably protoxide, even within a few feet or even inches from the surface; and the opinion that paleozoic ice-action cannot be proven by the same evidence as that attesting Quaternary glaciation is directly opposed by the facts that the Talchir (lowest paleozoic) beds of Peninsular India contain striated and polished boulders imbedded in the finest silt, and that the underlying Vindhyau (azoic) rocks bear similar markings; though even in this case the original observers do not refer the phenomena, with any degree of certainty, to glacial action. Again, while the discussion of glacial hypotheses and theories of glacial movement is tolerably full and (unless possibly in a single instance) eminently impartial and candid, the "composite theories" adopted are quite valueless, since the detailed investigation given to the subjects is in almost every case much less searching and exhaustive than that upon which each hypothesis was originally based. It may be questioned, indeed, whether the method of throwing together a number of essentially distinct and imperfectly weighed hypotheses, and taking the sum or the mean of all as the only consistent theory, will ever come into general repute, however strongly it may be supported by a confusing array of glittering generalities. Thus, with regard to the problem of ice-motion; it is of course true that the dilatation, fracture-and-vegetation, and pressure-melting hypotheses are based on the observed behavior of ice; but it does not necessarily follow that these properties, either individually or collectively, produce the phenomenon of flowing in large bodies of ice. To illustrate:—solids generally expand and contract with alterations in temperature; they may be fractured by irregular strain or impact; they may be united into homogeneous bodies by pressure as shown by Spring; and those which expand on solidifying may be melted by pressure; yet no physicist attributes the flow of solids (which has been investigated by Tresca, Roberts, Ware, and others) to any or all of these properties. Finally, there are important omissions, notably with respect to the widespread hipartite structure of the drift in many if not most glaciated regions, which has led many European and several American geologists to conclude that it was formed during two distinct periods, separated by a considerable era of mild climate. The general neglect of the results of American (and indeed Foreign) study has been incidentally noted in preceding paragraphs.

The illustrations, which are of the finest character and elaborately described, are mainly reproduced from photographs of existing glaciers and of interesting phases of glacial phenomena in America and elsewhere. They are of course open to the objection which may be urged against all photographs of natural scenery—*i. e.*, that the most instructive and valuable details are often obscured or concealed;—though many of the plates are of remarkable clearness and beauty. The illustrations are not, however, superior or even quite equal to many which have already been published by Agassiz (for instance, in the atlas accompanying "*Etudes sur les Glaciers*;" Neuchatel, 1840—"Untersuchungen über die Gletcher," Solothurn, 1841), and others.

It should be added that the necessity for all the foregoing criticisms appears to have arisen from the peculiar design of the work and the circumstances under which it was prepared. The authors explain in the preface that in order to meet the agreement with the publishers, it was necessary to prepare the text with far greater haste than was desirable; and remark that,—"if the reader will consider that the main object in the book is not to afford a complete history of glaciation, but to present a body of graphic illustrations of glacial phenomena, and that the text is designedly subordinate to this purpose, he will then better understand the apparent short-comings of the work."

Viewed as a whole, it appears that the work describes no new phenomena and presents no new theoretical views, while it exhibits many deficiencies and inaccuracies. It cannot therefore be regarded as in any sense a valuable contribution to the subject dealt with, or even as a satisfactory exposition of the present state of that subject. To the working student it will accordingly be worse than useless, since it will impose upon him a heavy financial and

[42] "Primitive Industry," Abbott, 1881, pp. 541, 551.

[43] 14th Ann. Rep., Peabody Museum, 1881, p. 23.

[44] *Am. Jour. Sci.*, vol. XXII, 1881, p. 402.

[45] "Forms of Water," pp. 155-7.

[46] *Edin. New Phil. Journ.*, Jan. 1842; "Occasional Papers on the Theory of Glaciers," 1859, p. 3.

[47] *Edin. N. P. J.*, Oct., 1842; Occ. Papers, p. 9.

[48] Occ. Papers, p. 10.

[49] "The solution of this important problem [the theory of glacier motion], would be obtained by the correct measurement, at successive periods, of the spaces between points marked on insulated boulders on the glacier; or between the heads of pegs of considerable length, stuck into the matter of the ice, and by the determination of their annual progress." *Op. cit.*, p. 77; Occ. Papers, p. 10.

[50] "Manual of Geology," 1880, p. 694.

[51] "Principles of Geology," revised ed., 1854, p. 224, note.

[52] Occ. Papers, pp. 03, 269.

[53] *Geol. Mag.*, vol. III, p. 493.

[54] "A Criticism of Dr. Croll's Molecular Theory of Glacier Motion," London, 1880.

[55] "Systematic Geology" of the Fortieth Parallel Survey, 1878, pp. 447-8.

[56] Geol. Surv. O., vol. III, pt. I, 1878, p. 38, *et passim*.

[57] *Am. Jour. Sci.*, vol. XV, 1878, p. 339; *Proc. A. A. A. S.*, vol. XXVII, 1878, p. 198; *Geol. Mag.*, vol. VI, 1879, pp. 353, 412.

[58] Phys. Geol. and Geog. of Neb., 1880, p. 259.

[59] Geol. N. H., vol. III, 1878, p.333.

[60] *Proc. A. A. A. S.*, vol. XXVIII, 1879, p. 350.

[61] *Mem. Cour. et Mem. des Sar. Entr.* of the *Acad. Roy. de Belgique*, vol. XLIV, 1881. Noticed in *Am. Jour. Sci.*, vol. XXII, 1881, p. 80.

[62] Geol. Surv. Ind., 1872, p. 404; 1875, p. 171; and elsewhere.

[63] Geo. Surv. Ills., vol. III, 1868, p. 190; IV, 1870, p. 194 and elsewhere.

[64] Geol. Iowa, 1870, vol. I, p. 327; II, p. 9; and elsewhere.

[65] Geol. Surv. Mo., 1855-71, p. 162; 1873-4. p. 245; and elsewhere.

[66] Phys., Geog. and Geol. Neb., 1880, p. 254.—In each of the first four States above mentioned, the occurrence of the clay and its stratigraphical position has been determined mainly by personal observation.

[67] "Reliquiæ Diluvianæ." 1824, pp. 48-51, 171-84.

a no less mental tax, without adequate recompense. To the teacher, however, for whom it is especially designed, it will doubtless prove quite acceptable as an auxillary to the more elementary text-books.

Several considerations appeared to demand a rather full examination from the "standpoint of the working-geologist of Glaciers." (1.) There is so urgent a demand for a standard work representing fully the present *status* of American Surface Geology (or Kaineontology, as the writer prefers to term that branch of Geology), that almost any book on the subject might be adopted as such without duly weighing its fitness for the position. (2.) In its ambitious style and assumptious *ensemble* the work under review is quite unlike the ordinary text-books. (3.) It is the initial volume of an extended and costly series of works which, from their titles and the fact that they carry with them the prestige of a leading university, might naturally be regarded as the highest American authorities on the subjects treated. (4.) It was not deemed just to working geologists to suggest that the book could well be dispensed with without at the same time furnishing, as fully as practicable, the means of forming an independent judgment.

FARLEY, Iowa, *Nov.* 12, 1881.

LIVING OBJECTS FOR THE MICROSCOPE,

Mr. A. D. Balen, of Plainfield, New Jersey, has undertaken to collect living organisms suitable for microscopical investigations, and forward them by mail to those interested in such studies.

This is a great convenience to those living in cities, or those who are unacquainted with the localities where collections of particular forms can be made.

Among the living objects which Mr. Balen has sent out to his correspondents may be mentioned—

POLYZOA.—Pectinatella, Plumatella and Fredericella.

INSECTS.—Larva of Dragon Fly and Dyticus (water tigers).

ENTOMOSTRACA.—Bosmina, Daphnella, Diaptomus and Sida.

WORMS.—Nais, Stylaria and Planaria.

ROTIFERS.—Lacinularia, Conachilus, Floscularia, Melicerta, Limnias and Noteus.

POLYPS.—Hydra, with the curious parasite Urceolaria pediculus.

BELL ANIMALCULES.—Vorticella, Carchesium and Epistylis, Stentor, Vaginicola and Cothurnia.

INFUSORIA.—Spirostomium, Euglena and Dinobryon.

RHIZOPODS.—Arcella, Actinophrys and Clathrulina.

SPONGE.—Spongilla.

PLANTS.—Utricularia, Vallisneria, Anacharis and Nitella, Volvox, Protococcus and Pediastrum.

DIATOMS.—Surirella, Gomphonema and Fragilaria.

DESMIDS.—Scenedesmus, Desmidium and Micrasterias.

We hope that microscopists will support Mr. Balen in this little enterprise, for it will prove of the greatest benefit to them. A specimen package will be sent for 30 cents.

THE giant forces which scientific discovery is putting in the hands of engineers bid fair to develope a particular form of the profession.—*Engineering News.*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING DEC. 24, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

	BAROMETER.				
DECEMBER.	MEAN FOR THE DAY. Reduced to Freezing.	MAXIMUM. Reduced to Freezing.	MAXIMUM. Time.	MINIMUM. Reduced to Freezing.	MINIMUM. Time.
Sunday, 18	30.137	30.264	12 p. m.	30.100	7 a. m.
Monday, 19	30.309	30.382	10 a. m.	30.264	0 a. m.
Tuesday, 20	30.159	30.288	0 a. m.	30.112	3 p. m.
Wednesday, 21	30.276	30.318	10 a. m.	30.152	0 a. m.
Thursday, 22	29.864	30.228	0 a. m.	29.516	12 p. m.
Friday, 23	29.405	29.774	12 p. m.	29.268	1 p. m.
Saturday, 24	30.188	30.300	11 p. m.	29.774	0 a. m.

	THERMOMETERS.											
DECEMBER.	MEAN. Dry Bulb.	MEAN. Wet Bulb.	MAXIMUM. Dry Bulb.	Time.	Wet Bulb.	Time.	MINIMUM. Dry Bulb.	Time.	Wet Bulb.	Time.	MAXI'M In Sun
Sunday, 18	41.3	37.3	50	1 p. m.	42	1 p. m.	33	7 a. m.	32	7 a. m.	99.
Monday, 19	40.3	38.0	45	3 p. m.	41	3 p. m.	34	8 a. m.	34	8 a. m.	85.
Tuesday, 20	42.6	39.0	47	4 p. m.	42	4 p. m.	38	4 a. m.	36	6 a. m.	65.
Wednesday, 21	40.7	37.0	43	3 p. m.	39	0 a. m.	38	8 a. m.	36	8 a. m.	78.
Thursday, 22	49.3	47.3	53	4 p. m.	51	4 p. m.	39	2 a. m.	38	2 a. m.	49.
Friday, 23	42.6	42.0	55	12 m.	53	7 a. m.	25	12 p. m.	25	12 p. m.	100.
Saturday, 24	27.0	26.0	32	3 p. m.	31	3 p. m.	21	8 a. m.	21	8 a. m.	79.

Mean for the week .... 30.048 inches.
Maximum for the week at 10 a. m., Dec. 19th .... 30.382 "
Minimum " at 12 p. m., Dec. 23d .... 29.268 "
Range .... 1.114 "

	Dry.		Wet.
Mean for the week	40.5 degrees		38.1 degrees.
Maximum for the week, at 12 m., 23d	55. "	at 7 am 23d,	53. "
Minimum " " 8 am., 24th	21. "	at 8 am 24th,	21. "
Range " "	34. "		32.

	WIND.						HYGROMETER.						CLOUDS.			RAIN AND *SNOW.				OZONE.
	DIRECTION.			VELOCITY IN MILES.	FORCE IN LBS. PER SQR. FEET.		FORCE OF VAPOR.			RELATIVE HUMIDITY.			CLEAR, 0 OVERCAST. 10			DEPTH OF RAIN AND SNOW IN INCHES.				
DECEMBER.	7 a. m.	2 p. m.	9 p. m.	Distance for the Day.	Max.	Time.	7 a. m.	2 p. m.	9 p. m.	7 a. m.	2 p. m.	9 p. m.	7 a. m.	2 p. m.	9 p. m.	Time of Beginning.	Time of Ending.	Duration h. m	Amount of water	0 10
Sunday, 18	w. s. w.	w. n. w.	n. w.	230	2	7.00 am	.168	.153	.199	89	44	74	3 cir. cu.	1 cir.	0	-----	-----	----	--	0
Monday, 19	n. w.	s.	s. w.	92	1	11.10 pm	.183	.195	.221	90	67	83	1 cir.	1 cir.	0	-----	-----	-----	--	0
Tuesday, 20	w. s. w.	w. s. w.	w. n. w.	182	2½	3.15 pm	.194	.169	.208	81	54	75	8 cir. cu.	10	8 cu.	-----	-----	-----	--	0
Wednesday, 21	n. n. e.	e.	n. e.	119	2	5.50 am	.173	.164	.181	72	58	73	2 cir.	9 cir.	10	10 pm	12 pm	2.00	.01	0
Thursday, 22	e.	s. s. e.	s.	154	3	1.15 am	.231	.334	.348	83	86	86	10	10	10	0 am 7.45 pm	3 pm 12 pm	15.00 4.15	.23 .30	0
Friday, 23	w. s. w.	n. n. e.	n.	265	12¼	5.00 pm	.376	.267	.174	87	100	100	10	9 cu.	8 cu.	0 am 5 pm	4 am 8 pm	4.00 3.00	.20 .01	10
Saturday, 24	n. n. e.	n. w.	n. w.	178	2	1.20 am	.113	.113	.167	100	67	100	0	0	0	-----	-----	-----	--	0

Distance traveled during the week .... 1,220 miles.
Maximum force .... 12¼ lbs.

Total amount of water for the week .... .75 inch.
Duration of rain .... 1 day, 4 hours, 15 minutes

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.

SCIENCE:

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JOHN MICHELS, Editor.

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SATURDAY, JANUARY 14, 1882.

We refer our readers to another column of this issue where a letter written by Professor George S. Morris, A. M., of the University of Michigan, and Lecturer in the Johns Hopkins University, may be found. This letter is a reply to an editorial in the *Popular Science Monthly* for January of this year, which repudiates the assertion that Herbert Spencer is an atheist, or that his writings have an atheistical tendency, the writer claiming for Spencer that the world is under an obligation to him for elevating man's conceptions of the character of the Deity, and that Spencer, so far from being an atheist, has contributed new and powerful arguments for the existence of an "Infinite and Eternal Spirit," and further that Spencer is ever bringing us to the underlying truth and therefore doing the highest religious work.

As a masterpiece of special pleading the article in the *Popular Science Monthly* to which we refer, will be read with interest, and if it were possible to reason or talk an Augean stable into cleanliness, the editor of the *Popular Science Monthly* might have succeeded in the task he had in hand. Professor Morris has torn off the hypocritical mask of divinity assumed by the editor of the *Popular Science Monthly* for Herbert Spencer, and exposed the real nature of his teachings. Had the editor of the *Popular Science Monthly* merely claimed some mitigating circumstances,or some underlying truths in Spencer's teachings which merited recognition, he might have succeeded in deceiving his readers, who he evidently believes are at the mercy of his sophistry, but to claim for Spencer, the position of a great religious teacher was proving too much, and gives a ludicrous aspect to the whole discussion. As Shakespeare says:

> 'Tis too much prov'd,—that with devotion's visage
> And pious action, we do sugar o'er
> The devil himself.

Let us calmly examine what Herbert Spencer really teaches, and to those who desire to follow us, and have no time to wade through Spencer's voluminous works, we advise a perusal of Professor Morris's valuable work, "British Thought and Thinkers," published by S. C. Griggs & Co. of Chicago. We will now make a few quotations from this work, and as the author is a teacher of this subject in two of the leading Universities in the United States, he may be acceptable as an authority sufficient for our purpose.

What is the "Infinite and Eternal Spirit" which Spencer would have us accept as God? Spencer merely terms it "*the unknowable*," a something or a nothing, which "is absolutely beyond our knowledge." Whatever it may be "it does not come within the range of sensitive consciousness." In plain English this "*unknowable*" may be a God, a Devil, or it may be an ether, electricity or anything else. One thing is certain, that it is not spiritual and is devoid of intelligence.

All that relates to mind or matter is purely mechanical in Mr. Spencer's estimation. He contemplates man in common with the whole universe as the subject and scene only of purely mechanical, automatic, irresponsible and unreasoning processes, in fact the whole knowable universe is brought under the one category of mechanism.

Man is simply "sensitive flesh and blood alone," his very individuality denied, for Spencer says that "the reality of a belief in *self* admits of no justification." *Mind* is a mere bundle of phenomena of a mechanical nature, and *consciousness* simply "molecular oscillations and the transmission of motion in the nervous system," and as if to strike from man the last vestige of his humanity, *morality* is annihilated, for *good and evil* are measured by the amount of pleasure or pain which results. Thus the perfect man, like the perfect hog, is the one whose nervous organization is perfectly adapted to surrounding physical conditions, the man and the brute on one level, soulless and devoid of any spiritual nature.

Such is the Spencerian theology. Readers, picture to yourselves such a God, and man as we have described, and then knowing the real nature of his teachings, imagine Herbert Spencer elevated to the rank of a spiritual teacher who "is ever bringing us near to the underlying truth, and therefore doing the highest religious work," and the sickening hypocrisy of this whole business is apparent.

Well did the rugged philosopher Carlyle exclaim, "There is but one thing without honor; smitten with eternal barrenness; inability to do or be: Insincerity, Unbelief. He who believes no*thing*, who believes only the show of things, is not in relation with Nature and Fact at all."

Much more could and perhaps should be said on

this subject, but as we cannot spare eleven columns to editorial remarks, we will conclude by stating that a wrong is inflicted upon Science by those who suppose it is answerable for Mr. Spencer's debased views of God and man. In summing up Spencer's teachings Professor Morris exclaimed to the students of the Johns Hopkins University, "all this is gratuitous, extra-scientific absurdity, contradiction and dogmatism." Professor Morris does not stand alone in this opinion, and he has at least our hearty endorsement.

It is possible to believe strongly in the theory of evolution and accept every scientific fact that has ever been demonstrated, and yet receive no shock to a belief in a Divine Providence, while the accumulation of scientific facts in our opinion all tend to confirm such belief, and to demonstrate scientifically that an intelligent Creator has designed and pre-arranged the order of both matter and mind.

In conclusion, we desire to say decisively, that science is not answerable for the vagaries of Mr. Herbert Spencer, his editorial supporters, and others of the same class; his atheistical dogmas are neither founded on scientific investigations or in harmony with scientific discoveries. The mere fact that a scientific journal is made use of for proselyting such views even to the extent of attacking editorially, a President of a university who declined to use a recent work of Spencer's as a class-book, should not be considered evidence that scientific men, as a body, have any regard for the extreme views of Herbert Spencer. On the contrary, those engaged in real scientific work, do not care to interfere with their neighbor's religious opinions, much less do they desire to force atheistical views upon them.

Lastly, we say emphatically, that there is no real conflict between Science and Religion at this present day. Some persons appear to consider that they have a mission to stir up discord and contention between scientific men and their best friends, and the worst feelings are engendered by continued attacks against men holding any religious views who form nine-tenths of the population in all civilized countries.

What better evidence can be given for the correctness of the position we take than the fact, that a large number of our most esteemed scientific workers are men in holy orders. We could fill a page by the mere enumeration of their names. Dallinger, the biologist, who has carried off the highest scientific honors, is a Protestant Clergyman. In astronomy we have a Catholic priest who successfully investigates the mysteries of the heavenly bodies, for Secchi's name will always be classed among eminent astronomers. If there was any real conflict between science and religion, would these men have continued their investigations? Of course not. The conflict at this day is wholly imaginary, invented and kept alive for sensational purpose. If these men would cease their irritating interference, science would be welcomed in every home and be considered one of the most convincing evidences of a divine Providence, instead of being hated and dreaded, as not in harmony with any religious belief.

We do not deny that there are many who cling to religious dogmas which have been exploded by facts revealed by science. For such we have compassion, but we hold in far higher contempt the bigoted blustering fanatic who has no religious belief whatever.

Hume admitted that he dared not select his own confidential servant from such as held his own principles. We believe we are correct in saying that Professor Huxley, who holds views somewhat akin to Spencer, is careful in selecting a school for his children where the Bible is taught. These facts appear to show conclusively that these advanced thinkers considered that there was a possibility that they might be wrong, and that some discretion was necessary in teaching their atheistical views, at least in their own families.

We apprehend that similar prudence should be practiced by all who are directly or indirectly answerable for now *popularizing* views and principles which, if successfully propagated, will be destructive even to a simple belief in God, and aim to undermine society itself by denying the intrinsic value of morality.

Finally, we ask that science shall no longer bear the odium of atheism; that it be freed from this pernicious parasite, and that atheism being published in journals devoted to that subject, shall be supported only by its own devotees.

WE trust the above remarks may not be interpreted as an attack on the "*Popular Science Monthly*" as a journal, or personally on the editors. The latter are gentlemen, honored and respected wherever science is known, and have been pioneers in the good work of introducing scientific knowledge into the homes of the people; their journal has always been conducted in a manner to defy criticism, and is an honor to the house which publishes it. The recent editorial was a bold demand for criticism on the policy of the journal teaching doctrines, which appear to lie outside of its province as a scientific journal. To this we have responded.

The root of the question at issue lies in the interpretations of the works of Herbert Spencer. We consider Professor Morris a safe guide in this matter, and a perusal of his letter will show that Spencer's writings have a dual character, they *partly* confirm the position taken by the "*Popular Science Monthly*," so far as showing Spencer believes in a "*something*," but are fatal to all the deductions drawn by the editors of that journal, and strictly in accord with the position we have reluctantly taken in this controversy.

NEW YORK ACADEMY OF SCIENCES.

Dec. 12, 1881.

SECTION OF GEOLOGY.

The President, Dr. J. S. NEWBERRY, in the Chair.

Forty one persons present.

Mr. N. L. BRITTON presented

"ADDITIONAL NOTES ON THE GEOLOGY OF STATEN ISLAND." *

Two wells have recently been sunk to a considerable depth on Staten Island, in the vicinity of Stapleton. One of these is on the property of Mr. J. J. Cisco, near the summit of the Serpentine hills; the section as given by the Superintendent of the Pierce Well-boring Co., who sank it, is as follows:

Glacial drift, ...	50 feet.
Soapstone, ...	150 feet.

* These notes are supplementary to the paper on this subject read by Dr. Britton on April 4, 1881. (Ann. N. Y. Ac. Sci., II, 161.)

The well is six inches in diameter, and sufficient water was obtained to make it a success.

The other well is at the pump-house of Bischoff's Brewery, some 500 feet east of the most eastern serpentine outcrop at the foot of the hills. This has now (Dec. 1st) reached a total depth of 210 feet, and the boring is still unfinished. The section thus far has been as follows:

Glacial drift........................80 feet.
Various kinds of tough hornblende schist, apparently varying to serpentine,....130 feet.

As yet no gneiss nor granite has been reached.

An outcrop of clay occurs near Clifton, about three-fourths of a mile south of the Forts, near the southern edge of the terminal moraine; it has been found, by borings made by Mr. Charles Townsend, in excavations for cellars, to be at least ten feet in thickness, and of a light color.

The clay is probably of Cretaceous age, and if so, this is the most eastern point at which beds of that age are known on Staten Island.

Mr. W. T. Davis has recently observed a large fossiliferous boulder of Schoharie Grit on the shore at Brighton Point. The fossils have been submitted to Dr. Newberry, and the following species identified:—*Dalmanites anchiops*; *Orthoceras Pelops*, *Strophodonta hemispherica*; *Atrypa reticularis*; *Strophomena rhomboidalis*; a *Fenestella*; and *Zaphrentis prolifera*.

Glacial groovings have recently been noticed on the hornblende-rock, which is exposed at tide-level on Brighton Point. Some of the grooves are at least one-quarter of an inch in depth, three inches wide and four feet long. Their bearing varies from N. 15° W. to N. 17° W.

DISCUSSION.

Prof. D. S. MARTIN considered the specimen of so-called hornblende schist from the well-boring, not to consist properly of that rock, but to be partly hydrated—apparently a less altered condition of the rock which higher up gives us the soft, semi-fibrous serpentine of the island.

Dr. NEWBERRY regarded the serpentine of Staten Island as probably a pseudomorphous condition of hornblende slate. It differs considerably from the mottled serpentine of New York Island, which is "verde antique"; that is, is composed partly of serpentine and partly of carbonate of lime, and is scarcely distinguishable from the Moriah marble, which is quarried at Moriah, Thurman, etc., in the Adirondack region. It is a peculiar rock, and one of the connecting links between the rocks of New York Island and those of northern New York and Canada. Taken together, these afford strong indications of the Laurentian age of the New York Island and Staten Island crystalline rocks.

Dr. Newberry further said that the accurate determination of the age of the rocks of New York Island, of Staten Island, and of those underlying the drift of Long Island, was in the highest degree desirable and important; and while he was satisfied that the former were Laurentian, and the latter Cretaceous, it was eminently desirable that unquestionable proof should be found of this, if it is true. At present no positive assertions could be made, and the duty devolves on the geological members of the Academy to rid the subject of doubt.

The fossils in the boulder referred to by Mr. Britton prove to have come from the Schoharie Grit. In its original condition this was a hard, compact blue limestone, but is here presented in a leached state, by the passage of waters containing carbonic acid, with a loss of its lime, color, and density. It was derived from northern New Jersey, to which locality a belt of this rock runs down from Schoharie county. Its transit by ice was effected without doubt through the valley of the Hackensack, which lies east of the Orange Mountains and west of the Palisades. This glacial movement is indicated by the direction of the striæ observed by Mr. Britton, as well as by those in the Hackensack valley.

Mr. A. A. JULIEN recalled the results of his lithological examination of the serpentines both of Staten Island and of Hoboken, presented before the Academy two years ago, in which it was shown that sections of all these rocks abounded in minute fragments of more or less altered amphibole. The conclusion then stated, that these serpentines must be certainly derived from hornblende schist, was confirmed by the interesting discovery of the latter rock, both in well-boring and on Brighton Point. Serpentines of the same general character and origin occur frequently throughout New York and Westchester counties. The mineral serpentine is also found in small quantity as a vein-deposit, not pseudomorphous, like the main mass, but presenting an amorphous material with banded vein-structure, associated with magnesite, dolomite, etc.; e. g., the marmolite of Staten Island, a translucent green variety found at Hoboken, and also at West 60th street on New York Island, etc. At all these localities the amphibole survives in a more or less altered condition; e. g., the tremolitic talc schists and slaty tremolitic serpentines of Staten Island and Hoboken, the hydrous anthophyllyte and unaltered tremolyte rock of West 60th street, New York, the tremolitic amphibolyte of New Rochelle and Rye, in Westchester county, etc.

Mr. BRITTON confirmed the last remarks, by the statement that a vein of material, strongly resembling the hydrous anthophyllyte of New York, had been struck at the bottom of one of the wells on Staten Island; also that veins of mixed serpentine and calcite were observed at Stapleton, possessing a banded structure parallel to their walls. At that point the apparent thickness of the serpentine bed is 150 feet, but the crest of the hill is composed of talcose schist.

MR. W. LE CONTE STEVENS then read a paper on "THE MAMMOTH CAVE OF KENTUCKY,"

He also exhibited specimens of the blind fish (*Amblyopsis spelaeus*), and blind crawfish (*Cambarus pellucidus*), and stereoscopic views of various points in the interior of the cave.

(Abstract.)

At the close of the Cincinnati meeting of the American Association for the Advancement of Science, in August last, he was one of a party of seventy-five members who visited the Mammoth Cave, remaining there two days, during which the greater part of the time was spent in exploration. He made no claim to new discoveries, but wished to call the attention of the Academy especially to recent observations, for the most part by Rev. H. C. Hovey, of New Haven, in regard to the temperature and structure of the cave. Mr. Hovey read a paper on this subject in Cincinnati, only a brief abstract of which has yet appeared in print, making use of a map, which is the first of its kind ever exhibited. The strictest precautions are observed by the authorities controlling the cave to prevent visitors from taking surveying instruments in with them: but the present manager, Mr. Francis Klett, has made a careful survey of the most interesting parts, and in time this will probably be given to the public, though possibly the scale of measurement may be withheld.

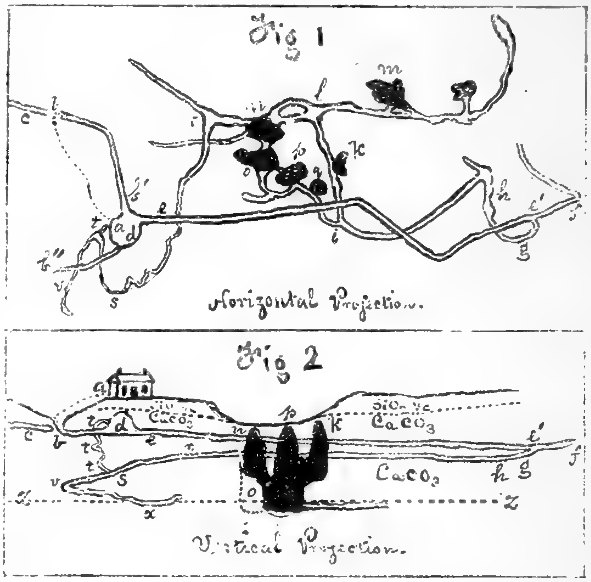
The central and right-hand portions of the map exhibited by Mr. Stevens had been enlarged by him from a copy of Mr. Klett's map. The left-hand portion was drawn only from recollection of the localities traversed, and not to scale, being intended only to illustrate principles. The same remark applies to the vertical projection, the lettering of which corresponds with that of the horizontal projection.

The temperature observations of Mr. Hovey were con-

ducted with much care, and the very best instruments had been confided to him by the Director of the Winchester Observatory at New Haven. In August, 1881, while the external temperature at the neighboring hotel varied between 90° F. and 100° F., at points farther than 100 yards within the cave, the reading of the thermometer was never more than 56° nor less than 52½°, the mean temperature being 54° for the summer months. At a point 1,000 yards within, a thermometer had been left for six months, including the autumn and winter, and daily visited by Mr. Klett, who reported the variation to be only from 54° down to 53°. The underground temperature in this latitude, for points 60 or 70 feet below the surface, is usually assumed to be constant and about the same as the mean annual temperature above. According to Prof. Guyot's maps, the isotherm of 60° passes about thirty miles south of the Mammoth Cave, while that of 50° passes about forty miles north of Cincinnati. The temperature of the Mammoth Cave is fully 6° lower than has been commonly supposed, and may be taken as a fair representation of that of the crust of the earth in the country immediately surrounding it.

Mr. Stevens exhibited a geological map of Kentucky, showing the area of sub-carboniferous limestone in which the Mammoth Cave is situated. This is overlaid with a thin stratum, mostly of sandstone, that is pierced by thousands of sink-holes, through which the surface drainage is carried down into limestone fissures and thus to the general drainage level of the Green River. This stream passes at the distance of less than a mile from the Cave Hotel, the floor of the latter being 312 feet above the water and 118 feet above the mouth of the cave. He briefly explained, with a diagram, the general mode of cave-production in limestone strata, showing that subterranean tunnels must be started by the solvent action of slightly acidulated rain-water, and subsequently enlarged by erosion, along the fissures in the limestone. These agencies are still at work in portions of the cave, and the whole of this limestone country is thus honey-combed with caverns. No tunnel can be thus formed at any point lower than the general drainage level, since there must be an exit for the saturated water. The production of the fissures is referable to the general upheaval of this area at the close of the coal period: but, that there has been subsidence since the completion of much of the Mammoth Cave, is indicated by the fact that at its lowest parts to-day the floor is covered with water to the depth of thirty feet or more, having subterranean connection with Green River. The fissures intersect at various angles, but many of them are nearly or quite coincident with the dip of the strata, which is very gentle. Water passing through these forms the tunnels, while that passing through the vertical fissures scores out the pits which pierce them. The same pit, starting from a sink-hole at the surface, may have successively lower tunnels as exit passages. If the visitor encounters it while walking through the higher, and therefore older, tunnel, the upper part appears to him as a dome, the lower as a pit.

The rate of erosion in the Mammoth Cave has been variable. The older parts are perfectly dry, and entirely free from stalagmitic deposits, indicating rapid erosion, followed by elevation, so as to deviate the water completely into other channels. In the newer parts the water is still dripping from the surface above, and depositing stalactites and stalagmites; but as a whole the cave is by no means remarkable for these formations, being much surpassed in this respect by the neighboring White's Cave, of more recent origin. Those which do occur are moreover deeply colored with iron, which exists in the soil in the form of both oxide and sulphide. In the dry parts, the ceiling of the cave is more or less covered with efflorescent calcic, magnesic and sodic sulphates, which contrast with the iron-stained limestone, giving rise to the beautiful effects that have conferred celebrity on the opening known as the Star Chamber, and the myriad rock flowers of Cleveland's Cabinet.



The structure of the pits and domes was then illustrated with the aid of the accompanying map, by describing a journey through the cave. From the hotel, (*a*, figures 1 and 2,) the visitor walks to its mouth (*b*), by the side of a shallow ravine, terminating in what was formerly a large sink-hole. The door of this fell through, about seventy years ago, producing the present mouth of the cave, and cutting off part of the gallery, now known as Dixon's cave (*c*), which opens out near the Green river, a half mile distant. A walk of 1000 yards brings him to the Great Rotunda (*d*), about 170 feet in diameter and 100 feet high. It is immediately under the hotel, its roof being not more than 40 or 50 feet from the surface. Besides the gallery, called the Narrows (*b'*), by which access has just been obtained, another tunnel from the further side terminates in the Rotunda, to which the name of Audubon's avenue (*b''*) has been given. The large, almost hemispherical opening, seems to have been cut out by the meeting of nearly opposite streams of water, which found exit, probably, through the main cave (*e*). At some distance within Audubon's avenue, a small opening in the floor is found, connecting it with the roof of the Mammoth Dome, a vast cavern 400 feet long, 100 feet wide and 250 feet high. These figures are of course only approximate, but it is believed that they are not exaggerated. Into this cavern the water is still trickling, and stalagmites are forming with sufficient rapidity to have cemented firmly to the floor a lamp dropped in 1812 and found in 1843. Returning to the Rotunda and passing through a half mile or more of the main cave, the visitor reaches, at *e'*, a large fallen slab of limestone to which has been assigned the title of "The Giant's Coffin." This makes the entrance to a side passage (*g*) which leads off to the lowest part of the present cave. The main cave forms an acute angle (*f*) and may be followed for several miles, terminating abruptly in a pile of rocks, where the roof has fallen in the same manner as at the terminus of Dixon's cave. Many of its side passages and avenues are yet unexplored.

Returning and entering the side passage near the Giant's Coffin the visitor passes obliquely beneath the main cave, starting upon what is known distinctively as the Long Route. At an expansion (*h*) are successive deposits of gravel, sand and clay, indicating the downward course of the water which was here partially arrested.

Some distance further on, the passage forks (i). Keeping to the right, the dangerous Side Saddle Pit (k) is encountered, which measures 65 feet in depth and 20 feet across. It is surmounted by Minerva's Dome, 35 feet high. The pit yawns across the right half of the floor of the tunnel, leaving a narrow path on the left. A short distance beyond (l), the tunnel again forks. Keeping to the right as before, Gorin's Dome (m) is reached, and may be viewed with the aid of magnesium lights, from a small opening on the side, ten feet above the pathway. The abysm extends 117 feet downward, 100 feet upward and 60 feet across. Leaving this and passing the fork (l), the tunnel is completely interrupted by the so-called Bottomless Pit (n) across which a bridge has been laid, resting upon a ledge. Despite its ominous name it does not defy measurement, having been found to be 95 feet deep on one side of the ledge and 105 feet on the other. Almost immediately overhead is Shelby's Dome, 60 feet high. Between the Bottomless Pit and Side Saddle Pit are a pair of very large pits, discovered not a year ago by one of the guides, William Garvin, and examined for the first time last August by Mr. Hovey, who gave to them the names Scylla (p) and Charybdis (o) on account of the narrow, rugged passage which separates them and the great difficulty and danger of access. By timing the fall of pebbles into the water at the bottom, the depth of each was ascertained to be about 200 feet. Charybdis was seen to be directly connected with the Bottomless Pit. Indeed the latter may be regarded as only a part of Charybdis, its depth, 105 feet, being only that of a jutting ledge, or the floor upon which water ceased to fall after being slightly deviated into Charybdis, where the sound of its trickling is still audible. Shelby's Dome is simply the upward continuation of this combined pit. So narrow, moreover, are the ridges separating Scylla from Charybdis on the one side and from the Covered Pit, (q), on the other, and so small is the distance to the Side Saddle Pit (k), that it seems in the highest degree probable that this group of pits compose merely the upper branches of a single large pit into which they are all united, or at least directly connected before the bottom is reached, and the small relative depth of the Side Saddle Pit is explicable in the same manner as that of the Bottomles Pit. Such an extraordinary group of pits, forming an apparent nucleus of cave drainage, might be expected to have its counterpart in an unusually large depression, or group of sink-holes, at the surface. Impressed with this idea Mr. Hovey found in the woods, scarcely half a mile from the Hotel, in the known direction of these pits, a depression (p Fig. 2.), many acres in extent, and so deep that from its edge he could overlook the tops of the pine trees that rose from the middle.

Leaving this region of pits and domes, the route leads still downward, passing again under the main cave through the narrow tortuous channel known as "Fat Man's Misery" (s) where the distance from floor to roof is in many places not more than three feet. Through the floor a winding passage has been worn away, varying in width and depth from one to three feet. This terminates in a chamber which has received the appropriate name of "Great Relief," where the succession of pebbles, gravel, sand and fine clay again records the work of erosion and deposit. This bed is not more than 50 or 60 feet above the drainage level, and from here down to the River Styx, the ground becomes more or less damp. A succession of bodies of water are then encountered, including the tubular Echo River, which is navigated in boats. It is a part of the tunnel which has subsided below the water level, and is in connection with Green River, being filled to within a few feet of the roof in summer, and completely closed in winter when the Green River rises. The column of air between the water and the impervious roof, closed everywhere except at the two ends, which are three-fourths of a mile apart, serves as a resonator for any note within the range of the human voice, and multiple echoes gliding imperceptibly into each other, continue to be returned for many seconds after the voice has been hushed.

Beyond Echo River, the cave may be followed, with continual ascent, through Silliman's Avenue, the Pass of El Ghor and Cleveland's Cabinet, for about five and a half miles. A pile of jagged rocks, 100 feet high, is then surmounted and the wearied climber is confronted with a large cavern, 100 feet wide and 70 feet deep, where three short branches have united in one tunnel. Following the left branch for a few yards, a hall is found, in the floor of which is a pit 175 feet deep. The corresponding dome overhead is scarcely noticeable as such, for the surface of the ground is not more than 30 or 40 feet distant. The end of the Long Route has been reached.

In returning, the passage through Fat Man's Misery is avoided, and nearly two miles of walking are saved by climbing through a very steep, narrow, winding "Corkscrew" pass (t, Fig. 2), starting from the neighborhood of Great Relief and terminating at the side of the Great Rotunda. The vertical ascent is about 140 feet. To even stout-hearted mountaineers, if stout-bodied also, this Corkscrew is an intensified Fat Man's Misery, and upon them it rarely fails to leave strong and deep impressions, which may be of more kinds than one.

In regard to the animal life of the Mammoth Cave, conflicting opinions have been expressed by those who have made a special study of this subject. The bats, lizards and rats that have been found cannot be strictly called cave-dwellers, as they are always at points not so far removed from the outer light as to make this inaccessible, The cave crickets and blind crawfish have particularly long antennae and acute powers of hearing. Most of the crawfish are pale in color, some of them almost white; and this feature has been attributed to the continued absence of light. Crawfish, however, with well developed eyes and of dark color have been often found. These are without doubt either wanderers from Green River or the immediate descendants of such; and many generations of cave-dwelling are required to bring about such changes as have caused the application of a specific name, *Cambarus pellucidus*, to the white variety with only rudimentary eyes.

In regard to the blind fish it is a significant fact that the rudimentary eyes of the young are apparently less atrophied than those of the mature fish. Although to these cave dwellers also a specific name, *Amblyopsis spelaeus*, has been given, they are by no means the only fish found amid this stygian darkness. The existence of fish with perfect eyes, apparently prospering where eyes are useless, shows how much less dependent these creatures are than more highly organized vertebrates upon approximate uniformity in external conditions. To those who have already accepted evolution, there is far less difficulty in believing that the colorless blind fish are the modified descendants of dark-colored ancestors with perfect eyes, which have wandered from Green River into Echo River, than in concluding that they have always constituted a separate species, as held by Prof. L. Agassiz, and subsequently contended by Prof. F. W. Putnam.* Nevertheless, Prof. Putnam has shown that the differences between the blind fish (*A. spelaeus*) and their nearest living congeners are much more than in respect to mere color of skin and power of vision. Whether the internal anatomical differences on which he reasonably lays much stress can be proven to be a natural result of the external conditions imposed by cave life, is a question which, if settled at all, must be settled by zoologists alone. Prof. A. S. Packard, Jr., and Prof. E. D. Cope are as pronounced in their opinion that the blind fish have been evolved from fresh-water ancestors possessing good vision, as is Prof. Putnam in the opinion that their ancestry were denizens of salt or brackish water, with which

* The Mammoth Cave and its Inhabitants. By A. S. Packard, Jr., and F. W. Putnam, Salem, Mass., 1879.

he believes that the cave was supplied at a time when this region was a salt or brackish water estuary. Prof. Putnam therefore concludes that the blindness of these fish has been in no respect a consequence of subterranean life.

DISCUSSION.

Mr. BRITTON inquired whether any flora existed in the cave.

Mr. STEVENS replied that, so far as he was aware, no kind of vegetation had ever been found within it.

Dr. NEWBERRY remarked on the geology of the region adjacent to the Mammoth Cave. The limestone beds of this high table-land are jointed in the manner common to rocks, apparently by some sort of polarisation, producing fissures which run in a north and south, and an east and west, direction. The plateau is about 500 feet above the drainage, part of the drainage passing into the Green River, and part into the Ohio. No streams occur on the surface and the drainage is quite gradual. At the angle between these two rivers several streams are seen, bursting out of the cliffs at various heights above the Ohio; they are, so to speak, subterranean sewers, representing the underground drainage of the country; at one point three such streams pouring out of the rock form very beautiful cascades; and near Sandusky a full grown river flows out of the cliff of cavernous limestone. The beds consist of lower carboniferous limestone, with sandy layers beneath. In the vicinity occur portions of the great "blue grass region," one of the oldest parts of the continent, once an extensive highland, forming an island in the sea. Around this, rims of sediments were deposited, consisting of sandstones and limestones; while on the other hand, the continuous process of erosion, during the lapse of a vast period, removed the material of the table-land within, and converted it into a broad depression or basin, the "blue grass region," above which the present plateau of the encircling sediments now rises to a height of 500 feet.

The erosion of the joints in this plateau has resulted in the formation of the pits described by Mr. Stevens, but it is probable that some of these may reach 200 or 300 feet below the Ohio and Green Rivers. There is evidence, from borings in the Delta of the Mississippi, etc., that the Continent was formerly more elevated, standing 500 to 600 feet higher at New Orleans than at present; the drainage was much freer, the Mississippi being a free flowing stream, as well as the Ohio and other tributaries. Borings have been sunk in the present trough of the Ohio river, to a depth of over 100 feet below its present bottom, without reaching the true bottom of the trough, the ancient bed of the river, which is perhaps from 100 to 200 feet further down.

Evidences of the same elevation of the continent were observed in caves on an island in Lake Erie. Long stalactites projected from the roof of a gallery whose end was ordinarily filled with water at the present level of the lake. At times a strong and steady wind has blown down the level of the lake and partially drained this gallery; but even then a guide, John Brown, resident on the island, has swum through the gallery and found the stalactites projecting from the roof as far as he could go.

In regard to the origin of the blind animals, the view of Prof. Cope is probably correct, that they have been derived from the degeneracy of ancestors who once had perfect eyes. No fish is formed with poor eyes; but any organ may be atrophied by disuse, with consequent feeble flow of blood, decreased nutrition, and inevitable shrinking of important parts. An analogy is shown in a comparison of the jaws of prehistoric and modern men. At present our "wisdom teeth" are useless, there is no room for them in the shortened under-jaw; our food being softened by cooking, cut up, and boneless, requires less vigorous mastication; and from disuse, and the consequently insufficient development, these teeth often speedily fall away. In the prehistoric man, on the contrary, the jaws were longer, roomier, supplied with more teeth—the "wisdom teeth" being well developed and kept in strength by constant use on coarse and rough food. The absence of the well-known stimulation produced by light, from the dark waters within the Mammoth Cave, has in the same way resulted in the atrophy of the organs of sight.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of "SCIENCE."

We can sympathize sincerely with the Editor of *The Popular Science Monthly* in his indignation at being held a promulgator of the views of "pronounced atheists," because of his publication of "the papers of Herbert Spencer, and others of his class." "Pronounced atheism" finds little place in the history of philosophy or science, as in the history of mankind and human civilization in general. And Dr. Youmans is certainly in the right with his emphatic denial that Mr. Herbert Spencer, in particular, pronounces himself an atheist and seeks to persuade others to do likewise. He "and others of his class" have, indeed, been very out-spoken in questioning the literal truth of many popular beliefs and sacred traditions. But that there is in "religious ideas" no "vital element," that they correspond to no fact and represent no truth, Mr. Spencer has been far from asserting. On the contrary, the precise opposite is most strenuously maintained by him (see especially Spencer's *First Principles*, Part I.).

And yet, while all this is verbally true, we fear that Dr. Youmans, in his just zeal to defend himself and his friend, both goes too far in his statement of the latter's real position, and forgets those grounds which lend color of justification to the perfectly sincere supposition of many thoughtful people, that the practical, if not the professed or intended, tendency of Mr. Spencer's philosophy, is in the direction of virtual atheism.

If it were really true that "no man of the present age has reasoned out the foundations of man's belief in the existence of the 'Infinite and Eternal Spirit' with such a depth of analysis and logical force as Herbert Spencer," if, as Dr. Youmans further declares, it were strictly true that Mr. Spencer "has sought to show that the 'Infinite and Eternal Spirit,' of which all the phenomena of the universe are but manifestations, is the most absolute of all realities," then religion would owe to him a debt of gratitude, which it is inconceivable that the intelligent defenders of religion should not gladly recognize and avow. But we are at a loss to know on what grounds the above assertions are made by the Editor of *The Popular Science Monthly*. Perhaps it is in esoteric discoveries, delivered to a select few of his admirers, that Mr. Spencer has "reasoned out" the aforesaid "foundations" and "sought to show" the pre-eminent absoluteness and reality of the 'Infinite and Eternal Spirit,' and Dr. Youmans's statements may have been made on the basis of what he has personally been privileged to hear of these discoveries. Thus the writer of these lines was once informed by an admirer of Mr. Spencer's, who had recently come from a personal interview with the philosopher, that the latter believed in "a God"—*supposing, not without a good deal of reason, that this would be a piece of news to one who knew of Mr. Spencer and his opinions only through his published writings.*

It is in the latter way, only, that Mr. Spencer is known to the general public. We, for our part, cannot claim for ourselves familiarity with every line which Mr. Spencer has ever written. But we have studied with great care and with great interest, what we supposed to be Mr. Spencer's of the redistribution of matter and motion. Some of these phenomena have indeed a *mysterious "obverse"*

most important philosophical works, and we do not remember any where to have noticed any evidence of concern on the author's part to prove the existence of an "Infinite and Eternal Spirit." On the contrary, we are every where forbidden by him to regard the Infinite and Eternal, or the Absolute, as either Spirit or matter. Both of these "antithetical conceptions" are held to be purely finite, relative, phenomenal. The absolute is simply "the unknown reality which underlies both," (see *First Principles*, last sentence of the book, *et passim.*) The absolute we are constantly reminded is "wholly unknowable." It is neither Infinite and Eternal Spirit, nor Infinite and Eternal Matter, but simply an altogether indefinable and incognizable somewhat. "That through which all things exist" is in Mr. Spencer's language, "The Unknowable."

The Unknowable is further held to manifest itself to us only as an "inscrutable force" whose operation is exclusively confined to the evolutionary and mechanical "redistribution of matter and motion." Since this operation takes place under the form of rule or law, it is held to conflict with, and render impossible, the supposed "free will," and hence the truly *spiritual nature* of man.

The case is therefore as follows: That there is an absolute reality, we are held to know through "a dim" or wholly "indefinite consciousness," which is called the "*raw* material of mind," but which utterly refuses to be grasped, defined, or known. The "Infinite Something," which is thus demonstrated for us, is, so far as our definite knowledge extends, and hence practically, an "Infinite Nothing." Strictly known to us are only phenomena aspect, which we term spiritual, ideal, or mental. But no scientific interpretation of these is possible, no knowledge proper is possible concerning them, except so far as they are reducable, directly or proximately, to terms of the redistribution of matter and motion in physiological processes. All our definite knowledge, therefore, is both in its data and its substance, exclusively physical and materialistic, and even the "indefinite consciousness," by which we are held to be assured that an Absolute Something exists, in as regards both its subject and object, also physical; it is certainly not spiritual.

Now, if God, provided he exist, is necessarily a spirit; if man, as the subject of religious emotions and relations, must also be a free spirit; and if, as is the case, there is found in Mr. Spencer's philosophy *no recognition of either God or man as a spirit*, then it is obvious that much ground is given by Mr. Spencer for the supposition that his doctrines—considered *per se*, or independently of their author's intentions—*are virtually atheistic and anti-religious*, and those who honestly entertain this supposition are entitled to be met, not simply with a vigorous *assertion* that they are in error, but with a dispassionate and objective demonstration that they are so.

The whole basis of Mr. Spencer's theory of knowledge is, as is well known, sensational and physical. From such a basis it is and has always been found impossible to rise to the recognition of the absolute as spirit, or man as spirit, or to *comprehend religion otherwise than as a necessary historic incident in the development of ideas.* But the whole basis of human knowledge is not sensational and physical. Free religion implies this, and the grander historic forms of philosophy demonstrate it. The *pre-eminent* intention of knowledge in physical science is indeed sense. The attempt to make this criterion universal leads necessarily to agnosticism with reference to the non-sensible (the Spiritual, Living and Powerful). But it is not *science* which dictates this attempt, and *so Mr. Spencer's agnosticism is not to be charged to science.* The rather, it is due to a purely arbitrary determination on his part, supported, it is true, by the influence of a conspicuous line of predecessors in the history of British speculation. The fact that many theologians have been equally—and some of them—*e. g.*, William of Ockham—even more absurdly agnostic than he, is not to Mr. Spencer's credit, but to the theologians' discredit. Besides, the agnostic theologians have generally made vigorous affirmation, on the authority of the heart, of that which to their heads was inscrutable. They have, like Kant, practically affirmed that which seems theoretically incomprehensible. However, all this belongs to the sadder side of the history of human thought. Philosophy and theology have existed and still exist in larger, more positive, and more fruitful forms, founded on a completer science of knowledge, which recognizes the spiritual factor in knowledge, or the knowing agent, and so, necessarily, the spiritual nature in the absolute object of knowledge or God.

We say, then, that Mr. Spencer is by no means to be charged with intentional atheism or irreligion. To theism and religion he gives all the meaning which it is possible for him to give them on the basis of that physico-scientific theory of knowledge, which he sincerely believes to be the only possible one. But this meaning really falls absolutely short of meeting the actual requirements of theistic doctrine and living religion. And Mr. Spencer's doctrine in this regard is not that of science, whether "popular" or otherwise, but of a highly artificial and arbitrary "philosophy" It has no more necessary relation to the doctrine of evolution than to the doctrine of gravitation, both of which have been and are (in some form) unquestioningly held by many leaders in spiritualistic or positive (*vs.* agnostic) philosophy.

The dissemination of the eminently valuable results of Mr. Spencer's scientific labors is certainly in place in a *Popular Science Monthly*. But with what special propriety such a periodical should also be made the peculiar vehicle for the promulgation of his extra-scientific *philosophy* it is hard to see. It is not that we would have a line, which Mr. Spencer has written, suppressed or kept from the knowledge of the world. But regard for the honor and purity of * science, to mention no other consideration, is enough to make one ardently wish that it should not be constructively put forward as sponsor for doctrines whose basis is only quasi-scientific, and which, in truth, belong to another domain—the domain of philosophical inquiry.—GEORGE S. MORRIS, *Professor of Philosophy, University of Michigan; and Lecturer in the Johns Hopkins University.*

THE HOLLAND HYDROGEN FIRE APPARATUS.

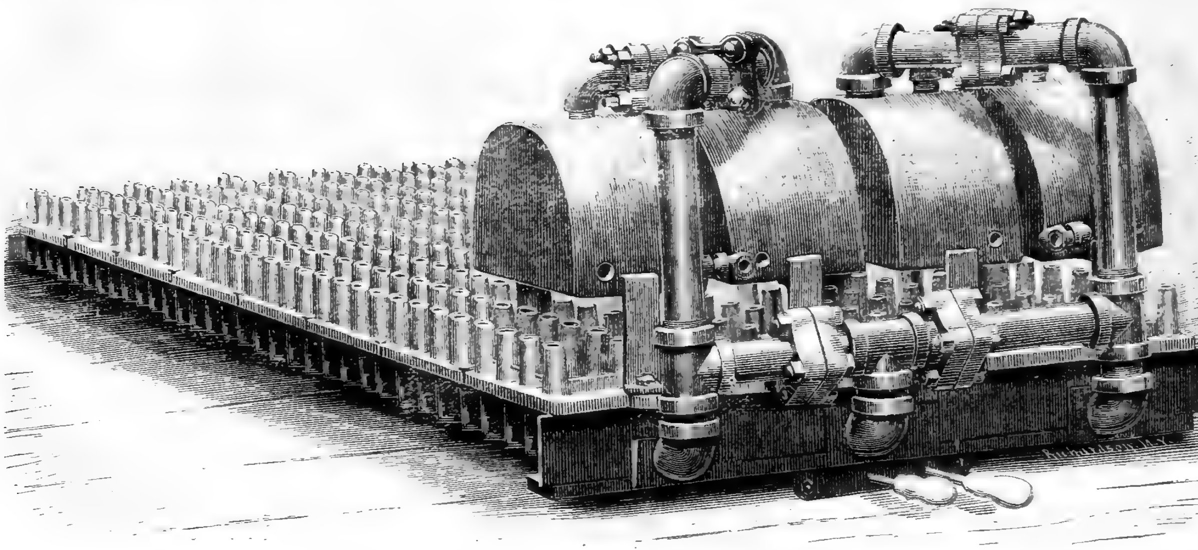
No little interest has been excited during the past year, both in the scientific and practical world, by the remarkable development of results from the Water Gas Apparatus of Dr. Charles Holland, in an ordinary locomotive, as reported by a careful and disinterested observer, through the daily press, and subsequently discussed from a scientific point of view in this journal.

A review of the subject, which has lost none of its importance in the light of further experience and deliberation, will be timely and interesting at the present date.

At Flatbush, the apparatus was placed in the fire-box or furnace of a large (forty-ton) passenger locomotive, of the usual coal-burning pattern, with 16x24 inch cylinders, 5-feet 2-inch driving wheels, and a boiler 23 feet long. In place of the ordinary grate bars are laid three hollow bars or pipes the length of the furnace (8 feet). and from each side of each pipe rise burner-tips at short intervals, making 352 in all. On these pipe-bars, as sleepers, is laid a floor of iron plates studded with open thimbles, through which the 352 burner tips rise to within half an inch of their openings. Over the first 44 burners, next the door, are set four retorts —heavy, hollow blocks of iron—in a row. Two of these retorts receive naphtha, and two water or steam, through separate pipes, and when heated, unite and discharge their vapors through connecting pipes into the pipe bars under the iron floor, and thence through the 352 burners.

The observations at present available enable us merely to compare the firing-up of the same locomotive to the same pressure under substantially equivalent conditions,

* By "science" we mean, in accordance with the now prevalent usage, the mathematico-physical or descriptive science of sensible *phenomena.*



FIRING APPARATUS OF THE NEW HYDROGEN-BURNING LOCOMOTIVE.

first with the Holland Hydrogen Process, and afterwards with coal and wood. This comparison is practically sufficient in a broad sense, yet for more exact purposes it is to be expected that opportunity will soon be taken to obtain the result in pounds of water evaporated per pound of naphtha, and also to exhibit chemically a specimen of the gas. We learn that the boiler of the new hydrogen-burning locomotive, since built at Paterson, after having been tried with very small flue, has proved that the highly expansive and voluminous gas produced requires more room for its most advantageous combustion, and the small flues, are now being exchanged for larger. The production of steam pressure was still more rapid and economical than in the Flatbush experiments, but the heat was so unequally distributed, and the fire so insufficiently vented, through the small flues as to make it evident that they were not adapted to do justice to gaseous fuel as had been expected. For the present, therefore, we are obliged to content ourselves with the latest of the series of tables in which the comparative results of Holland gas and common fuel have been reported.

STEAM, POUNDS.	Time, Minutes.	Naphtha, Gallons.	Naphtha, Per lb. Steam.	Naphtha, per Minute.	Total Naphtha.
10	104	9.9	.09	.054	9.9
20	22	3.21	.32	.146	13.11
30	9½	2.14	.21	.25	15.25
40	8	1.07	.1	.13	16.33
ENGINE MOVES OUT.					
50	10	1.6	.16	.16	17.94
60	5½	1.34	.13	.24	19.27
70	4	1.34	.13	.34	20.61
80	3½	.8	.08	.23	21.41
90	3½	.8	.08	.23	22.22
100	4	1.07	.1	.27	23.29
110	3	.53	.05	.18	23.83
120	3	.53	.05	.18	24.37
SAFETY VALVE BLOWING OFF.					
130	0	1.07	.1	.18	25.44
BOTH VALVES BLOWING OFF.					

This report concludes with a statement of the fuel used in firing up to 120 lbs pressure in the same locomotive, as follows, on June 20: Half a cord of hard wood, cost $3.75; a large quantity of loose pine stuff not measured; and two tons best anthracite steam coal, cost $10; out of which about half a ton was left after reaching the above pressure. Allowing half a ton of half-consumed coal left in the furnace, say one-fourth of a ton in value, the net cost was over $10, against 73 cents for identical work with the Holland gas; 24.37 gallons of naphtha, costing 3 cents per gallon, being consumed. The boiler was specially adapted for coal, but badly encrusted with scale, to the equal disadvantage of both fuels. The difference in direct cost was more than eleven to one in favor of Dr. Holland. The following description of his fire gives some data for an explanation of this surprising, and yet often repeated and verified, result:

The maturer process attained in the experiment of April 29 (and since usual) gave no light visible by day from without the cavern where it was pent, dark and stormy as the cave of Æolus. Raging, roaring, vibrating with a vehemence that shook the iron monster and the ground beneath, and vied with the increasing din of steam from the valves above and under, it was a kind of ghostly noise as well as heat, that to the more habitual organ of perception gave no sign, but seemed causeless or supernatural. In vain we peered through the mica in the door, or peeped and dodged at the small orifice from which a scorching heat spurted fully two feet. Smoke and smell had clean vanished from all parts long before; no unconsumed carbon anywhere escaped to lend the faintest lustre; the carbonic acid formed in the retorts of course came out transparent and inodorous, and so did the hydrogen, with the product of its combustion, the invisible gas called superheated steam; in short, there was nothing of a nature to be seen or smelt in all this *melee* of nature's great elements. Even the illuminating effect of heat upon iron was lost through the expulsive pressure of gases in the retorts, which doubtless projected the flame too far to heat the thimbles that no longer enclosed it, and now stood, like all else, invisible. All ended and came to light again (save the carbonic acid) in a delicate cloud of vapor that rose from the smokestack scalding hot, but too pure to soil white cambric.

To this description it is pertinent to add the following remarkable fact since observed in the trial of the new hydrogen-burning locomotive at the Grant Works, with the small experimental flues referred to as having been afterwards condemned. The fire had been turned down low and the valves set so as to allow the steam gauge to remain stationary at 120 lbs., which it did with perfect steadiness, showing the peculiar controllability of the heat in this process. After about half an hour of this test, the experiment of turning on double water (steam) into the two water retorts was tried, and the valves were set open for this purpose, without restoring the oil feed. The fire was evidently unchecked, and no further notice taken for a few moments, the engineer sitting with his back turned toward the boiler, as the weather was cold; when a violent discharge from the safety valve suddenly caused him to jump nearly to the middle of the tender from the unexpected shock. The steam had run up to 132 lbs., with which the valve was loaded, before a change had been noticed, and so continued blowing off indefinitely, showing a rate of evaporation many times

multiplied by no other change than the addition of more water (steam) from the boiler.

The phenomena and effects of combustion above cited seem to justify the following statement of theory:

The Holland locomotive retorts liberate a pure hydrogen fuel from the mutual decomposition of certain proportions of naptha and steam. The regular temperature of the furnace keeps the retorts hot enough to disengage the oxygen of steam in the presence of the carbon of naptha, the chemical attraction of these two elements causing them to unite in the proportions of full combustion, and to form carbonic acid within the retorts. The released hydrogen is the only combustible ingredient left to issue at the burners. All the heat of both of these combustions—that of the carbon within the retorts and that of the hydrogen at the burners—is conserved and utilized in the same furnace for the making of steam.

The question now arises on the true causes of the enormous excess of calorific power developed by a given amount of fuel through the water gas process, as compared with the results of direct combustion of the same fuel. Assuming that the amount of carbon entering the retort takes oxygen from the steam with which it mingles, to the proportion of full combustion, and thus liberates just sufficient hydrogen to re-engage the same amount of oxygen; we have first to inquire what proportion exists between the amounts of heat generated by the union of that or any given quantity of oxygen with its proper complements of carbon and of hydrogen respectively. A number of authorities have determined this question experimentally, with results not widely different. According to Grassi, the number of pounds of water raised one degree by the union of one pound of oxygen with its full combining equivalents of carbon and hydrogen, respectively were 2,893 and 4,333. The direct gain by exchange, therefore, would be almost exactly fifty per cent. Numerous experiments by Bunsen and Fyfe are also said to have proved (in indirect accordance with those above referred to) that the fuel (hydrogen) obtained by the decomposition of water, yields a considerable excess of heat above that absorbed in producing the decomposition.

We have made a close scrutiny of the Holland apparatus and its operation for domestic purposes, as exhibited in this city, at the offices of the Heat, Light and Power Company, No. 18 Vesey street. An even pressure of both water and naphtha is secured by an elevated tank for each purpose, at the top of the room. The pipes running from these tanks to the cooking-stove and range are laid in full view, and strict tracing and examination of their course and connections at every point showed that there was no other possible source of supply, of any kind, for the retorts and burners. The oil tank measured 25⅞ inches in diameter, and was computed to hold nearly 2.22 gallons to the inch in depth. In running the cooking-stove, with the oven constantly at a sharp baking heat, the oil was lowered only $\frac{1}{16}$ inch in half an hour, or about one quart (¾ cent) an hour. The whole interior of the store, which had been used a year last May, was free, not only from ashes and soot, but from discoloration, which, obviously, much assisted the effectiveness of the fire, as compared with the coating of non-conducting material accumulated in using crude fuel. It was found impossible to obtain a trace of smoke or odor from the flame upon a white handkerchief; so that, of course, the usual free carbon, hydro-carbon, carbonic oxide, and other gases of crude fuel, could affect neither the atmosphere nor the flavor of food cooked in direct contact with the flame. In the large cooking-range, a third pipe is introduced for the distillation of illuminating gas, simultaneously with the ordinary use of the range. The adjustment and operation excited much admiration. In the progress of the oil through successive coils of this pipe, within the fire box, the several hydro-carbon mixtures it contains are converted, by successive gradations of heat, into a single homogeneous and fixed gas, which resists the most extreme cold of our climate, without condensation, and runs free from sulphurous and other impurities, requiring only dilution with air. After burning a scant $\frac{1}{16}$ inch of oil, the time being taken, the gas-making pipe was opened by simply turning a cock, and in exactly one half the time enough gas was made and measured to amount to 12.55 cubic feet, if diluted to twelve-candle power; when the total oil out was found to be exactly $\frac{3}{32}$ inch, showing a barely perceptible difference from the rate of consumption without making gas, but too fine to measure with the instruments at hand. Roughly allowing it to be $\frac{1}{64}$ for the gasmaking, the cost of the 12.55 feet would be .0347 gal., or about 2¾ gal. per 1,000 feet. This is ¼ gal. in excess of more exact measurement previously taken by gas experts.

But the rough experiment with the locomotive evidences a gain of fully one thousand per cent, from the exchange of carbon for hydrogen, estimating the fuels by cost, in a practical way; although the liquid fuel is of course the dearer of the two, and the gain over the intrinsic value of the exchanged carbon, if it could be ascertained, would therefore be still greater. Fifty per cent from the exchange, then, is at best but five per cent of the total gain, and the remaining 95 per cent must be otherwise accounted for. Nor is there any lack of good reasons for even this enormous difference. In the first place, the carbon is consumed in pure oxygen from steam, no atmospheric air having access to it in the retorts, and therefore the large absorption of heat by the nitrogen of the air that feeds the coal fire, is wholly saved in the water gas process. The consumption is also perfect both of the direct and the produced fuel, against a semi-consumption in the coal furnace. Thirdly, the combustion of the carbon, with all its heat antecedent and consequent, is closely confined in the retorts, from which the heat can escape only by radiation into the boiler, with the exception of the very restricted vent of the hot gases through the burners. Fourthly, the hydrogen obtained issues from the burners at a very high prior temperature, whereas the coal enters the furnace cold. Finally, the hydrogen flame is a vastly more advantageous heating agent than any form of crude fuel, from its unequalled intensity and rapidity of action, and also from its direct contact with the iron, as against the slower processes of radiation and conduction employed by the coal in the furnace. The rapidity with which heat is imparted increases in a geometrical ratio to the increase of it sintensity, and since the hydrogen flame is many times hotter than incandescent carbon, this concentrated heat must have a vastly greater effect, unit for unit, in any given time of passage through the flues. Considering that 90 per cent of direct waste is a moderate allowance in the ordinary firing of a locomotive, it would seem on the whole that we are justified in expecting yet greater economy from this process rather than in theoretically distrusting the results so far reported.

PROGRESS IN MIXED METALLURGY.

By William C. Conant.

Of the fundamental mechanic arts substantially developed before Science or History had a name, Metallurgy was the beginning and the common parent. When Adam was yet in middle life, the genius of Tubal-Cain divined and explored the capabilities of the workman's metals, copper, tin, zinc, and iron; fused and mingled them, wrought from them the tools of every craft, and became "the instructor of every artificer in brass and iron." It were hopeless, therefore, to question subsequent records of Time for the era or the occasion of any of the more essential developments in this art of arts. So far as the native surface metals are concerned, it is probable that all the more important metallurgical processes were understood, for substance, long before the flood. Copper and tin, the principal ingredients of bronze, being found comparatively pure at the surface, were naturally the earliest

metals combined and used for mechanical and military purposes. The same precedence of these metals continued, locally at least, and, so far as can be discovered, down to historic times. The Roman historians record it. Weapons, armor, and tools of bronze, hard enough for cutting granite, abound among the remains of primitive antiquity, and have given to that vague epoch the title of the age of bronze: the so-called stone age being of no particular order in time, but rather the universal age of savageism, from the earliest vagrants of the human race down to the recent aborigines, so-called, of our own country.

The disposition to try what will come of mixing materials may be assumed as a prominent native factor of invention in all ages, and as especially prominent in the infancy of knowledge, when every material and every property of materials was a mystery only to be experimented on. Nay, the bare factor of accident would be sufficient to insure the mixture of various metals, ores and earths in the very first experiments. Copper and tin coming out first, as bronze, at once stimulated and assisted an eager search for further discoveries. They built the furnaces and quarried the fuel that ultimately brought to light the treasure concealed in dull brown rocks of iron ore. The more facile ore of zinc (in the presence of copper) if accidentally at hand, would enter earlier, or, possibly, earliest, into the kaleidoscopic series of the smelter's products, with the most exciting brilliancy of effect; promising coveted gold without limit, and preparing, perhaps, the first sad catastrophe of inventive expectation unrecorded in any patent office or prospectus of incorporation. Down almost to the 19th century (1781) brass was made by mixing the zinc ore directly with copper; and down to the 16th century this had been done, from the earliest times, without a suspicion that the magical stone was anything but one out of hundreds of like mysterious minerals, among which a potent and supreme "philosopher's stone" might yet be found. No wonder that infinite possibilities of metallic splendor and preciousness stretched out before the imaginations of the alchemists in the vast field of unexplored mineral combinations before zinc was discovered as a metal and the nature and scope of the alloys were defined.

To these limitations, however, modern philosophy and discovery have given a new and again undefinable extension. They have not revived alchemy, but they have revived a more trustworthy probability, and a more philosophical pursuit of radical improvement in the character of the alloys. In general it may now be stated that the labors of new experimenters with the new lights and resources already promise to popularize little less than the beauty and incorruptibility of the precious metals in the equipage of common life and arts, and in combination with some of the qualities hitherto inseparable from a coarse, dull and corruptible texture, as in iron.

The discovery of nickel and German silver were marked steps in the direction of the new era, yet failed to approximate it; as we see in the fact that nickel has proved hitherto but a material, and German silver a basis, for the temporary and unsatisfactory varnish of beauty called plating. Nickel was found too refractory a metal to be worked solid for purposes of general utility or even of ornament. Qualified by other metals, as in German silver, the same refractory temper still rendered it impracticable, except in proportions too small to impart its clear color and splendor, with its clean, resistant texture and temperature, to the composition formed. Every workable substitute for silver betrayed, more or less, a constitutional sickliness, of jaundiced tint, sweaty feel, and corrupt odor. No actual progress was possible until the discovery that the refractory quality of nickel was due to its absorption, when in a molten state, of certain gases which might be chemically removed. This discovery was made and published in Germany, two or three years ago, with the magnesium method of purifying and reducing the metal to malleability. In practical results, however, we know of little to report as yet from the other side of the Atlantic; whether it be on account of expensiveness in the process, or inability to form alloys of this in itself too expensive metal that are satisfactory at once in cost and in qualities. One suggestion of possibly great importance comes from the German nickel manufacturers, in the comparison of the purified metal with Bessemer steel, with which it is represented to be almost identical in practical properties; raising even a suspicion in some minds whether the two may not be modifications of one metal, or of some protean metallic element, of unconjectured range. The whole range of metals, indeed, would not be more extreme, nor, perhaps, more difficult of practical reconstruction than that of carbon. The speculations and partial successes of Mr. Lockyer in the direction of a theory of unity, or rather of duality, and convertibility, in matter, are here forcibly recalled to mind; suggesting that all its families may be the progeny of two universal parent substances, such as hydrogen and oxygen. What if the truth of Nature, after all, lay, metaphorically, near to the surface and to the mystical vision of the old child-like sages, who saw in the "elements" all-generative powers, as Science finds in one of them (fire) a form, and as represented in the Sun the prime form perhaps, of all organic energy? What if Science should yet discover that the alchemists themselves had a true, if not practicable, end in view? Man's dominion in Nature is too marvellous in its present beginnings, for anything to be incredible as to its future perfection.

On the American side the modern philosopher's stone has beensought of late years, with sanguine ardor. A number of different fortunes of respectable size have been sunk in the melting-caldrons, out of which have arisen a succession of bright apparitions, only to prove intractable for use, or turn the old inevitable sickly cast after brief shining. None of our practical experimenters, so far as I am aware, have struck the true lead, the purification of nickel, with one exception. No other man of our day, probably, has given so many years of metallurgical labor so hard and practical, together with study so profound, to the assimilation of the necessary alloys for gold and silver, to the appearance and properties of those metals, as Mr. Charles Wessell of New York. What our chemists may have accomplished, or seemed to accomplish, with a few ounces of metal in the laboratory, it is of less importance to inquire than would be generally supposed. Such achievements, whatever they might be, would have no necessary value, or even validity, in practice: none possible, indeed, unless there were given with them a practical metal-mixer uniting scientific genius and research with the technical knack and stalwart physical capacity for handling metals by the ton in the furnace, to the purpose for which he intends them. Mr. Wessell, indeed, meets this description; but it would be needless to tell any manufacturer of brass or German silver, that no other known counterpart exists. The men they depend on for this service work solely by blind knack, which they catch and lose alternately in the most unaccountable manner, contributing a material percentage to the market cost of these common compositions by their own inevitable percentage of uncertainty, failure, and destruction of materials.

Charles Wessell, the metallurgist of the Holmes & Wessell Metal Company of New York, came to this city from Rome, in this State, a modest working man, whom the future famous discoverer of a genuine popular rival to the precious metals must make haste to head off, if indeed it be not already too late. It is thirteen years since Mr. Wessell began his metallurgical experiments and inventions, by undertaking successfully to electro-deposit a combination of three metals which most chemists would even now pronounce it impossible to hold together under the battery. A very distinguished chemist to whom the product was submitted, gave this assurance in absolute

terms, before making his analysis. His own analysis confounded him; he frankly certified the three metals found in the deposit; and in his subsequent lectures has referred very pointedly to undiscovered possibilities in the philosophy of metals. Undiscovered—for it is the good fortune of the solitary discoverer that the mediating agent vanishes in his thaumaturgic-metallurgic act into thin air, leaving no clue by which the scientific detective can shadow him as yet. Hence it is impossible for the metal worker, or even the chemist as yet, to recast certain of the Wessell metals; since it is too much to ask of him to give away the "combination" that locks his own hard-earned reward against the rapacity of mankind. Science itself must be content for the present with the accomplished facts shown, and with what their author can afford to disclose of methods and principles. Thus, it has become possible to give the necessary alloy to silver and gold with more suitable ingredients than could be used heretofore; ingredients which but imperceptibly deteriorate either the color or the incorruptibility of the precious metals, and, so far as silver is concerned, effect even a remarkable improvement. Stranger results have followed and are still progressively following, from the same discovery, in building up towards gold and silver from the basis of alloys. All of Mr. Wessell's novel compositions—already sufficient to constitute an era in the history of the art—have their origin and their constant method of development in that stroke of genius; for I know not what else to call it in view of the systematic unfoldings and correlations of it in the hands of the same master. The depositing battery, instead of the crucible, is the instrument by which the practicability and the effect of every combination conceived by him is tested in the first instance. As a consequence, partly of this scientific certainty in method, partly of practiced genius in adjusting heat and other delicate conditions according to bulk, weather, color, radiance, fumes, and the hundred unspoken mysteries of his art, Mr. Wessell's matured mixtures, from common brass and German silver upward, come out uniformly and infallibly what they are intended to be; astonishing to veteran manufacturers of metals who have associated him with them in their affairs, and a fact which I hazard little in opining to be without precedent in mixed metallurgy.

In the course of his novel processes of research, Mr. Wessell discovered, probably first, or at least long before it was promulgated elsewhere, the secret of making nickel pure and malleable, and not only so, but also of keeping it pure and malleable, throughout all combinations, processes and proportions, in which he chooses to introduce it. Magnesium was first or independently tried by him; but discarded for a more practical and economical agency, incidentally discovered in experimenting on qualifying or auxiliary mineral ingredients. The means also by which it turns out that the gases so fatal to nickel are kept out after being once eliminated, were a part of the general precautions of an extremely vigilant and, as it were, sensitive operator, rather than preconceived expedients for that express purpose. The methods and results are no worse, perhaps all the better, that their theory was learned from them, rather than they from the theory. It became a significant observation to Mr. Wessell, that various metals, malleable in the original smelter's ingot, grow unmalleable by remeltings. He reasoned that in the large smelting furnaces, from which the metal is drawn off at the bottom, the most of the mass is secluded from unfriendly influences, whatever these may be, until it is suddenly poured into close moulds and cooled: whereas, in the small open furnace or crucible of the foundry, the metal is poured off from a freely exposed surface; suggesting that to his own closely-covered processes was due the continued freedom of the metal from the refractory temper once extracted.

With the chronic intractability of the superior metal has been removed the hitherto insurmountable obstacle to its introduction in sufficient force to impart its noble qualities to a workable composition. It can now be used in any percentage necessary, and the Wessell process for malleability, unlike the German, is one that adds no extra expense. Its remarkable lustre and beauty of color are now as familiar as those of silver, through extensive use in electro-plating, and are rapidly approaching equal favor in the public taste. What is not so familiar to most minds, is the palpable superiority of nickel, at all points, for fine utensil service, such as we require of spoons, forks, knives, &c., for the table. Color is a matter of taste; but there is no disputing the superior durability of lustre and polish as well as of form, that belongs to the harder metal. It yields only to gold in point of resistance to oxidation and corrosion, and defies the attacks of organic acids, sulphur, &c., that instantly mar the beauty and cleanliness of the best silver. Still less commonly understood is the force of character, so to speak, with which this metal suppresses the meaner colors and weaker susceptibilities of lower metals united with it, by its own noble qualities, even when the odds in quantity are largely against it. To this we already owe solid Wessell-silver table ware, not noticeably inferior in any respect to pure nickel, yet at no greater cost than the perishable sham of plated goods. Manufacturers of the latter may not look with favor on the substitution of goods that would last four generations for goods that must be renewed four times in a generation. But such a revolution as this comes of its own weight and carries all before it. The present vast production of plated ware must in a few years become a mere reminiscence, in all its numerous departments.

To an important class of readers and interests, the bearing of Mr. Wessell's discoveries on the metallurgy of gold and silver will seem most worthy of attention. Alloys are necessary to these metals, both for mechanical and commercial reasons. It is no longer necessary, however, to impair their properties or appearance in making them workable or saleable. All grades of gold treated with the Wessell alloys are of uniform color and lustre with eighteen-karat gold, and require more than usually severe and expert testing to detect any differences whatever, between them. By way of illustration, it may be stated that the alloying compositions themselves do not oxydize perceptibly when exposed to the action of the atmosphere in cooling from the molten state, nor yet in the process of granulation. Manufacturing jewellers pronounce the alloy for gold in all respects equal to eight-karat gold itself, although there is not a particle of gold in it. The alloy for silver is a specially important improvement in the non-tarnishing quality. This may be illustrated by an incident in the experience of a leading manufacturer of sterling silver ware—the celebrated Whiting Manufacturing Company. A quantity of sterling had been made up with Wessell alloy, according to standard, 925-1000ths fine. Of the goods manufactured from this lot, a few were wrapped up with others of the same standard (uniform in all the goods of these manufacturers) but made with the usual copper alloy. After lying some twelve months forgotten and undisturbed, the parcel was met with in taking account of stock and opened. The regularly alloyed metal was found coated with the inevitable black oxide, while the original brilliancy of the Wessell-alloyed metal had barely acquired a warm tint. The writer is indebted for this information to one of the chief managers of the Whiting Manufacturing Company. The alloyed silver, electro-deposited on a spoon by Mr. Wessell, was declared pure by the testing chemist of one of our large plating establishments, who hotly called the metallurgist a fool to his face for insisting that it was or could be otherwise. Being requested to expose the spoon to the action of sulphuretted hydrogen in company with another of chemically pure electro-plate, the chemist was non-plussed by finding that while, of course, the latter was instantly blackened, the

color and brilliancy of the Wessell-alloyed silver remained unaffected. The same peculiarity has been observed by the writer personally in Mr. Wessell's low-priced nickeline metal, which holds a pure and strong lustre throughout indefinite exposure to every test that befalls (and befouls) a silver spoon in domestic use.

"INTEGRAL LUBRICATION."

Integral lubrication is an expression that has been selected to describe the effect of a lubricating element which is itself an *integral part* of the surfaces in contact and relative motion, as distinguished from a foreign or extraneous lubricant introduced between the surfaces, requiring constant renewal, and subject to displacement, consumption, waste, deterioration by heating, &c., and to various other imperfections and inconveniences.

Friction results from the resistance of particles in contact to change of position. Lubrication consists in their non-resistance to change of position, as in fluids. Within themselves, therefore, fluids have the property of integral lubrication. Interposed between solid surfaces, whose fixed particles resist change of position, fluids serve to separate such surfaces by a stratum of non-resistant or mobile particles, and thus supply *extraneous* lubrication.

The idea of establishing the lubricating, non-resistant or mobile element integrally in the bearings themselves, rather than extraneously as a distinct intermediate stratum, was the conception of Dr. Stuart Gwynn, the noted engineer and inventor, of two generations, to whom we owe the Gwynn pump and numerous other long established appliances. This idea is the basis of more than twenty patents, relating to the series of compositions by which it is realized under different conditions, all known under the common designation of METALINE.

The conception of union between the opposite properties of solidity and non-resistance, and of integrity and distinctness, in one metallic body, certainly had the boldness, as its realization showed the power, of a stroke of genius.

The important point to be reached by Dr. Gwynn, after his discovery of the possibility of "Integral Lubrication," to render it of practical value, was to make exact determinations of the effect produced on metals, their alloys, oxides, etc. by enormous pressure when they are put into hardened steel moulds of great strength. These trials extended over several years of time and under pressures from five tons or 666⅔ atmospheres to one hundred tons or 13,333⅓ atmospheres per square inch. In these trials he found, without doubt, the true law of the "*Flow of Solids.*" His determinations were finished in 1860.

This department of physics has, since then, been extensively worked by other scientists, and many of the results arrived at have been published. One of the latest of these contributions is an interesting memoir published in 1881 in the "*Revue Scientifique,*" by Mr. W. Spring, a German chemist, from which we abstract as follows:

The substances experimented with were taken in the form of fine powder, and subjected, in a steel mould, to pressures varying from 2000 to 7000 atmospheres per square centimeter. Lead filings under a pressure of 2000 atmospheres were transformed into a solid block which no longer showed the least grain under the microscope, and the density of which was 11.5, while that of ordinary lead is 11.3 only. Under 5000 atmospheres the lead became fluid and ran out through the interstices of the apparatus. Toward 6000 atmospheres, zinc and tin appeared to liquefy. Powders of zinc and bismuth at 5000 to 6000 became solid blocks of a *crystalline* fracture. Powders of soft and of prismatic sulphur were transformed into solid blocks of octahedric sulphur. Red phosphorus appeared to pass into the denser state of black phosphorus. Binoxide of manganese and the sulphides of zinc and lead in powder, *weld* when compressed, and exhibit the appearance, respectively, of natural crystallized pyrosulite, blende, and galena. A number of pulverized salts solidify through pressure and become transparent, thus proving the union of the molecules.

The common property in Metaline and the natural lubricants (fluids) is, of course, mobility or non-resistance to change of position in the particles. This property or effect, results, again, from exceeding minuteness, hardness, roundness and polish of particles; obtained in the artificial instance, by pulverization, attrition, and extreme sifting of metallic particles. It is obvious that the particles of soft or brittle substances, such as flour of wheat or dust of stone, are not capable of the rounding polish and consequent slipperiness (integral lubrication) of metallic particles; nor yet of a kindly interpenetration with the surface particles of solid metal. Hardness, also, or resistance to change of form, coupled with non-resistance to change of position, may be an essential requisite to fluidity; so that possibly the particles of water or oil may be much harder as well as finer than those even of metals. The metals, however, are generally susceptible of a degree of polished and rounded comminution that yields a very slippery product. The fluid-like mobility of small shot is a rough illustration of this condition.

The next stage of the invention is to penetrate and incorporate the solid bearing surfaces with the non-resistant or mobile particles. This is effected by two operations, one the product and complement of the other. The prepared particles are in effect compressed into frequent sockets in the bearing surface, so as in the first place to occupy directly the larger part of its area, and in the second place to allow the outer particles (slightly raised) to attach to the microscopic inequalities of the revolving journal, and so migrate, filling both surfaces with a fine permanent ingredient of particles non-resistant to change of position. The particles are forced in with great power, by running a heavy journal at moderate speed, or a light one at a swift rate, with the cap screwed tight enough to stop the machinery or twist off the journal if oil instead of metaline were the lubricant. Under such incalculable concentration of force, the particles, instead of being worked out from between the surfaces, are held and incorporated, forming new surfaces of a permanent but peculiar character. Thenceforward, the interaction of these surfaces works infinitesimal movement, or mutual yielding to each other in their numberless infinitesimal particles, which nevertheless retain permanent cohesion by the same law that unites the more fixed particles of the solid metal; a state of movement in stability, foreign to our sensible impression from solids, yet quite as conceivable as the universal molecular motion supposed to constitute heat. A mechanical union of metallic substances seems to be realized, as different in effect as in method—and yet perhaps not so different in principle—from the results of cofusion or amalgamation.

There is nothing in experience to indicate abrasion between these surfaces, except from the gradual breaking off of the high points which the microscope reveals on the surface of the most highly polished journal, projecting above the metaline surface. In the course of years of running on heavy bearings, these points (which so rapidly blacken oil where it is used as the lubricant), become dislodged in such quantity as to cover the surfaces with rigid specks looking like emery under the glass. To prevent this, it is found advisable, once in two or three years, according to circumstances, to replug the bushing or box with metaline (again projecting a hair's breadth) so as replace that which is removed. By this means the bearings improve with use and progressively acquire a higher and higher finish, such, as tested by the microscope, that it is impossible to approximate it by any other method of finishing. Running in oil, on the contrary, wears out journals and misshapes boxes. The longest periods for which journals have as yet been run in metaline—say ten years—have developed no heating or wear, if the bushings have been properly cleaned and replugged once in two or three years. A "shakeless fit" can be secured with metaline, which, as before remarked, would render movement impossible with any mere interposed lubricant. Journals in metalined bearings, under the heaviest weight, or at the highest rate of speed (as in spindles and polishing lathes)

and even hot, as in the case of calendering and laundering rolls, or coffee-roasters, run perfectly dry, the year round, without attention, without heating unduly, or being injured by external heat, without perceptible wear or loosening, and with a usual reduction of power required, as compared with companion bearings running with oil under the same conditions. Thus the cost of oil is entirely saved while the cost of power is materially diminished; the usual wear of journals and bearings is practically eliminated, while the fit is so close as to exclude dust and preserve or rather improve their round and polish; the labor of cleaning and oiling, and interruptions and bills for repairing are saved; the greasing of fabrics, goods, buildings and machinery is abolished; and the serious danger of fire from oil and the spontaneous combustion of oily waste is wholly removed.

The authority for these comprehensive statements stands in the form of numerous certificates from prominent manufacturing firms, a few of which it will be only proper to cite in this connection, using their own words. Thus: Messrs. Bagnall & Loud, the Boston manufacturers of pulley-blocks, certify that their planing machine was fitted with metaline bearings four and a half years ago, and is still running on the same at the rate of 5000 revolutions per minute, averaging six or seven hours a day. No oil has been applied, and the shaft shows as handsome a polish as could be desired.—Day, Farrington & Co., hardware manufacturers, Brooklyn, report that their emery grinder, with heavy journals running 1600 revolutions per minute, after three and a quarter years without oil or attention, required new bushings from neglect to replug with metaline, which would have kept them up indefinitely. "The journals are a shakeless fit, and run cooler than another grinder running in oil."—In the machine shops of the New York & Harlem Railroad, a circular saw and a Daniels planer had been running on metaline bearings, 1800 and 2000 revolutions a minute, respectively, for three and four years: no lubricants being used, no care or attention being given them, and no wear perceptible.—The Inman line of Atlantic steamships, have used metaline in their wharf machinery for ten years. Their wharfinger and engineer certify to having used metaline gibs on a forty-horse wharf-engine for five and a half years, without lubrication or perceptible wear: where both gibs and slides running in oil used to cut out and require replacement every few weeks or months.—The Excelsior Brick & Stone Company, Philadelphia, state that the metaline bushings of their loose pulleys—48 inches diameter, 12-inch face, 2⅞-inch bore, friction-clutch, and runing 225 revolutions a minute—are as good after four years as when first put in, and fit the shaft as well, having had no lubrication or attention whatever.—The Washington Steam Laundry, New York, state that they introduced metaline bearings for the heated rolls of their ironing-machines about four years ago; resulting in complete relief from the constant difficulty, disadvantage and expense caused by such machinery running with oil.—A number of the most prominent manufacturing jewellers in New York, give certificates to the same effect with that of Baldwin, Sexton & Peterson, who say that they have used metaline bearings for five years without lubrication, at very high speed on polishing-lathes etc., the journals running cold and with less power than others running with oil.—The Windsor Hotel, New York, after using metalined gibs for passenger elevators for several years, certifies that they are in good order and save the difficulty of keeping the well-way clean and free from the smell of oil.—One of the most extreme pressures that could be tried was that to which the leading blocks were subjected in hoisting granite and iron for the New York & Brooklyn Bridge, frequently causing a strain of four tons on a sheave. Before introducing metaline, the bushing and hardened steel rollers of a patent sheave would be cut completely out (says Engineer Collingwood) in four or five days. "Since metaline was put in, (over 18 months) we have had no occasion even to take out the pin, nor can we discover any appreciable wear."

We learn from *Iron* (London) the contents of a paper read by A. H. Bateman, Esq., F. C. S., before the British Association of Foremen Engineers and Draughtsmen. Mr. Bateman stated that, in London, there had been running, on metaline bearings, for the best part of a year, various kinds of main and counter shafting from 1½ to 3½ inches diameter, and from 150 to 450 revolutions per minute, loose pulleys as high as 700, and latheheads, 2000 revolutions. Elsewhere in England, there were five-inch shaftings and calendering rolls, under more than ten tons pressure; also, spinning frames, circular saws, planing machines, sewing machines, printing machines, cranks, bicycles, etc., running on the same material, without the use of oil. Works have been established on an ample scale in Dundee, Scotland, for metalining all kinds of machinery.

On the practical importance of this invention it seems unnecessary to enlarge, as every practical mind realizes at once that its value must be as diversified as the uses of machinery, and its desirable applications would form a catalogue too long for reading. A few of the lines of vast extent, in which beginnings or preparations have been made for applying metaline, may be noted with interest. The value of metaline to the millions of sewing machines in use suggests itself forcibly enough, from the repulsiveness of oil to the ladies who use them in contact with their carpets and clothing, and in making up rich and costly or delicate fabrics, which a spot of oil from the machine often ruins. The time taken up in oiling the machine is a burdensome tax on the operator, and the destruction of thousands of machines, through forgetfulness to oil them, is a still larger loss. Moreover, the nearly frictionless running of a metalined sewing machine yields the operator a sense of almost spontaneous motion in the instrument, and a delightful relief to the usual fatigue of propelling it; a strain which has, in fact, resulted in sad consequences to many female constitutions. No less obvious, too, is the value of integral lubrication, from its absolute cleanliness, in all machines for making and dressing fine fabrics of any kind.

Railroad journals running with oil cause daily detentions on every road, and frequent disasters by heating their boxes until the Babbitt or other metal is melted out and the train can be moved no further without great caution, delay and danger. A tragical train wreck resulted in Iowa, but a few days since, from the bursting of a wheel by a hot journal, in consequence of the exhaustion of the oil. Great numbers of men are constantly employed in examining, cleaning and oiling, and the expenditure for oil alone is an enormous amount, as well as that for replacing worn-out bushings and axles. When once a car or locomotive is properly fitted up with metaline bearings, these are all in order for one year at least, without a penny-worth further of material or labor, and without a possibility of danger or detention from hot boxes, want of lubrication, or wearing-out of journals and bushings.

Under the several patents for special applications, such as these and others, the American Metaline Company gives exclusive privilege to proper parties wishing to develop a particular use of metaline as a specialty. Metalined sewing machines are already the property of a New York company under the presidency of Madame Demorest, of fashion and pattern fame. Railway cars and engines are to be metalined by a close corporation of capitalists headed by Wm. Jennings Demorest, Esq., with a capital of $3,000,000. The application to sheaves, pulleyblocks, &c., has been taken up very successfully by Bagnall & Loud, Boston. Samuel S. Webber & Co., Manchester, N. H., have the manufacture of metalined spinning frames, &c., which has been tested thoroughly for years, and is now going into mills with many thousands of spindles. Metaline packings for steam, water and gas joints, pumps, &c., &c., are a specialty of Frank

Baldwin, 33 South street, New York. The Hopkins and Dickinson M'f'g Co,, 76 Reade St., N. Y., and Darlington, N. J., have the exclusive specialty of metalined sliding door sheaves and builders' hardware generally. But these illustrations need not be extended.

THE SUN.

By Professor C. A. Young.

To the Editor of "SCIENCE."

Dear Sir,—May I avail myself of the columns of your journal to correct a few serious errors which have come to light in my recent book on the Sun.

P. 16, near bottom.—The interval from the vernal equinox to the autumnal is 186 days, instead of 184, as stated. Of course the remaining part of the year is 179 days, not 181.

P. 44.—The earth would fall to the sun in about two months, not four.

P. 240, 241, and 279.—The candle power of the sun is given just four times too great. The figures printed express the number of candles which, distributed over the surface of an opaque globe, would give the same amount of light the sun does, each flame being considered as a small FLAT radiating surface.' But this does not express the true ratio between the sun's light and that of a candle radiating freely in all directions.

P. 271.—In the formula for the number of calories of heat generated by the stoppage of a moving body, the denominator ought to be 8338 instead of 850. The factor g (9.81^m), having been accidentally omitted. In consequence, a few lines below, another 850 becomes 8338 also, and 300° becomes about 30°

There are a number of other minor errors, which it is hardly worth while to notice here, though they will be corrected in the second edition. C. A. Young.

To the Editor of "SCIENCE."

A friend of mine who is a reliable observer relates an incident which forcibly illustrates the power of parental affection to overcome fear. The gentleman found a nest of young mice and removed them to the ground near by. The mother mouse made her appearance and carried away one of her young and while she was gone the gentleman took the remaining mice in his hand. When the mouse again appeared and could not find her young she seemed to hesitate a moment and then ran up the gentleman's clothes, took one of the young and carried it away. This was repeated until all the young were removed to a place of safety. J. H. PILLSBURY.

Springfield, Mass., *Dec.* 27, '81.

BOOKS RECEIVED.

A Treatise on Comparative Embryology, by Francis M. Balfour, LL. D., F. R. S. Vol. II. Macmillan & Co., New York, 1881.

An extended notice of this admirable work will appear later, we now simply announce that Messrs. Macmillan are ready to supply the second volume which completes the work, and we feel sure that every Biologist and Anatomist will avail himself of the mass of information included in Professor Balfour's book, which in competent hands must prove one of the most valuable aids to original work in this direction.

Elementary Lessons in Electricity and Magnetism, by Silvanus P. Thompson, Professor of Experimental Physics in University College, Bristol. Macmillan & Co., Bond St. New York and London. Price $1.25.

An Elementary Treatise on Electricity. By James Clerk Maxwell, Professor of Experimental Physics in the University of Cambridge, England. Clarendon Press Series, Oxford, 1881. Price $1.90. Imported by Macmillan, Bond Street, New York.

Students, and the many practical men who are now studying Electricity with a view to its application to the manufactures and arts, will find that these two books will exactly meet their requirements, in being comprehensive thoroughly practical and reliable. Those who cannot purchase both works, should commence with that by Professor Thompson, and follow with Professor Maxwell's as being more advanced.

The doctrine of the *Conservation of Electricity*, now growing into shape, but here first enumerated under that name, is thoroughly explained in Professor Thompson's book, and may be studied with profit by all interested in the science of electricity. This theory teaches us that we can neither create nor destroy electricity, though we may alter its distribution. According to this view all our electrical machines and batteries are merely instruments for altering the *distribution* of electricity by running it from one place to another, or for causing electricity, when accumulated or heaped together in one place, to do work in returning to its former level distribution.

Ideality in the Physical Sciences. By Benjamin Peirce. Messrs. Little, Brown & Company. Boston.

This work by the late Professor Benjamin Peirce is an admirable illustration of the fact, that a man of individuality and sound judgment may pursue the highest scientific work and still find himself in harmony with the religious sentiments of his fellow man.

A great portion of this work is devoted to a review of past astronomical research, and will be read with interest as a reliable exposition written for those who require scientific work explained in simple language.

Photographic Exhibition.—The substitution of a film of dried gelatin for the thin layer of wet collodion, which the photographer formerly employed as a vehicle to retain the sensitive salts of silver in a suitable condition on his glass plate, has involved considerable alterations in the mechanical appliances used in photography. For out of doors work, or work away from home, the photographer no longer requires to carry what was practically a portable laboratory. Not having to "develop" his pictures on the spot, he need take with him neither dark tent nor chemicals. On the other hand, he must have some provision by which his store of dry plates can be placed, one after the other, in the camera and properly "exposed" without the risk of the slightest particle of light reaching their sensitive surface, other than the light properly directed upon them by the lens. As he wishes to carry an ample supply of plates with him, and as the glass plates themselves make an appreciable burden in a long walk, it is essential that the apparatus for carrying them should be as light as possible; hence have arisen considerable improvement in the camera and its "slides." Again, the increased sensitiveness of the gelatin films makes it possible to give exposures shorter than can be affected by the hand uncapping and re-capping the lens; hence the invention of numerous "instantaneous shutters," by which exposures of a few hundredths of a second can be given, and pictures of moving objects readily secured. These are but instances of the many novel appliances which recent progress in photographic science has originated, and, besides these, there has been, during recent years, many and important improvements in the application of photography to the production of permanent illustrations for books and newspapers. All these varied applications of the art are to be illustrated by an exhibition of photographic appliances which the Council of the Society of Arts announce will be held during January and February next, in connexion with a course of Cantor lectures to be given before the Society by Capt. Abney. Full particulars of this exhibition are given in the *Journal of the Society of Arts* for last week.

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JOHN MICHELS, Editor.

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STANDARD TIME.

At present there are said to be more than seventy distinct "railroad times" in the United States; in some single cities there are as many as four, differing from each other by amounts varying from five to twenty minutes. This state of things of course involves confusion and inconvenience to travellers, and all Americans travel. In some cases it has been the cause of serious disasters.

It is beyond doubt, then, that there would be great advantages in a uniform standard of time for the whole country. Can they be secured without too much counterbalancing inconvenience and expense? We believe they can, and without any very great difficulty.

A *single* standard for the United States (and still more, for the whole world), while in many respects highly desirable, would be exposed to the fatal objection that it would bear no relation to the true local time determined by the Sun's position. Now this local time is what we must necessarily live by. Nature compels us to work by day and sleep by night; to rise in the morning and retire at evening. A time-standard which does not recognize this cannot be practically convenient, and will never be adopted. Suppose, for instance, that Washington time were made the standard for the country; at San Francisco every thing would be three hours out of joint; and though undoubtedly, such good people as live there, and always stay at home, could, after a while, become accustomed to having noon come at 3 o'clock by their watches, and other things to match; yet there would probably be some grumbling first. Changes so radical are always hard to accomplish. But, what is worse, whenever the San Franciscan journeyed, or changed his residence, he would have to unlearn all his time-relations, and begin again.

In fact, if a uniform time-standard were adopted over the whole world, all allusions to the time of day in literature now existing, such statements of the hour as are involved in almost every accurate description of an event, would become unintelligible except by a process of translation.

The late Professor Pierce proposed a plan, which, while securing most of the advantages of the uniform standard, avoids its worst difficulties. It is to adopt, not *one* standard for the country, but a series of standard, (*four* would be needed) agreeing exactly in their minutes and seconds, but differing by entire hours. We should then have Atlantic time, Mississippi time, Mountain time, and Pacific time. Since the minutes and seconds would be everywhere the same, telegraphic signals from a correct clock would be directly available for regulating the time wherever received; the difference of one or more entire hours could never cause confusion. And yet the standard time at any place need never differ more than thirty minutes from the true local time. This amount of difference, though of course in itself undesirable, is not so great as to be intolerable in view of the attendant advantages. We hardly notice the discrepancy of fifteen minutes between sundial and clock, which occurs at certain seasons of the year, in consequence of the Equation of time.

As to the time to be chosen for the standard of minutes and seconds, unfortunately there is not yet an agreement among our astronomers. Naturally enough many think it should be Washington time, just as in England, Greenwich time is used. So far as landsmen are concerned it is really a matter of almost no importance what time is selected, but with the shipping interest it is different. Almost all nations use Greenwich time on the ocean; and for this reason it would probably be best to lay aside national prejudice, and make our Atlantic time differ from Greenwich time by just five hours; this would agree with the correct local time for a meridian passing between New York and Philadelphia. The meridian of Mississippi time (six hours from Greenwich) would then pass between Chicago and St. Louis, that of mountain time would run near Denver, and the Pacific meridian near San Francisco.

The meridian theoretically dividing Atlantic from Mississippi time would nearly bisect the State of Ohio. In a case of this sort the legislature would be likely to adopt one or the other of the two times as the standard over the whole State; so that in practice the boundaries between the standards would probably follow State lines.

The establishment of some such system need not be very difficult or long delayed.

The Signal Service proposes to receive by telegraph, from such observatories as choose to co-operate, their respective time-determinations; to combine them, and then to transmit the resulting standard-time daily to every important place in the country; besides this, at every port they would drop a time-ball, at some exact hour of Greenwich time, so that navigators would be able to rate their chronometers.

At present we have a number of more or less extensive and accurate time-services run by different observatories. But the signals sent out are more or less discordant, not unfrequently to the extent of one or two entire seconds, for the simple reason that no clock can be depended on for any length of time unchecked by star observations; and such observations are sometimes prevented by cloudy weather for several days together. Since it would seldom happen that the observatories in widely different parts of the country would all have bad weather at once, the Signal Service plan would obviate the difficulty. The most serious objection to the proposal seems really to be that the observatories which now distribute time would lose the revenue they derive from the work, unless, indeed, as would be only fair, the Signal service should continue to pay them for their observations the same compensation they now receive.

If the Signal Service can obtain from Congress the small appropriation they ask for ($25,000) to carry out their plan, and if the railroad, steamboat and telegraph companies will adopt the standard time and use it *exclusively* in their business advertisements, the thing is done. The community will follow suit and hardly notice the change.

C. A. YOUNG.

PRINCETON, N. J.

THE NEW YORK ACADEMY OF SCIENCES.

December 19, 1881.

SECTION OF PHYSICS.

Vice-president, Dr. B. N. MARTIN, in the Chair.

Thirty persons present.

A specimen of acicular hornblende in quartz was exhibited by MR. W. L. CHAMBERLAIN.

The following paper was read by Prof. W. P. TROWBRIDGE:

ON THE DETERMINATION OF THE HEATING SURFACE REQUIRED IN STEAM PIPES EMPLOYED TO PRODUCE ANY REQUIRED DISCHARGE OF AIR THROUGH VENTILATING CHIMNEYS.

To ventilate a room properly requires the frequent removal of vitiated air and the introduction of fresh or pure air, the quantity, by weight, of the air introduced and rejected being equal in a given time.

If the process be continuous, and the proper amount of air be admitted and removed every hour or minute, the only other requirements are that the entering air shall be pure, that it shall be properly warmed in cold weather, either before it enters the room or by the mixture and diffusion of warm and cold air in the room; and that the introduction and removal of air shall take place by gentle or inappreciable currents in such a manner that the pure air may be thoroughly diffused throughout the room before it is removed.

These simple rules are easily stated and comprehended. It is also well understood that to produce a movement of air requires force in proportion to the mass moved and the velocity imparted to it.

The problems which arise in ventilation consist mainly in determining the position, arrangement and sizes of the passages through which the air enters and leaves, and the proper adaptation of these passages to the forces which produce the movement.

On the correct solution of these problems, too often misapplied or misunderstood, successful ventilation depends.

The various modes of producing the movement of air for ventilation are:

First.—Ventilating chimneys or flues in which the movement is caused by the difference in weight between the heated air in the flue and the cooler air outside. This requires that the air before entering the flue shall be warmed, and the heat necessary may be that due to the heat of the room when fires are necessary for warmth; or the heat may be imparted by stoves in the base of the flues, by gas jets, or by steam heated pipes.

Second.—The movement may be produced by fans or blowers or by steam jets—the latter being seldom applied.

The object of this paper is to investigate the laws which govern the ventilation when the air is heated at the base of the flues by steam pipes, the air in its passage to the flue receiving heat by its contact with the exterior surface of the pipes. As far as I am informed these laws have not heretofore been developed, and, as this system is a very simple one, capable of very extended applications, it is hoped that the following analysis may at least lead to a full discussion of the subject:

Let it be supposed that the air in a room is to be renewed at the rate of (W) lbs per second. Suppose also that it is to be rejected through a flue whose cross-section in square feet is (A), and height in feet (H). And that it is to be heated by steam coils whose aggregate exterior surface in square feet is (S)

The following notations will be used:

W. Weight of air removed per second (lbs).

H. Height of flue in feet.

S. Exterior surface of steam pipes (sq. feet).

A. Area of cross-section of flue (or flues).

T_a. Absolute temperature of external air (found by adding to the thermometric temp. Fahr. the number 459.4).

T_c. Absolute temperature of air in the flue.

T_s. Absolute temperature of steam in the pipes.

D_a. Weight in lbs. of a cubic foot of the external air.

D_c. Weight in lbs. of a cubic foot of the flue air.

V^1. The theoretical velocity of the air in the flue.

V. The actual velocity.

r. The rate in units of heat per hour, per square foot of the surface (S) (and for each degree difference between T_s and T_a) at which the air receives heat from the pipes.

k A coefficient of loss of velocity such that $kV=V'$.

p The unbalanced pressure (upward) due to the difference of weight between the column of air in the flue and a corresponding column of external air.

Then,

$$p=H.D_a-H\,D_c \text{ or } p=H\,(D_a-D_c) \qquad (1)$$

This pressure may be represented by the weight of a column of flue air of a height—

$$\frac{p}{D_c} = \frac{H(D_a - D_c)}{D_c} \quad , \quad . \quad (2)$$

and the velocity in the flue will be found from the expression

$$\frac{V'^2}{2g} = \frac{H(D_a - D_c)}{D_c} \quad . \quad . \quad (3)$$

or,

$$V' = \sqrt{\frac{2g\,H(D_c - D_a)}{D_c}} \quad (4$$

But from the Mariotte-Guy Lussac law we have—

$$\frac{D_c}{D_a} = \frac{T_a}{T_c} \text{ or } D_c = D_a \frac{T_a}{T_c} \quad (6)$$

substituting this value of D_c in formula (4) then results—

$$V' = \sqrt{2g\,H.\left(\frac{T_c - T_a}{T_a}\right)} \quad (7)$$

In this expression the theoretical velocity of flow is expressed in terms of the height of the flue and the absolute temperatures of the flue air and the external air. From formula (7) we have—

$$T_c - T_a = \frac{V'^2}{2g\,H} \times T_a \quad . \quad (8)$$

The quantity of heat transferred to the air may be represented by

$$\phi = W.\,c.\,(T_c - T_a) \quad . \quad (9)$$

in which ϕ represents the quantity of heat in units of heat per second, and c the specific heat of air at constant pressure ($c = 0.238$.)

All of the above formulas are well known. The following are believed to be new:

The quantity of heat imparted to the air may also be represented by $\phi' = \frac{S.\,r.\,(T_s - T_a)}{3600}$ in which is the quantity of heat imparted per second, and as from the nature of the problem $\varphi = \phi'$ we have

$$\frac{S.\,r.\,(T_s - T_a)}{3600} = W.\,c.\,(T_c - T_a) \quad (10)$$

or

$$T_c - T_a = \frac{S.\,r.\,(T_s - T_a)}{W.\,c.\,3600} \quad . \quad . \quad (11)$$

combining this equation with (8) we have—

$$S'.\,r.\,\frac{(T_s - V_a)}{3600} = \frac{V'2}{2g\,H.} \times T_a \quad . \quad (12)$$

and

$$S' = \frac{V'^2.\,W.\,c.\,T_a}{2g\,H.\,r.\,\frac{(T_s - T_a)}{3600}} \quad . \quad . \quad . \quad (13)$$

This expression gives the total heating surface in the pipes in terms of the velocity, the height of the flue, the weight of air discharged per second, and the absolute temperature of the external air.

If we substitute for V′ its value in terms of V, the actual velocity, we have—

$$S' = \frac{K'^2\,V^2\,W.\,c.\,T_a}{2g\,H.\,r.\,\frac{(T_s - T_a)}{3600}} \quad . \quad . \quad (14)$$

and since

$$W. = D_c\,V.\,A.$$

$$S' = \frac{K^2\,V^3\,D_c\,c.\,A.\,T_a}{2g\,H.\,r.\,\frac{(T_s - T_a)}{3600}} \quad . \quad . \quad (15)$$

another expression for S'.

These two expressions exhibit the laws of the movement of the air, giving the quantity of heating surface required under any special conditions of area and height of flue, temperature of external air, and velocity of discharge.

The constant (r) may be found approximately from the experiments of Mr. C. B. Richards, made at Colts Arms Co., of Hartford. The constant K depends upon the frictional resistance which the air encounters in its passage into and through the flues. The velocity V may be assumed, and should not be greater than four or five feet per second. The smaller the velocity and the larger the flues, the less will be the required heating surface, and the greater the economy of the apparatus for ventilation.

The following paper was read by Prof. H. L. FAIRCHILD:

ON A PECULIAR COAL-LIKE TRANSFORMATION OF PEAT, RECENTLY DISCOVERED AT SCRANTON, PENN.

The material which we shall notice this evening has naturally been regarded, on account of its associations, as illustrating in some degree the formation of coal. A brief description of that alteration of peat which has resulted in the formation of coal, is therefore desirable.

Peat results from decomposition of vegetable matter under water. The latter excludes the atmosphere and largely prevents the oxidation, which removes the vegetable debris on the upland, and which if rapid we call combustion, or if slow, decay. In northern regions peat-swamp vegetation is commonly a sort of moss (Sphagnum) which grows upward as it dies below. Great peat deposits are also produced in lower latitudes from the debris of forest trees. The great Dismal Swamp is a fine example, and in the Hackensack and Newark meadows we have examples of peat-formations of great depth, produced by the slow subsidence of the region and the accumulation of salt-marsh vegetation.

In former geological ages, immense peat deposits were produced in the vast lowlands along the borders of the continents, or at the deltas of the ancient rivers. These great swamps were frequently submerged in the sea and deeply buried beneath mud and sand. This event occurred perhaps many times in a single locality. The buried peat slowly decomposed. Much of the hydrogen and oxygen of the vegetable tissue, and some of the carbon, were eliminated. The remainder was consolidated by the weight of the superincumbent strata, and the result is bituminous coal. Thus we have the six to twenty coal beds of Pennsylvania, or the one hundred coal-seams of Nova Scotia.

The evidence that our coals are primarily formed in this manner is abundant, clear and incontrovertible. Few subjects are by our inductive science more definitely settled than this. We find these buried vegetable deposits in every stage of decomposition and alteration. Where the containing rocks are undisturbed, lying in their original positions, the coal contains a large proportion of volatile matter, and is bituminous. But where the rocks are dislocated and folded the coal is, by the pressure and heat, changed to anthracite or perhaps to graphite. The proportion of fixed carbon, or the degree of alteration, is always proportionate to the amount of disturbance which the associated rocks have suffered. Hence anthracite coal is a metamorphosed coal, just as marble is metamorphosed limestone, or quartzyte is metamorphosed sandstone. The metamorphism of coal is still going on. The escape of the volatile matter, in which the change consists, is observed in the mines, in the production of the explosive "fire-damp," and the poisonous "choke-damp."

Running from cellulose through wood, peat, and coals up to graphite we have a complete series; the difference being the loss of hydrogen, oxygen and in a less degree of carbon. This table, after LeConte, exhibits the proportions of the elements *by weight*, the carbon being reduced to a fixed quantity:

	Carbon.	Hydrogen.	Oxygen.
Cellulose	100	16.66	133.33
Wood	100	12.18	83.07
Peat	100	9.83	55.67
Lignite	100	8.37	42.42
Bituminous Coal	100	6.12	21.23
Anthracite Coal	100	2.84	1.74
Graphite	100	0.00	0.00

Anthracite coal, it will be seen contains a very small proportion of volatile matter, and graphite none at all. No two specimens of coal from different beds or areas are likely to yield upon analysis exactly the same results. This is due to differences in degree and manner of decomposition, the varying degree of metamorphism, the varying impurities, and perhaps a difference in the kind of vegetation. Anthracite coal naturally contains more ash than bituminous, because it is more concentrated, and of course peat has the least proportion of ash, simply derived from the inorganic matter of the plant.

The substance to be described was found in a peat-bog in the city of Scranton during the past summer. It has received attention from the newspaper and scientific people of the eastern coal region of Pennsylvania; and has been recently mentioned in the *Am. Jour. of Science* for Dec. by a quotation from a letter of a Scranton gentleman to the *Engineering and Mining Journal.*

Scranton lies in the midst of the Lackawanna anthracite coal-basin, which forms the northern half of the Wyoming basin. Since the financial panic of 1872 the city has grown but slowly, and a swamp lying in the midst of the city had remained unoccupied, except as an old dumping-ground for cinders from the furnaces. The city having been lately made the county-seat of the newly created Lackawanna county, this swamp was selected as the site for a court-house. In excavating for the foundations there was found a bed of excellent peat, 10 to 14 feet deep. I visited the excavation and collected specimens from depths of 3, 5, 8, and 13 feet. These specimens, of which a series are before you, were, of course, when fresh and saturated with water, several times their present bulk.

The peat from the greatest depth was highly decomposed, or very "ripe." It was fine-grained, close in texture, and although soft held its shape well, cutting like cheese. The color, when freshly cut, was a yellowish-brown, but changed rapidly to a dark-brown, almost black, in a few minutes. Upon drying, the color becomes a lighter or grayish-brown. The rock below the ripe peat is a clayey sand. This is somewhat impervious to water; but it is likely that beneath it is a more clayey bed which originally held the water and occasioned the swamp.

In the midst of the ripe peat, termed muck in the letter above mentioned, there was found, at various times and at different places, in excavating for the division walls, a substance resembling to the eye a bright coal—anthracite if you please. This did not occur in beds or layers, or in any apparent regular manner, but in irregular scattered or branching masses. You will observe in these dried specimens how intimately the coal-like matter and the ordinary peat are mingled. The two kinds cannot be separated, and it is with difficulty that the dried material can be gotten entirely pure for purposes of analysis. It shrinks upon drying, to a greater degree than the unchanged peat. Masses which I thought would afford fair-sized dry samples have nearly disappeared. The fresh material has been described as a tough jelly, which is perhaps a fair description. It was somewhat elastic, like a mass of soft india-rubber, but would break before bending greatly. I should compare it to a very firm but brittle jelly. The fracture had the lustre of a true coal, and in the dried state the resemblance is perfect. Being found in the midst of an anthracite basin, unscientific people naturally supposed from its associations that whatever bearings it might have upon coal would relate to anthracite coal, not knowing, or not remembering, that anthracite is a metamorphic coal.

Mr N. L. BRITTON, Geological assistant at the School of Mines, New York, has made approximate analyses of this altered peat, from material which I carefuly selected; also of the peat contiguous to the transformed matter (within the distance of an inch); and of the ripe peat from a depth of 13 feet in another part of the excavation. The analyses are of thoroughly and equally dried samples, and afford the following percentages:

	Moisture at 115° Cent.	Volatile Matter.	Fixed Carbon.	Ash.
1. Ripe Peat	6.225	63.875	4.625	25.275
2. Peat adjacent to 3	3.775	22 125	4.625	69.475
3. Transformed Peat	11.350	52.800	24.725	11 125
4. Transformed Peat	66.758	9.826	4.012	19.404

Number 4 is by the Pennsylvania State Chemist, as published in the *American Journal of Science.* The moisture is taken at 212° Fahr., and the analysis is evidently of the fresh material.

To obtain a fairer comparison, and if not strictly accurate, yet sufficiently so for our purpose, I have computed the percentages with the moisture eliminated.

	Volatile Matter.	Fixed Carbon.	Ash.
1. Ripe Peat	68.115	4.932	26.953 (White)
2. Adjacent to 3	22 993	4.806	72 201 (White)
3. Transformed Peat	59.560	27.891	12.549 (Pink)
4. " (State Chemist)	29.559	12.069	58.372
Bituminous Coals	30. to 60.	40. to 70.	3 to 6

From this table it will be seen that the composition of the transformed peat, number three, is about that of a very "fat" bituminous coal, that is, one containing a large proportion of volatile combustible matter, such as are desired for making gas. In number four, the volatile matter and the fixed carbon have nearly the same proportion to each other.

The very large amount of ash in these samples is to be expected, on account of the small size of the peat-swamp, which allowed much inorganic matter to be blown or washed in over the whole surface. But the varying amount of ash would indicate that the peculiar physical character of the peat was not due to the *amount* of inorganic matter. The ash of numbers one and two was white, while that of number three was decidedly pink. This color probably indicates iron; which may possibly afford a clue to the cause of the transformation. The presence of considerable iron either inherent in the mass itself, or derived from the surrounding mass by something like concretionary action would probably hasten the decomposition; bearing upon this point, the large amount of inorganic matter without iron in the peat contiguous to the transformed peat is remarkable. The physical characteristics are undoubtedly due to the finely divided state of the carbon, mingled with the water and volatile matter. But, however produced, we have here something that is apparently coal, in the midst of peat that is not yet coal.

Except as this substance illustrates a degree or phase of peat decomposition, it is not likely to have any bearing on the formation of coal. The decomposition of a buried peat, bed under great pressure probably involves the whole mass at the same time, and does not proceed by the expansion of such centres of decomposition as are here found.

Samples have been placed in the hands of Mr. Spencer B. Newberry, of Cornell University, who is making a full chemical examination.

DISCUSSION.

DR. L. ELSBERG then said that some 20 years ago he was engaged in experiments on the subject of converting peat into coal by a more rapid process than that occuring in nature. He found that moisture, heat and pressure were, as he supposed, the elements which, together with time, nature had employed; and these three factors could and can be used really to make a very good coal. On some future occasion he would bring specimens of the manufactured coal and of various kinds of coal to the Academy, and

give an account of these experiments and the methods. For a long time his experiments were futile, because it was impossible to make a machine of iron or steel strong enough to withstand the pressure which must be applied to the prepared pulp to reduce it to coal. By the action of super-heated steam, peat is converted into a perfectly homogeneous pulp. By passage of this through any of the ordinary compressing machines used for making bricks, etc., blocks or cylinders are obtained of a substance which, so far as its economic uses are concerned, is not inferior to most qualities of bituminous coal, for gas or fuel. Every effort was made to render the bore perfectly smooth and polished in the cylinder from which the peat was finally pressed out, and for this purpose even glass and porcelain were employed. However the peat was found to be so impalpable that it was forced into the microscopic pores of the metal, and even of porcelain and glass. The peat thus inserted itself in the finest possible particles which acted like wedges, chipping off small pieces from the interior of the cylinder. No matter how fine and smooth the bore of the cylinder was made, after very beautiful working for a few days, gradually this material would insert itself in the microscopical interstices of the metal, until gradually the working of the machine was stopped or an explosion ensued. A great many trials were made and much money spent, and finally the enterprise was given up.

MR. A. A. JULIEN remarked upon the voluminous literature connected with the study of peat, and the widely varying results, notwithstanding the enormous amount of labor that has been expended. The study of this material has been approached by investigators from two economic points of view; its relations to agriculture, and its employment as fuel. In investigations of the former class the larger number of analyses have been ultimate—*i. e.*, to determine the carbon, oxygen, hydrogen, nitrogen, etc., which make up peat and its allied products. This gives very conflicting results; the slightest possible change in the amount of water, the oxidation or dissociation of the material, even while during analysis, yielding very different results even in the hands of a single investigator. The other method is approximate, simply intended for the estimate of the value of coal or peat as applied to the purposes of fuel, and is that represented in the analysis of Mr. Britton. Such analysis, however, can throw but little light on the origin of the substance; organic acid seems to be further indicated by the red ash derived from the coal-like substance (Analysis No. 3), the white ash of the enclosing peat showing the residue of silica and alumina insoluble in the humus acids.

Further, the physical characteristics of the substance described by Prof. Fairchild, its brittle jelly-like character while moist, and extreme shrinkage on drying to bright coal-like brittle flakes, are identical with those of apocrenic, humic and other organic acids. These considerations render it highly probable that this substance has been produced within the peat at Scranton merely by the leaching out of the upper portions of the bog and the concentration of soluble salts of organic acids, in part crenates, along certain planes and in small cavities within the denser part of the peat toward the bottom of the bog. There is as yet no evidence, however, that these facts have any important connection with the formation of bituminous coal, much less with that of anthracite, represented by these specimens. A third method of the examination of peat is founded upon the determination of its proximate constituents or compounds, both those of amorphous character and various organic acids. From insufficient knowledge of the exact constitution and nature of these acids, especially in their various hydrated forms, the method is very difficult and has thus far had but limited application. Only such a mode of examination can throw light upon the character of the bright jelly-like substance in the Scranton peat.

Some statements by Prof. Fairchild, however, give a clue to its identity. He has mentioned a rapid change of color in specimens of the peat taken from a depth of thirteen feet, the yellowish brown color of the surface becoming blackish brown in a few moments while being handled. This seems to indicate not the trifling change produced by drying, but the characteristic reaction of crenic acid, well known to chemists by its immediate oxidation and partial conversion into apocrenic acid. This affects not only the acid but its ordinary salts, *e. g.*, those of iron, and has been observed both in its artificial product in the laboratory, and in nature, in the deposit of iron crenate beneath peat bogs and from the waters of many springs.

Prof. D. S. MARTIN called attention to the resemblance of the lighter colored and solid variety of this peat to the darker variety of the "turba" of Brazil. In the latter he had also observed thin seams of a black bituminous substance which was much like that which occurs in this peat.

The subject was further discussed by Prof. Hubbard and Mr. Parsons.

MICROSCOPICAL SOCIETY OF ILLINOIS.

The regular meeting of the State Microscopical Society of Illinois, was held at the Academy of Sciences, No. 263 Wabash avenue, on Friday evening, December 9, 1881, President Dr. Lester Curtis in the chair. After the reading of minutes and other routine business, the secretary announced the following donations:

From Dr. Schmidt, of New Orleans, one dozen slides, consisting of nerve-fibers and other Histological preparations.

"Botanical Notes" from Prof. E. J. Hill, of Englewood, Ill.

Bulletin of Microscopical Society of Belgium, and the report of the Microscopical Society, of Liverpool.

Dr. Angier, of St. Madison, Iowa, spoke in reference to some Acari which he had found under the skin of a chicken.

Prof. Burrill, of Champaign University, was introduced and spoke in reference to the poison of the poison ivy. He took some of the exudation and found it teeming with bacteria, and he questioned whether the poisoning and the bacteria come from the plant or otherwise. The speaker stated that upon examination of the workings of the leaves, he found the same forms; the milky fluid which exuded from stem contained numbers of them and the effect of placing some of this upon his arm had been attended with quite serious results.

The speaker went on to say that he had found the foregoing facts true with other plants among which he mentioned the chicory, buckwheat and dandelion.

Dr. Curtis described a new half-inch objective made by Gundlach and owned by Dr. J. Hollist. The glass was claimed by the maker to have an angle of 100°. Its angle had not been measured since leaving his hands.

It has the society screw and can be used on any ordinary stand. The back lens of the objective is large and extends beyond the border of the opening in the screw. This opening, therefore, acts as a diaphragm. In order to secure the benefit of the full aperture the portion of the objective can be removed and an adapter furnished with the Butterfield broad range screw can be substituted. It has also another screw of about the same diameter as the Butterfield screw, but provided with a finer thread, the name and description of this screw was not known. The front of the objective is ground down to a conical shape. For ordinary use this front is covered with a brass cap, having an aperture in the centre to allow the conical end of the objective to pass through. The cap can be removed when it is desired to use the objective for the examination of opaque objects. On removal of the cap the conical sides of the lens are seen to be covered with some sort of black varnish to prevent the passage of

outside light. A lieberkuhn is furnished with the glass which can be screwed on in place of the cap while examining opaque objects. The speaker had not had the glass in his hands long enough to become perfectly acquainted with all its qualities, it certainly is a good one, however. It resolves angulatum very satisfactory, and bears eyepiecing extremely well, working admirably on anatomical structures.

The lieberkuhn seems to be a valuable addition for some sorts of study as it brings out surface workings with unusual clearness, even in transparent objects. Mr. E. B. Stuart exhibited a Hitchcock lamp which he stated commended itself to the use of microscopists. No chimney is required, it being a blast lamp, the flames of which is fanned by a passage of air from the bottom, the top of the lamp driven by a noiseless clockwork. The oil well is entirely separate from the outside part of the lamp, and is kept cool by the cold blast of air constantly surrounding it. It gives a light of about a six-foot gas burner and the flame is steady and more free from flicker than gas or the ordinary carbon burner. He also showed under the microscope specimens of the gelatine-bromide plates for photographic work, that had been submitted by a photographer as imperfect. An inspection under the microscope showed three kinds of spots. One caused by dust particles which had settled on the gelatine while still soft, and as the emulsion hardened, became firmly fixed on the plate. The second kind of spots were caused by, apparently, the solvent action of some substance on the film, as it could be seen to be less dense at those points, while the third were thicker and evidently caused by carelessly spattering the emulsion on partially dried plates.

The meeting was then declared informal.

WM. HOSKINS, *Secretary*.

THE AMERICAN CHEMICAL SOCIETY.

The papers appointed to be read on the evening of the December meeting were, owing to the election of officers, omitted and therefore at the *Conversazione* held on Dec. 16 they were again brought up for consideration.

The first and second papers were "On the Separation and Estimation of Manganese" and "On a Modification of Mohr's Burette; adopting it to use in delivering corrosive solution" by Nelson H. Barton. Both of these papers consisted of descriptions of details of manipulation which the author had been lead to use in his own laboratory resulting from his experience and which under favorable considerations might be desirable to employ.

The third paper was by Mr. Casamajor and titled "Analysis of Soghum Juice" an enumeration of the results obtained by him in his laboratory with comments on them.

"A new Laboratory Filter and Aspirator" was the next paper, also by Mr. Casamajor. The apparatus referred to has recently been patented, and in the above paper it was thoroughly described and a model exhibited. The fifth paper was by Dr. A. R. Leeds, entitled "A Chemical Inquiry into the Self-purifying Power of a Flowing Stream." In this paper the complete results of the work done by Dr. Leeds for the New Jersey Board of Health were presented. It will be recollected that in a previous number a synopsis of this paper was given to the readers of SCIENCE. On the present occasion charts were exhibited showing the exact relations existing between the various estimations which were made. These were peculiarly interesting to chemists although unfortunately the entire subject of water analysis is in such a state of confusion that it is difficult to make much headway in the accumulating and conflicting mass of literature which is current on this subject. The entire paper of Prof. Leeds will be published in the N. J. Board of Health Reports. The final paper of the evening was "A New Method for the Analysis of Mustard" by the same gentleman with the assistance of Mr. Everhart. The ordinary methods given by Hassall, Blyth and others were so unsatisfactory in their results that an effort was made to produce something more definite. After some little study it was found best to separate the various constituents by different extractions with various reagents, so that an addition to the conventional determinations of moisture, oil and ash (for the mineral adulterants) extractions of alcohol and ether are made for the remaining ingredients.

M. B.

SUICIDE, an Essay on Comparative Moral Statistics. By HENRY MORSELLI, M. D., Professor of Psychological Medicine, Royal University, Turin. Being abridged from the original, as Volume XXXVI of the International Scientific Series. New York. D. Appleton & Co.

The present moment seems peculiarly favorable to the presentation of a work on the subject of suicide. Whether it be the great accumulation of financial and political crises, or the increase of mental derangements, or a fundamental change in the *morale* of the civilized races, it would seem as if a great suicidal wave was sweeping over our social horizon. The labors of Buckle, Wallace and Bagshot have taught the necessity of studying such complicated problems synthetically. The statistics of no one community, the analysis of no one cause, will suffice to explain their phenomena. Professor Morselli, fully recognizing this fact, has undertaken a study of the question of suicide from a statistical point of view, and one involving in its analysis the results of Social Scientific, Anthropological, and Medico-Psychological inquiries.

The first fact demonstrated by a careful study of statistics is the regularity and the increase of suicide in civilized countries, which finds its expression in the painful conclusion, that "in the aggregate of the civilized States of Europe and America, the frequency of suicide shows a growing and uniform increase, so that generally, voluntary death since the beginning of the century has increased and goes on increasing more rapidly than the geometrical augmentation of the population and of the general mortality."

Among individual elements serving to explain this increase of suicide, climate deserves the least prominence as a direct factor. The only ascertained fact in this direction is that in the centre of Europe on an area of about 942,000 square kilometers comprised between 47-57° of latitude and 20-40° of longitude, are found the people who manifest the greatest inclination to suicide. The least amount of suicide is found on the isothermal line of + 17.°5 C, running through Portugal, Spain, Italy, Corsica and probably Greece. That the mere feature of temperature is not a very important one, is shown by the fact that on the isothermal line of + 10° C, there is the greatest variation. In the United States for example the suicidal rate is 35 per million; in Ireland 16, in England 67, in Belgium 55, the Netherlands 35, Hanover 140, Prussian Saxony 228, Galicia 98. A more direct and constant relation is found with other cosmical influences, thus the regions of the great rivers are most afflicted by suicide *coeteris paribus*, while on the contrary marshy or excessively low lands, like the Landes in France, the low countries about the Zuyder Zee and Jutland, show a lesser proportion. That suicide is most frequent in the warm seasons, is confirmed by Morselli, this observation is a familiar one to New Yorkers. In our city a perfect suicide *furore* occurs in certain summers, and the direct influence of the heat has no doubt much to do with this as with the summer increase in violent crimes similarly the results of insanity or passion, a fact to which, however, no reference is made by the author before us. It is certainly a noteworthy fact, in which he confirms Guerry, that the maximum of suicide falls under the summer solstice, the minimum under the winter solstice.

The most interesting portion of the volume, is the one relating to the influence of race and nationality as determining the suicidal rate. We have always believed that a most important contribution to the elucidation of the problem of suicide could be made from this side of the question. And it is to be regretted that the talented writer before us has not added to the numerous tables, which render his volume, a mine of valuable information, one showing in four columns, the name of the nation, the proportion in same per million, the proportion of each form of insanity, and the suicidal rate. We believe that a noticable parallelism would be observed in these columns. The Germanic race preponderates over all others, and the German and Scandinavian branches divide the supremacy. The Anglo-Saxon stock has, however, gained by its long separation from the German mother, and its admixture with other races, for its suicidal tendency is much smaller. The Celto-Romans, on the whole, show a small suicidal rate, this increases, however, with the geographical approach to the Germanic borders, and the fact is of startling interest, that as keen an analyst as Morselli, attributes the higher suicidal rate in France and Belgium to the remote, continuous and the in modern times as persistent invasion of German elements sweeping up the valleys of the Scheldt, Seine, Somme, Meuse and even that of the Loire! The lowest suicidal rate is found among the sclavonic peoples. Morselli in this part of the work fails to refer to the fact that the Bohemians, isolated from the sclavonic parent stocks by an ocean of German States, have lost the relative immunity of suicide, just as the Anglo-Saxons have gained in this respect by separation from the "suicidal" race. The general conclusion, however. would seem to be flattering to the nations having most suicides. Savage peoples commit suicide only under the stress of hunger, but as civilization progresses a thousand new motives arise, with the mental needs. The reflection is not made directly by the author, but it can be read between the lines, that a similar reason accounts for the lesser proportion of suicides among Catholics as compared with Protestants. Judaism has a very favorable influence; but this is an exceptional instance, it being the only religion tied up in a single race. A very interesting fact, is that other conditions duly considered, the votaries of that creed which is in a great minority in a given country, show a lesser number of suicides; the reason given by Legoyt is that the intolerance of the surrounding population exercises a sort of moral coercion, making the dissenters desirous to avoid giving any excuse for harsh criticisms.

As to social influences, it is concluded from the general parallelism of suicide and criminality that a deterioration of morals is favorable to suicide. To this there are however some marked exceptions, especially in southern Italy, where grave crimes are common and suicide is rare, and a revision of the question induces Morselli to modify the conclusion ordinarily held by saying that in those countries "where crimes against property predominate, suicides are more frequent than where crimes of blood are frequent." Remarkably enough it is found, with regard to the influence of economical conditions, that it is not the exact period of economical crisis, but a subsequent one that shows an increase of suicide. The influence of the Austrian crisis of 1858–1859 was shown by an increase of suicides in 1860–1861. The Franco-Prussian war of 1870–71 led to more suicides in 1872–1873.

Without any question the most interesting part of the volume consists in its appended "suicidal" maps. These are maps of Europe and of the individual European countries, exhibiting by the intensity of shading, the proportion of suicides in the population. On glancing over the map of Europe it is seen in a moment, that the highest proportion is found in Saxony; in the neighborhood of Paris and of Vienna. It is not alone race but also the density of the population which exert an important influence here, and as the contest for existence naturally culminates in the destruction of the weak, the only advice the author is able to give as a preventive against suicide, is "to develop in man the power of well-ordering sentiments and ideas by which to reach a certain aim in life; in short, to give force and energy to the moral character."

While we venture to regard this advice as a fruitless one, believing that in view of the author's earlier conclusions expressed in the same volume, all the good advice and training that might be given would not materially change the suicidal ratio. We can only commend the perusal of the work to the reader as alone calculated to furnish an adequate conception of the vast array of useful facts gathered by its author, illustrative of many profitable lessons in sociology and ethnology. That in a treatise dealing with the statistics of so many lands and with authorities who have written in so many tongues, an occasional error should creep in, is not to be marveled at, and it is only where such errors are made the basis of conclusions that the reviewer considers it his duty to call attention to them.

It is stated, in speaking of the influence of religion on suicides, that in Saxony half the population are Catholics. The fact is that Saxony is one of the most intensely Protestant countries in the world, the stronghold of the Reformation, and a land in which the slight vestige of Catholicism (not consisting of one-twentieth of the population among its votaries), is only maintained by the court which is Catholic since the time of the libertine, Augustus the Strong. ED. C. SPITZKA.

THE SUN: by PROFESSOR C. A. YOUNG, with numerous illustrations. International Scientific Series. D. Appleton & Co., New York, 1881, pp. 321, 12mo.

It is an extremely fortunate thing when we have a book on a special subject, written by a man who has himself made capital discoveries in this subject and who, at the same time, has a culture wide enough to appreciate the philosophical relations of his special subject to science in general.

If at the same time the whole exposition is written in a graceful style, perfectly plain and easy to follow, and dignified as well, we have special reason to be grateful. Professor Young is the descendant of a line of professors, and lucid exposition is natural to him, as we find from this work. It is not necessary to say that in the other degrees mentioned Professor Young is precisely the one person to whom we should first look as authority.

There are certain things which an author can best say for himself. In Professor Young's preface we find this: "I have tried to keep distinct the line between the certain and the conjectural, and to indicate as far as possible the degree of confidence to be placed in data and conclusions."

Throughout the work we have found this carried out consistently, not as a task, but as a natural outcome of the author's method of thought.

The work opens with an introduction which treats of the sun's relation to life and activity upon the earth. In this section (page 18) the accepted beliefs with regard to the sun's constitution are laid down. This is a point of departure.

Chapter I. deals with the distance, dimensions and mass of the sun. The low density of the sun is quoted as showing the strong probability that the sun is mainly a mass of vapor or gas, powerfully condensed in the central portions by the superincumbent weight, but prevented from liquefaction by an exceedingly high temperature.

Chapter II. deals with the methods of studying the solar surface.

Chapter III. relates to the Spectroscope and to the solar spectrum in general.

On page 87 we have a table of the twenty-two elements

which are present in the solar atmosphere. Oxygen is included. Nitrogen is not. The point is here made that the elements *not* present in the atmosphere of the sun are precisely those which are most common on the earth, and Mr. Lockyer's *dissociation* explanation is given and a very full and fair statement of the reasons for and against it.

We would have been glad to see in this place an examination of a paper by Dr. Hastings in the first number of the *American Journal of Chemistry*, in which the writer attempts to show that Lockyer's hypothesis is entirely untenable, and in conflict with received kinetic theories of gases.

The fourth chapter deals with the sun spots and the solar surface. In this chapter is quoted a very remarkable account of the phenomena attending the growth and decay of a sun spot, written by that veteran observer of the sun, Dr. Peters, of Hamilton College. A foot note to page 137 suggests a most interesting research in relation to the acceleration or *drift* of the spots in longitude, and it is in such suggestions as this as well as in its general views that the book will owe its great value to the astronomical student.

Chapter V. deals with the periodicity of sun spots and with the theories as to their cause and nature. "On the whole," Prof. Young says, "it seems probable that the cause of the periodicity is in the sun itself" and is not due to external causes. The relations of sun spots and climate are discussed completely, yet briefly. Professor Young is one of the few English speaking astronomers who can keep his temper upon this subject.

In giving the various theories as to the cause and nature of sun spots, the author deserves our thanks for a few very simple diagrams, for the want of which many of us have gone astray in reading the sun spot war records in the *Comptes Rendus*.

The next chapter deals with the chromosphere and the prominences, their appearances and the theories of their formation and causes.

In dealing with the lines of the chromosphere spectrum we have two lists: First, those always present, and, second, those readily seen by suitable manipulation ("on slight provocation"). The catalogue of 273 lines seen by Prof. Young at Sherman in 1872 is not given here. The discussion of the causes of the great velocities observed in prominences on pages 211, 212 is especially interesting and suggestive.

Chapter VII. is upon the Corona—its phenomena and the theories of its cause. The figure on page 225, with its explanation on page 215, appear to the writer to give too much weight to observations of a streamer in the direction of the sun's poles at the solar eclipse in 1878. It is not impossible that such a streamer existed, but it seems at any rate very improbable in the light of the photographs given in the Eclipse Volume of the Naval Observatory.

Chapter VIII. on the sun's light and heat is a rapid survey of the important work which has been done on these subjects. The light is first considered, and an expression for the sun's light in candle power deduced.

Prof. Langley's interesting comparison of the light of a Bessemer Converter to the sun's light is quoted as showing the brightness of the sun to be over 5,000 times that of the glowing metal. The positive carbon of the electric arc is from two to four times fainter than the sun.

The light from various portions of the sun's disc is next considered, and the absorption of the light near the limb brings us to the question of a solar atmosphere.

This solar atmosphere has usually been considered as gaseous, but the author quotes Hastings' lately proposed theory that this absorption is produced by matter in a pulverulent condition at a lower temperature than the photospheric clouds and dispersed through the lower portions of the sun's true atmosphere.

"If the sun's atmosphere were removed, its brightness would be increased several times. It is almost certain that the amount of light received by the earth would be doubled; it is hardly likely that it would be quintupled."

The data as to the sun's heat are more precise; and the results of experiments (fully described) are put in a striking way. The sun would meet in a single swing of the pendulum a solid column of ice 2¼ miles in diameter and 93,000,000 miles long, provided his whole power would be concentrated upon it. What is the source of this enormous energy which amounts to something like one horse power *continuously* acting to each thirty square feet of the earth's surface? Simple combustion of any matter which we know would not suffice to keep up the the sun's heat for any length of time. The *effective temperature* of the sun is next considered, *i. e.*, the temperature which a uniform surface of lamp black of the same size as the sun would have to keep, in order to radiate the same quantity of heat as the sun itself. The results of Rossetti (18,000° Fahr), are quoted with approval. The two most important theories as to the way in which the solar heat is maintained—the meteoric theory—and the contraction theory are next examined. Both causes are undoubtedly operative. Probably the contraction of the sun is the most effective agent. If this theory be accepted then the sun has a limited future as well as a finite past, so far as we can now see.

Chapter IX. opens with a valuable table of numerical data relating to the sun—a table of statistics for the solar globe.

The constitution of the solar nucleus and atmosphere with an examination of various theories of this constitution constitutes the main portion of this chapter, which closes with the statement of some of the more important and immediate problems of solar physics.

The usually received theory of the constitution of the photosphere is given (p. 290) and the first authoritative criticism of the recently proposed theory of Dr. Hastings is given on pp. 291-294. It seems to the writer, however, that Prof. Young, in urging as an obvious objection to this theory, that whatever is precipitated at a lower temperature than is the photosphere element must increase the depth of the photosphere, has overlooked an essential point of the argument. The photosphere substance is supposed to have a much higher vaporization temperature than those of other elements, *e. g.* iron, therefore any precipitated iron belongs, not to the photosphere, but to the over-lying "smoke" envelope.

This chapter closes the work proper of which we have been able to give but the barest outline. Its chief characteristics seem to the writer to be: perfectly clear statements of the facts of observation and what is far more valuable, of the theories to be considered. These are made definite by every way possible—by lucid statements and by diagrammatic figures; candid discussion of these facts and theories in the light of the best information now attainable; and lastly the drawing of the most certain conclusions which are possible from the data, taking care in each case to give a proper idea of the degree of certainty which our present knowledge allows.

These are high excellences and make the book a most important one. In pointing them out the writer has done no more than any reader can do for himself.

EDWARD S. HOLDEN.

M. COCHERY intends to spend the surplus of the Electrical Exhibition, which is said to exceed 16,000*l.*, in establishing a research laboratory for electricity.

PROFESSOR HAECKEL is at present in Ceylon, where he is to stay for three months making a scientific exploration of the island.

STEAM AND HYDRAULIC SAFETY ELEVATORS.

In large cities, where every inch of land is precious, the modern power elevator has virtually effected for building, what the locomotive engine has effected for travel and transportation—namely, a revolution. Hotels, office buildings, apartment houses, and first-class stores, are now almost invariably carried to a height of eight or ten stories, and equipped with elevators; while a tendency is fast growing and will soon become controlling, to increase the value of third and fourth rate property in the same way, and even to eliminate the toil of stair-climbing from ordinary housekeeping. This great change in the conditions of living, together with the progressive fatality already developed, as elevators without adequate safeguards begin to wear and weaken, will soon be calling in terrible tones for legislative interference. Fortunately, there are standard safety appliances that have stood the test of every possible description of breakage and accident to which elevators are liable, during a quarter of a century past, without a single failure. The sole reason that we hear from time to time of cruel destruction to human life from the falling of elevators in our hotels and apartment houses, etc., is that there are proprietors too parsimonious, or too ignorant, to provide their buildings with the perfect and proved safeguards that are everywhere before their eyes in the standard pattern of elevators used in nearly all of the most valuable buildings. They ought to be compelled to do so, whoever may profit or lose by the requirement. Meanwhile, let us see what individuals can do to protect themselves against these people by avoidance.

The improved modern safety device, introduced by Otis Brothers & Co., in their best elevators, for some years past, is quite outside and independent of the other mechanism, and acts instantaneously by virtue of any acceleration of the standard rate of motion in the car, from whatever cause. Both arrangement and action are simplicity itself. There is an independent sheave at the top and another at the bottom of the hoist well or shaft, and an endless wire rope running around the two; the lower sheave being suspended, to keep the rope taut. The rope is connected at a proper point with the safety catches on the car, in such a manner as to run the rope as the car moves, and thus to run a pair of governor balls geared to the top sheave. If the car should commence descending faster than the rate for which the governor is set – whether by accident, by overloading, or by indiscretion of the operator—the extension of the governor balls by the accelerated motion (greatly multiplied on the governor) instantly operates a clutch on the rope which pulls out the catches into the safety ratchets, and stops and locks the car in its place. It is like an automatic iron hand, always ready to clutch and pull the rope that arrests the car, the moment it disobeys the set restriction on its rate of descent. It is literally impossible by any means to move the car downward faster than the rate prescribed.

HYDRAULIC ELEVATORS.

For the purpose most interesting to the general reader —that of passenger elevators—the very recently perfected application of hydraulic power has controlling advantages, and it is probable that most pasaenger elevators will hereafter be constructed on this principle. The best hydraulic elevators are preferred to steam for this purpose on account of the perfect smoothness of their motion, their remarkable simplicity of construction and operation, easy management, and reduced opportunities for breakage, derangement or accident. To these advantages they add that of reduced expense for motive power to the extent of the head of water available on the premises. It is not to be understood, however, that all hydraulic elevators share in this preference. Most kinds heretofore, in fact have cost more in wear and tear of ropes and other parts, and in motive power to overcome extra friction, weight, &c., than any kind of steam elevators.

We shall make it our object to put the reader in possession of the leading criteria and principles necessary for a correct judgment among different hydraulic elevators in the remainder of this article.

Generally the less desirable kinds of hydraulic elevators are made with a short cylinder of large diameter, into which the pressure of a heavy column of water is introduced at one end, urging a solid piston like that of a steam engine from that end to the other. The piston rod pushes forward a crosshead bearing on each side of it a block of multiplying sheaves or pulleys around which the wire rope (from the sheave at the top of the hoist) passes, and returns many times to and from a similar pair of multiplying blocks in a fixed position at the rear of the cylinder. As these blocks of sheaves are thus forced farther apart by the motion of the piston, lengthening each of the twenty turns (more or less) of the wire rope between them, a length of rope many times the length of the piston stroke is obviously thus taken up, and the car is hoisted an equivalent distance. It is the same in effect as winding up the rope on a drum: but it is not so favorable in mode; the friction and strain being excessively increased. Moreover, the course of the wire rope from sheave to sheave in the blocks, must necessarily cross the plane of revolution of each sheave, both in taking and leaving it, so that the edge of every groove continually and severely rasps the rope as it runs into it and out of it under a tension of tons force. In point of fact, these ropes have to be frequently renewed, not only at considerable expense, but at much inconvenience from interruption. But a worse result is their great liability to snap suddenly at some point, and not only throw the enormous tension out of balance and the labyrinth of rope and blocks into violent snarl and wreck, to the destruction of everything animate or inanimate within reach; but at the same time, to cause the car to fall the whole distance to which it may have been raised. Another objection, of course, is the constant extra cost of power for the extreme friction peculiar to this mode of multiplying motion. The substitution of a rack of cog teeth on the piston rod, gearing into a pinion wheel, and that into a geared winding drum, does not mend the matter in point of safety or economy, since it is not practicable to use a belt from so slow a motor. Some of this class of machines are made doubly objectionable by placing the cylinder horizontally. The lift weight of a vertical piston can be counterbalanced; but this arrangement makes a nett increase of friction by the dead weight of the heavy piston to be dragged on the bottom side of the cylinder. Two other disadvantages are not to be avoided or counteracted: the constant wear of the cylinder and piston out of round by dragging the latter on its under side, and the accumulation under it of sediment from the water, to assist in the work of abrasion.

We conclude with a description of the more modern and matured form of hydraulic elevator, adopted for the Government buildings, on the unanimous recommendation of a board of experts appointed by the Secretary of the Treasury, and composed of Messrs. Frederick Graff, C. E., of Philadelphia; Master Machinist Geo. A. Wilson of the Washington Navy Yard; and Chief Examiner J. B. Durnall of the Patent Office. Their decision was made after exhaustive investigation in the principal cities and manufactories; and from the fact that out of nine competing methods only one was considered worthy of mention in their report, and that in terms of almost enthusiastic admiration, the reader may judge that the relative objections and advantages are fairly stated in this brief review. Six of these elevators have been running for three years, uninterupted for repair, in the "Boreel" and "Morse" buildings, and similar ones are going up in other famous piles, such as the "Vanderbilt," "Mills,

"Kelly," &c. All of the United States buildings having elevators, and in short, nearly all the most valuable public buildings, hotels, fashionable stores, apartment houses, &c., to the number of thousands, in this and other American cities, contain specimens of hydraulic or steam elevators of the same admirable manufacture.

The new hydraulic elevator is indeed a prodigy of simplicity and automatic power, with simple gravitation of air and water for its only law and mode of action, and with a conspicuous absence of the objections heretofore observed, as well as of all others conceivable. It consists of an upright cylinder and piston, only about a foot in diameter, and half the height of the lift; two pipes and two valves. That is all, save the car with its hoist ropes and sheaves, and whatever means, natural or other, may be used to bring a head of water into connection with the cylinder. One of the two pipes is a circulating pipe which connects the two extremities of the cylinder, and affords a passage for the transfer of water from one end to the other--that is, from above the piston to below it. It is also the medium for the pressure of water from the other or hydraulic pipe; a pressure thus made at all times continuous and uniform on the top of the piston head, wherever it may be, in motion or at rest. This pressure (when not neutralized) forces down the piston, thereby drawing up the car by the hoist rope attached to the piston rod.

Let us first suppose the car at the top of the lift, and the piston consequently down at the bottom of the cylinder; or, the car stationary at any point in the lift, and the piston at a corresponding point in the cylinder. As the cylinder is always full of water, and the full head of pressure always on, wherever the piston may be, the only possible way for the piston to move in either direction is for the water to get out of its way through some outlet. To let the piston rise (pulled up by the weight in the descending car) it is only necessary to open a valve that closes the lower end of the circulating pipe, thus opening communication from the part of the cylinder above the piston to the part of the cylinder below it. This allows the water above the piston to be pressed out before it, and down and back into the cylinder under it. The steadiness and ease with which the piston follows up the receding water—which, in turn, follows it up as steadily beneath—can not be exceeded by any movement in art or nature. At the same time, the movement is graduated perfectly to the will of the operator, whatever the variation of load, by opening or contracting, more or less, the valve orifice through which water is transferred from the top to the bottom of the cylinder. No water is expended.

Finally, to force down the piston and hoist the car, the circulating valve before mentioned must, of course, be closed; but this only renders motion either way impossible, because an immoveable body of water without vent fills the cylinder both above and below the piston, and it might as well be solid iron, for the matter of allowing the piston to stir. Another of the simplest things in the world must be done, namely, to open a discharge valve from the lower part of the cylinder, when the water there, in flowing out, begins not only to make room for the descent of the piston, but to make a vacuum beneath it which brings the atmospheric pressure upon the top of the piston, in addition to the pressure of the hydraulic column, which is never withdrawn. The descent is the same perfectly balanced, steady, soft and *fluid* motion previously noticed in the ascent; graduated likewise to perfection by controlling the size of the orifice with the valve rope in the hands of the operator in the car. The symplicity of the valve motion is also very beautiful. The two valves are simply two plugs a few inches apart on one stem, fitted inside a pipe, and drawn up or down by an easy motion of the hand rope. They are so adjusted with the orifices of circulation and discharge, respectively, that while they are at an intermediate position, all motion of water, and consequently of piston and car, is blocked; if lifted, they gradually and simultaneously open the discharge and close the circulation orifice, as much or little as the operator pleases, causing and graduating descent of piston and ascent of car; or if lowered, they cut off dicharge absolutely, and open circulation as gradually as desired, causing ascent of piston and descent of car.

The multiplication of the piston motion two or three fold in that of the car (which is all that can be necessary in the highest buildings with these long-cylinder machines) is done by single pairs of sheaves, and consequently without making the ropes cross the plane of revolution of their sheaves, and therefore without special friction, as well as without special strain and wear. All moves easily, naturally, straightforwardly, imperturbably, like the silent music of the spheres. The power, unlike that of steam, is as definitely limited and as invariable under all circumstances as the weight of so many cubic feet of water, with which the entire motive apparatus is exactly filled at every moment, never a drop less or a drop more, or the space of a drop vacant. The chances of breaking anything are reduced to a minimum so remote as to be hardly more than metaphysical; and yet all the standard safety appliances stand on guard against that conceptual possibility, so that there is probably no other kind of vehicle or mode of motion on sea or land so safe as that of the new hydraulic elevators above described. It is estimated that thirty millions of passengers are now annually conveyed to and from the upper stories of buildings in the elevators recorded on the salesbooks of Otis Brothers & Co. Up to the present time, this inconceivable amount of passenger business has been performed without a single reported instance of injury to life or limb from the failure of any part of the machinery. The fact is, so far as we know, without a parallel in the history of machinery, and may well direct earnest attention not only to the general qualities, but to the special features, of these remarkable machines.

OXIDIZED OIL.

To welcome a new industry is always an agreeable task, but special interest is attached to those instances in which the application of scientific principles have contributed to the results.

We have now to record a few facts relating to a means of manipulating oils, which result in the formation of a substance which has many of the advantages and characteristics of Rubber, but which can be manufactured at a fraction of its cost.

Reduced cost in the manufacture of a staple article, where a monopoly can be secured, naturally suggests great profits, and as capitalists are now competing for the privilege of manufacturing this new material, a few words respecting its nature and properties may be acceptable to our readers.

A few years ago a man of studious habits and inventive genius noticed that around the mouth of a can of oil, the oil had acquired the property of solidity, and finding that the effect was due to the oxidation of the oil, he conceived the idea of turning this property of linseed oil to practical account for various purposes in the Arts and Manufactures.

Mr. Frederick Walton, (for that was the name of the gentleman to whom we have referred) occupied several years in studying this subject, and making practical experiments relating to the behavior of oils under various conditions, and at length arrived at such successful results as to warrant his reading a paper before the London "Society of Arts," entitled "Introduction and Use of Elastic Gums and Analogous Substances." In this paper, after discussing the sources and qualities of india-rubber and gutta-percha, he described a method which he had invented of manufacturing an artificial product, which not only possessed the principal qualities of Caoutchouc and

of the gum of the Para tree, but which was considerably cheaper and had a wider application. The principal feature in the new process was the oxidation and consequent solidification of linseed oil. He found that linseed, nut, and poppy oil possessed the property of becoming concrete on exposure to the atmosphere, and that when spread in a thin layer, on a surface of wood or iron, they dried or changed into a thin skin.

This change is of course produced by the absorption of oxygen and the disengagement of carbonic acid. The power of absorbing oxygen rapidly is inconsiderable in the crude or raw linseed oil, but is greatly increased by boiling the oil, which is best effected by exposing a large quantity of raw oil to a strong heat in a cauldron, with a small percentage of metallic oxide of lead. In this condition it is called varnish, and has a viscid character. A layer of this oil requires from six to twenty-four hours to change into a skin-like substance, according as the state of the atmosphere is more or less favorable.

One of the first materials placed upon the market by Mr. Walton as the practical result of his experiments was a new floor cloth which he called Linoleum. This material has for its basis oxidized oil, which is mixed with a tenacious substance, to which is added finely powdered cork. The material thus formed is passed between rollers and pressed upon a fibrous texture.

The advantages of Linoleum over the previous oil cloths was apparent, for it was waterproof, a non-conductor of heat, while its natural body color permitted the addition of an agreable and artistic decoration. The manufacture of Linoleum has been a great success and has realized large fortunes to its original promoters, and fifteen years after its introduction was still paying 60 per cent as a dividend.

Linoleum may described as the first and crude result of Mr. Walton's expensive experiments with oxidized oil; he had however in reserve a higher development of his ideas, and at length produced a material which is a refinement of all his previous efforts.

Lincrusta-Walton, as its name implies is made from oxidized linseed oil which is skillfully manipulated with various substances, forming a material posessing most valuable properties and its principal characteristics appear to be unique.

Unlike Linoleum, which is adapted to one purpose alone, the application of Lincrusta to the Arts and Manufactures is most varied, and we shall soon find it in every house under so many forms that its future as a staple commodity is assured. One of the most valuable properties of Lincrusta lies in the fact that although originally so soft as to receive the most delicate impressions, it hardens within a few hours and permanently remains in that condition. It is a waterproof material with a natural color of a neutral shade, which can be changed in manufacturing to almost any tint. Lastly the manufacturer, by manipulating the cement, has it in his power to produce many modifications of the material, and, as we shall presently show, can make various substances which have a very wide application, and which will undoubtedly supersede many valuable monopolies, on account of its being both cheaper, more permanent, and possessing many advantages over its rivals.

Perhaps one of the most important adaptations of Lincrusta is a new and improved covering to walls, and as such it appears to us a perfect decoration. For this purpose the Lincrusta is passed through machines which leave an elegant design *in relief* upon its upper surface, and at the same time is pressed upon a thin backing of muslin or paper. Thus manufactured and hung as a wall paper in its natural tint, it is the most beautiful mural decoration known, and when colored or hand-painted the most varied effects are produced; it may present the appearance of a rich old tapestry, or the subdued tones of stamped leather. Combined with the gilder's art the brilliant effects of embossed metal in solid relief are obtained. The only limit to its development in this respect is the art of the designer and the skill of the die sinker. Bearing in mind that combined with these advantages we have a material as flexible as leather or even rubber; resilient, standing blows without injury, enduring and tough, not easily torn, waterproof and unaffected by temperature, and withal capable of being produced at a price below even the medium quality of relief papers, and it is clear advantages are combined in Lincrusta that will make it one of the most valuable products which can be manufactured, and one which will be of universal use.

The special use of Lincrusta in the United States as a wall decoration will be at once recognized, as its warmth and resistance to damp makes its application almost imperative in the case of frame buildings which form the majority of dwellings in this country.

Of the other applications of Lincrusta to the Arts and Manufactures, our reference must be brief, for they appear endless in their variety. Among other purposes Lincrusta can be used as an excellent covering for external walls. For bookbinding it takes the place of carton-pierre and papier-mâché, and even excels leather in its capability of receiving fine and incisive ornament. Mouldings of Lincrusta can be gilded with facility, and attain a hardness equal to wood, and can be applied in this form to picture frames, cornices, panels, mantelpieces, or any kind of furniture.

For wall advertisement-placards Lincrusta has many advantages, the letters are in relief, and neither sun, rain or damp has any bad effect upon it.

Railway and other traveling cars will in future be decorated with Lincrusta, and its application in steamships is acknowledged, having been largely used in the new Cunard ship *Servia*, which recently left our port. In this instance the builders state that it gave the greatest satisfaction both to the owners and to those who inspected the ship.

Lincrusta-Walton may be applied to many other purposes than those already enumerated, but the above are amongst the most important and offer an almost unlimited field for manufacturing and commercial enterprise. In short, all decorations executed up to the present time on *flat* surfaces, that is to say *without* relief, can now when desired be fashioned *with* relief, and their artistic value and appearance may thus be considerably augmented.

Among the art exbibits now to be seen in New York, that of the Lincrusta at 41 Union Square, corner of 17th street, is perhaps the most attractive. Specimens of the material have been imported from London and Paris and are here shown *in situ* and have elicited the admiration of all who have visited the rooms. The material is not yet for sale, and the exhibit has been made merely to show the public the wonderful effects of this beautiful material, by Mr. John R. Whitley, a gentlemen who has largely interested himself in this matter, and who is now making arrangements to give the American public the benefit of this manufacture.

If our description has aroused an interest in this subject, we would simply state that the exhibit is open to all who desire to verify the facts here stated and that those who desire information which is not given in this description, will there find there ample means of learning the fullest details.

PROFESSOR WILLIAM O. CROSBY has published an excellent little manual on "COMMON MINERALS AND ROCKS," which is sold by Messrs. Ginn, Heath & Co., of Boston, at 35 cents. A complete set of minerals and rocks named in the work can be had of Professor Crosby for $1, or a more extended set of larger size, including 75 specimens, for $3. With both sets 25 cents is charged for packing.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

CUMBERLAND UNIVERSITY,
Lebanon, Tenn., Jan. 23, 1882.

To the Editor of "SCIENCE."

DEAR SIR—When experimenting with the so-called nitrogen iodine a short time ago, I met with an accident which might have been very serious. I had prepared about a grain of this compound by the action of ammonia upon iodine, and it had stood over night in a watch-glass with a slight excess of ammonia. I proceeded to wash it with water preparatory to drying it for use in the lecture-room. When washing it through the third water and stirring it lightly with a glass-rod to make the cleansing more thorough, a violent explosion took place, filling my face and eyes. I washed them as quickly as I could with water and dilute alcohol, and there followed only a slight inflammation of the conjunctiva, which subsided in a few days.

I have repeated the experiment several times, and in every case have found that when the compound stands in an open vessel for twelve hours under ammonia, it contains a compound which is explosive under water upon slight causes. What this compound is I have not ascertained. At the same time the greater part of substance remains undecomposed and is merely scattered about by the explosion. This when dry presents the phenomena of the ordinary iodine. The subject deserves further investigation,

Very truly yours,

J. I. D. HINDS,

BOOKS RECEIVED.

THE BRAIN OF THE CAT (Felis domestica), a Preliminary Account of the Gross Anatomy, with four plates, by BURT G. WILDER, M. D., Professor of Comparative Anatomy, &c., in Cornell University, &c., &c.

This is a reprint from the proceedings of the American Philosophical Society, July 15th, 1881, and is the first of a series of contributions to the knowledge of the brain of the domestic cat. The present paper is divided into four parts, the second of which is a continuation of the paper by Professor Wilder on "A Partial Revision of Anatomical Nomenclature with especial reference to that of the Brain," published in SCIENCE on the 19th and 29th of March, 1881. Part III. relates to a number of points suggested for study, in which a knowledge of the cat's brain is not sufficiently understood. The four plates are very elaborate and well executed, and describe with great minuteness all that can be seen by natural vision of the cat's brain, both externally and in section. These valuable papers by Professor Wilder promise to mark an epoch in the literature of this subject.

THE THIRTY-SIXTH ANNUAL REPORT of the Director of the Astronomical Observatory of Harvard College, by EDWARD C. PICKERING. Cambridge, 1882.

An abstract of the report will be prepared for "SCIENCE." The report is a cheering one, speaking of the enlarged resources of the Observatory, the increased number of assistants, and efficient work of all engaged in making observations or their reduction.

THE FORMATION OF VEGETABLE MOULD THROUGH THE ACTION OF WORMS, with Observations on their Habits, by CHARLES DARWIN, LL.D., F.R.S., with illustrations. Messrs. D. Appleton & Company. New York. 1882.

As this interesting work will be reviewed in this journal the simple announcement of its publication will suffice.

STUDIES IN ASTRONOMY, by ARTHUR K. BARTLETT, M. D. 2nd edition, revised and rewritten. Published by the author. Battle Creek, Michigan. 35 cents.

As an introduction to the science of Astronomy, this little book presents many advantages, the subject is well handled and presented in a very attractive form.

BULLETIN NO. 1 of the American Museum of Natural History, December 23, 1881. Three articles by Professor R. P. Whitfield, illustrated.

This publication has been produced in a form worthy of the establishment that issued it. It proposes to be one of the most valuable bulletins published by scientific institutions.

BUREAU OF EDUCATION. Circulars of Information, No. 4, 1881; Washington, 1881.

This is an exhaustive description of the work of education in France.

PROCEEDINGS OF THE AMERICAN SOCIETY OF MICROSCOPISTS. Fourth Annual Meeting held at Columbus, Ohio, August, 1881.

This publication, which does credit to the publication committee, contains several valuable papers with seven pages of illustrations, and will be noticed at greater length on another occasion.

HOW TO SEE WITH THE MICROSCOPE, by J. EDWARDS SMITH, M. D.; Duncan Brothers, Chicago.

This book has been severely handled by some critics, but in our opinion it contains more original writing than any book on the subject issued during the last two years, and, coming from the hands of a thorough expert microscopist, merits attention from all using the instrument. The work would be useless to a beginner, who should use Professor J. Phin's excellent little manual, but to one who has made some progress with the instrument Professor Smith's work will prove quite useful.

LUMINOUS INTENSITY OF THE VOLTAIC ARC.

M. Niaudet, in his excellent work, *les Machines électriques à courants continus*, gives quite an exhaustive treatise on the voltaic arc; he particularly dwells upon the arc obtained by a continuous current, the positive pole above, and the negative below and on the same vertical line. It is to this case that the following extract has reference:

"*Relative luminous intensity of the carbons.*—It is very easy to see that the light directed against the lower pole is very much greater than that carried against the top. To see this, it is only necessary to place the two hands, the one above and the other below the arc, and to observe them. The difference is striking.

M. Fontaine has taken a series of photometric measures in a vertical plane, and in all planes varying from the horizontal to the vertical above and below the horizontal plane passing through the arc.

These experiments have proved that the intensity is maximum between 45° and 60° below the horizontal plane, and that it is about ten times greater than the intensity measured at 45° above the horizontal plane. In the same investigation, M. Fontaine has compared the luminous intensities of the voltaic arc furnished by a machine with alternate currents, with those we are now discussing. The same mechanical work was employed in the production of both arcs; the intensity was the same in the horizontal plane; but the mean intensity was much less.

According to M. Fontaine, the mean intensity of the light given by the first arc is three times that given by the second."

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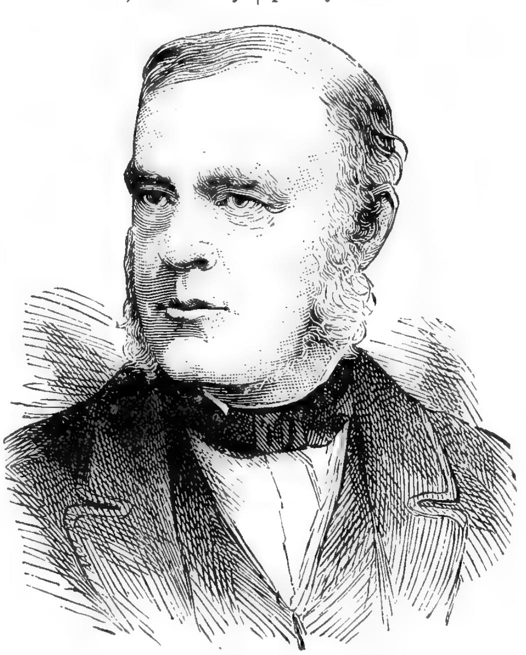
SATURDAY, MARCH 4, 1882.

JOHN WILLIAM DRAPER, M.D., LL.D.

The death of Dr. John William Draper removes from the circle of American Scientists a man who could be least spared, but the great results of his life of usefulness and devotion to science will, for many years to come, remind us of his familiar presence, and his memory will be cherished wherever the physical sciences are known and appreciated.

Born at St. Helens, near Liverpool, England, on the 5th of May, 1811, he came to this country when 22 years of age and took a medical degree at the University of Pennsylvania, and shortly afterwards he received the appointment of Professor of Chemistry and Physiology in the University of New York, where he afterwards remained until the time of his death.

Dr. Draper's scientific career may be studied with profit by all engaged in similar investigations, as an example of close application and persistent work, achieving the highest distinction in his own line of research, without any of those appeals to popular sympathy and support, which too many modern physicists are so eager to receive, and regard as elements of success.



Prof. JOHN WILLIAM DRAPER,
(Chemist and Historian) Died January 4, aged 70.

For more than 40 years Dr. Draper was quietly engaged in careful experimental researches in physiology and molecular chemistry; these researches covered a very large range of subjects, but were more particularly devoted to a study of the chemical phenomena of light in both the organic and inorganic world, a description of which may be found in his work "*Scientific Memoirs, being Experimental Contributions to a Knowledge of Radiant Energy.*" This volume is described by "*The American Journal of Science*," in its obituary notice of Dr. Draper as "a noble monument to his memory, made of the results of labors which have greatly advanced the sum of human knowledge."

In the department of spectrum analysis and photography, his original discoveries were of great value. He doubled the number of the recognized fixed lines in the spectrum, described those at the red end, demonstrated that the fixed lines might be photographed, and brought all these discoveries to bear on his investigations into the nature of flame and the conditions of the sun's surface. In conducting the long series of experiments which resulted in so many important discoveries, Dr. Draper drew largely on his private fortune, and it is asserted that no private person in America has expended more money in a purely scientific direction; his generosity kept pace with his scientific attainments, for whatever scientific discoveries he made—and they were very numerous—he freely gave to the world. He never took out a patent for any of his discoveries, nor sought to make them a source of personal profit.

In the "Scientific Memoirs," Dr. Draper claimed to have made "the first photographic portrait from the life," and he further states, "I also obtained the first photograph of the moon;" the other claims in this work to scientific discoveries are most remarkable, as the result of the work of one man's investigations in a single line of research.

In 1875 the American Academy of Arts and Sciences awarded the Rumford medals to Dr. Draper for his "*Researches on Radiant Energy.*"

Of the literary work of Dr. Draper we would speak in detail, for the subject has many attractions; but it appears unnecessary to describe books which are read

universally, and form part of the education of every liberal-minded and intelligent man. Dr. Draper's books show that he was a deep thinker in the department of the philosophy of history and human progress, and that he aimed to exalt the intellectual development of man.

The "*History of the Conflict of Religion and Science*," a work which many readers consider is incorrectly represented by the title, proves how broad and liberal were Dr. Draper's views, and it may surprise many to learn, that such opinions were not considered by him inconsistent with religious belief. The materialists appear to have written and published in vain for him, as we are told in the "*American Journal of Science*," that "it is a satisfaction to affirm that he was a theist and a firm believer in a future state of existence, for which the present is only a preparation."

On the 4th of January last, Dr. Draper peacefully surrendered his life, honored and respected by all nations, for his fame had been diffused throughout the civilized world by reason of the translation of his works into both European and Asiatic languages.

Dr. Draper leaves two daughters and three sons, the latter having already achieved distinction in pursuits kindred to the work of their father. As the work of Herschell was continued by his son, so in Professor Henry Draper we find all the special qualities for maintaining the high prestige of the family name ; his recent success in photographing such difficult celestial objects as nebulous matter, and the important discovery of the presence of oxygen in the sun, have placed him in the foremost rank of original scientific workers, and the furthur development of his investigations are anticipated with keen interest by physicists, both in this country and abroad.

VEGETABLE PATHOLOGY.

By T. J. Burrill, Ph. D.

It is not an easy accomplishment to separate physiology from pathology when, instead of dealing with the definitions of words, we attempt to classify the operations taking place within a living object, by placing them in the one or the other category of activities and effects. Indeed there is no well marked and uniformly acceptable line of division. We may speak of one as normal and healthful and the other as abnormal and injurious ; but these vary with the standpoint from which our judgment is made, and with the conditions which modify, this way and that, the results.

If these things are true as regards the vital processes of animals, they are much more evidently true concerning those of plants. In the latter, the standards of health and disease are not so well agreed upon ; less attention has been given to the life processes and their results ; the individuality of the plant has been less recognized, and its own particular good or injury less regarded. If an apple-tree produces for *us* a crop of good sized, highly flavored, richly colored fruit, we do not stop to ask whether these luscious pippins are the results of physiological or pathological operations, judged from the standpoint of the *tree*. If a cabbage has its terminal bud enormously developed so as to be called a head, the monstrosity never calls forth a compassionate word of sympathy as *we* enjoy the crisp and savory production, for a New Year's dinner. The ink with which we preserve our thoughts, flows no less freely because of a peculiar wound in a particular tree by a particular insect, and a most wonderful malformation of the growing tissues in consequence. We have not listened to the masterly disquisition of a learned and betitled oak upon *septicemia* or the curiosities of *traumatic tumors*. We have heard no complaints from suffering, bleeding grape vines ; no uneasy groans from plants perishing through the withering effects of blight and mildew.

But the terms physiology and pathology do have a meaning answering to the operations and conditions of health and disease in plants as well as in animals. The grasses of the fields and meadows may flourish in the luxuriance of bountiful supply and perfect adaption, their vital forces being attuned and harmonized into combinations of causes and consequences, all conspiring to the good of the individual and the welfare of the species ; or these members of the vegetable kingdom, may, through unfavorable conditions, through privation, through the attack of enemies, become dwarfed or distorted, weak or disproportioned, unfruitful or incapable of growth and self-perpetuation.

Without attempting to give in this place a classification of the diseases of plants, much less a description of the many that are now known and more or less clearly understood as to origin and progress, we proceed to give some account of a few of the more general facts and phenomena connected with our subject.

THE PROCESSES OF PHYSIOLOGY AND PATHOLOGY IN PLANTS ARE SLOW.

Except in the case of violently disturbing causes, like fire, frost, caustic chemicals, etc., disease or death never attacks a plant in the sudden and unheralded manner frequently known in animals. It is true that what has been called "blight," a very indefinite and loosely applied term, is usually supposed to be the work of a day or a night, perhaps of an hour ; but the facts have not been known by those who make this supposition. If by "blight" is meant the results of a tornado or even of a sirocco, with which we of Illinois can claim some acquaintance after the last summer's experience—if these are meant, we cannot say that "blight" is not sudden ; but the effects of such agencies should be classed as injuries rather than as diseases, There are really no exceptions to the rule that true pathological operations in plants are slow in their progress. The healing of wounds offers us a good illustration, if we examine the process in plants compared with that in animals. If from a healthy, rapidly growing tree, we cut off a limb close to the trunk, making a wound one inch in diameter, a whole year will scarcely suffice for complete healing, while in most animals a clean cut of this kind may be covered with newly produced tissue in a fortnight. We talk about the *circulation* of the sap ; but in plants the fluids do not circulate in any proper sense. The slow movement which does take place is at best a process of *soaking*. When water is most rapidly ascending in the stem of a leafy plant to supply the loss by transpiration, one foot per hour is more than is commonly gained ; and this is altogether exceptional speed for movements in plants generally. This feeble distribution of the fluids in living vegetation is no doubt one of the reasons for the slow workings of disease. Again the want of sympathetic

action of one part with another, so common in animal bodies, is almost or entirely wanting in plants, due probably to the absence of a well developed nervous system. Nothing is more common than to find a leaf dotted here and there with diseased parts, while the intervening tissues remain active and healthy. No organ suffers because another is affected, unless there is a direct dependence in the way of food supply or other similar reason. Hence diseased action can not be rapidly communicated from part to part. The tree never becomes flushed with fever because some one or more of its members have met with disaster. In order that disease may spread at all, it is necessary that the disease producing agent shall itself spread from its original point of attack.

INHERITANCE, OR CONTINUANCE OF PECULIAR EFFECTS.

The protoplasm of the cells is "the physical basis of life" in plants. From this flows the issues of life. Not only all other material products are secreted by the more or less plastic, often semifluid, substance known by this name, but the peculiar and unexplained products of vitality are due to the same source. Whatever potential difference exists in the seed of a thistle separating it from that of a turnip; in a bit of a twig of a Bartlett pear, used as a scion, from the worthless stock on which it is set; whatever difference there may be between health and disease, considered as a constitutional affectation, resides in this wonderful, ever-present, ever-important constituent of living plants—protoplasm.

There is nothing more wonderful in the phenomena of plant life than the peculiar tenacity with which an impression once made is held by living protoplasm. We may not be able, with all our skill, to introduce or cause a change to take place; but when an effect is produced, the changed cell may by reproduction become tens of thousands of similar cells, all having the same peculiarity of vital potency and power. It is upon this principle that horticulturists depend in the propagation of special varities of plants by grafting, budding, cuttings, etc. It is because of this mysterious but interesting quality of the "germinal matter" of the tissues of plants that Baldwins among apples, Bartletts among pears, Marechal Niel's among roses, etc., are possible.

Now any deviation from the normal character of the plant by which it is rendered less capable of succeeding in the struggle for existence on its own account, and by its own forces must be considered a disease. Usually our highly prized fruits are produced as abnormal growths, and the trees that produce them are notoriously liable to seriously suffer from enemies and unfavorable surroundings and conditions, which, to their rough, hardy progenitors would have been as the summer shower and the smiling sun.

So the blotched leaves and variously colored foliage of many decorative pets of the garden, are but indications of a protoplasmic impression continuing itself as a disease. When it happens that these disease changes of the plant are beneficial to us, or when they in any way please our fancy, we do not think of them as pathological conditions and effects; but when through the operations of the same law the opposite is true, we quickly enough talk of failure through disease. Our potatoes all go to vines with no tubers, our strawberries blossom profusely but the flowers are "blasted," our sweet corn becomes too big and coarse, our melons lose their sugar, etc. How the impressions originally occur we do not usually know, but that they are made we cannot doubt, nor too clearly see their permanence, if we would study the causes of health and disease, and try to learn how to profit thereby.

Connected with this topic is a most peculiar phenomenon not yet well understood on the botanical side and perhaps not yet adequately studied. Who can explain why it is that a certain and regular abnormal growth takes place on a given plant after the sting of a certain insect forming what is called a gall? Anyone who has seen the leaves of a jack oak ornamented with "oak apples," especially if he has broken them open and examined the complexity and regularity of their structure, can hardly have helped wondering at the peculiar something which could produce in an abnormal, diseased growth so close an imitation of a true and proper fruit. What can be more strange with our knowledge of the constancy of form and character in plant growth generally, than that a tiny wound with the injection perhaps of a minute drop of a special kind of poison by a particular insect, should entirely modify this growth and produce, not a distorted, irregular knot, but an uniform, constant and thoroughly characteristic though abnormal multiplication and shaping of cells, producing thereby an organic structure so peculiar and so uniformly the same that it may be subjected to all the procedures of natural classification and of specific identification! Though the subject has not been studied from the botanist's stand-point, especially in its physiological or pathological bearings as its importance would seem to justify and demand, it is at least questionable whether the microscope would reveal anything tending to explain the marvelous result. The structure is, like other plant tissues, formed of cells which through inherent forces and properties, rather than through external agencies, shape themselves, and by their co-ordinated and united impulses give form and character to the resulting production. It is also here as elsewhere, the living protoplasm that receives and bears the directing impulse. The cell walls passively bend and swell under its silent and incomprehensible, but dominating power. The wonder is increased when we remember that the growth is not a continued reproduction of the same thing, but that certain cells of the new structure are shaped and modified to form the external wall with its various and inter-differentiated layers, others to form the core or nucleus and still others widely differing from any of the former to make up the mediary parts of the gall. What subtle influence, what magic power is it, that thus toys with the vitalized forces and substances of the plant? What invisible barrier turns the usually inflexible current of life from its healthful and appropriate course and converts the onward rush into swelling pools with their own peculiar currents and eddies and waves, and self-controlled depths and boundaries? A gall produced by a plant in obedience to a particular act of an insect is certainly a most remarkable thing, and merits the closest and most profound study. Why should not man be able to effect as great a modification in the growth of a tender plant, as a buzzing insect? If we knew how why should we not gather grapes from thorns and figs from thistles?

But we must not lose sight of the fact that so far as the plant is concerned, a gall is a disease and sometimes a very serious one. If there is anything whatever, in plant pathology to support Dr. Lionel Beale's theory of "disease germs" being the degraded but still living cells of the ordinary tissues, it is this of insect galls. Is it not possible that a careful study of the latter might be of service to the specialist in gaining more and better knowledge of the origin and development of cancer in the human body?

PLANT DISEASES ARE DUE TO SPECIFIC AGENCIES.

There is no more important item of knowledge connected with vegetable pathology than that each disease has its own predisposing cause, or, in other words, that each disease is a specific thing itself, in the same sense and manner as a particular plant belongs to a species bearing relations to, but unlike every other species.

There is no clearer illustration of the truth of the foregoing than in the matter just before us of insect galls. Entomologists have given these structures much attention, and it is found as easy to recognize the gall as a species, as it is the insect that causes the growth. A skillful specialist in this matter will give us the name of the

insect on seeing its domicile, as readily as upon seeing the winged inhabitant itself. Each one of the hundreds of these curious structures differs from every other one, and owes its existence to a different agent from that of any other one. There are a very few possible exceptions to this if we limit the difference in insects to specific distinctions, for it is known that at least one species produces several kinds of galls on the same parts of the same plant, while others make somewhat different galls on different parts of a given plant, as in the case of the devastating *Phylloxera*. But every one knows that the individuals of a species vary much among themselves, so that our rule should be strengthened rather than broken by these apparent exceptions. There are at least one hundred kinds of galls known upon oaks; hence we may say there are one hundred specific agents, each working after its own fashion and producing its own peculiar results.

Much might also be said of diseases of plants caused by insects which do not form galls, illustrating the same thing; but these are passed without remark to save time and room for those more particularly falling under my own observation and the subjects of more personal investigation, viz: those caused by parasitic fungi.

Though having had a very good chance to find out, I do not know of a single flowering plant in our country which is not more or less injuriously affected by one or more fungi, living as parasites on, or in, its substance. Sometimes numerous species dwell on (or in) one host plant; sometime the same parasite preys on many kinds; but very often a particular fungus is found only upon a particular supporting plant. Nearly all of these myriads of thieves are so small that they cannot be seen, certainly are not usually seen by the unaided eye, except as they occur in masses. Many are at times exceedingly destructive, as witness the wholesale rotting of potatoes during certain seasons, while yet in the ground and attached to the stems of the plants; and the dreaded rust of wheat, which apparently cuts off the farmer's returns for a year's labor in a week or a day. We pay less attention to the diseases and death of uncultivated plants, but these, too, suffer as severely. I have observed great areas of thickly springing smart weed (Polygonum Pennsylvanicum and P. hydropiper) destroyed almost as effectually as by fire, by a vegetable parasite (Helmin thosporium) whose corrosive action caused the leaves, long before frost in autumn, and before the maturity of the plant, to shrivel and die, after which the entire plants soon succumbed.

Though these mischief-makers are usually invisible to untrained and unaided eyes, their peculiar effects are ordinarily recognizable at once, by an expert, even without his magnifier. Each has its own characteristic influence upon the host—one causing yellowish spots on the leaves, another a curled and distorted growth, another little cracks on the stems, another swellings like insect galls, etc., etc. A closer examination brings into clearer relief the different injuries or modifications of growth, caused by each different parasite. Each faded spot, each tumid projection, each rupture of the epidermis, each blister and canker, each puncture and corrosion, has its own more or less clearly marked characteristics; and each parasite has, as well, its own pathological influences and effects. The grapevine alone has at least thirty species of parasite fungi peculiar to it and all more or less injurious, while an entire book of some hundred and fifty pages has been filled with generic and specific descriptions of fungi, known to occur on the cultivated vine. (Thümen, Die Pilze des Weinstockes, Wien, 1878.) The list of specific and separate causes of disease in plants thus immeasurably exceeds that known to the wisest practitioner of medicine for man, for the illustration from the grapevine, though a strong example, is not exceptional. Of fungi, as a class, there are many more species growing in Illinois than there are flowering plants, putting the native and introduced together,—at least double as many. Large numbers of these, however, never grow on living plants; they are the scavengers of the vegetable world.

Two questions may now be raised. (1.) Are all these fungous growths really divisible into good specific forms? in other words, can the term species be applied to these various productions in the same sense as it is used for the higher plants and animals? (2.) Do those kinds known to inhabit living plants really cause disease, or are they mere concomitants of pathological conditions, due to other influences?

These questions are continually pressing for answer, founded on careful observation and skillful investigation. The first one has been, and must be, answered unhesitatingly in the affirmative by every botanist who has made, or may make, a special study of the plants. There are curious alternations of generation, as among the lower animals—a given species presenting itself under two, three, or even four forms. There are also deviations from the recognized type, and modifications due to circumstances and conditions; but it is doubtful whether these fluctuations are greater than in the species of higher and better known organisms. It does require, perhaps, keener perceptions to distinguish allied species than among those having greater differentiation of parts, as root and stem and leaf and flower and fruit; but none of these affect the general question. We admit the probability of evolution of species in the world, and should be theoretically disposed to look for greater plasticity in these low forms than in the higher; yet, except in the particulars cited, observation does not apparently support the deduction. I am sure that any botanist, equally familiar with the two, will as positively recognize *Puccinia graminis*, the rust, or, rather, one of the rusts of wheat, as he will *Triticum vulgare*, though the former is very variable for a fungus. The same may be said for *Ustilago maydis*, the smut of corn, and *Zea mays*; but, in this case, there seems to be no variableness nor shadow of turning in the characteristics of the parasite. Hundreds of even better illustrations might be given, all conspiring to enforce the opinion upon the skeptical, that these low, mostly microscopical plants, have specific distinctions as characteristic and rigid as those found among the higher organized, if not more respectable and reputable members of the vegetable kingdom. It is yet to be proved whether or not hybrids ever occur among the fungi; the very fact that none are positively known, or even reported as suspected, helps to indicate the goodness of the species to botanical eyes. Still there is much to be done in the way of experiment, by sowing spores and watching the development of the plants, before much confidence should be placed in the slight distinctions now used in many cases for specific separation.

Passing to our second question, a direct answer cannot be given. In the interdependence and complexity of relations existing among all living organisms, it is exceedingly difficult to pronounce upon the exact effect of any one of them, considered in and of itself. Cold water is not regarded as poisonous to man, yet individuals have severely suffered, even died, from the effects of a reasonable (so far as amount is concerned) draught. Arsenic is poisonous, yet there are those who swallow what would ordinarily be deadly doses, with impunity. Poisonous doses of opium are merely nerve restorers to the habitual eater of the drug. Shall we then say that water causes death when taken to allay thirst, and arsenic and opium are not poisonous? Shall we not rather say that whatever proves seriously injurious to man in a normal condition, under the usual circumstances of his life, is the poisonous thing, and the one which *causes* death; while we assign the peculiar and abnormal condition as the cause in other cases,—as the over-heating instead of the water.

Measured by this standard there are many parasitic fungi which must always be acknowledged as a "cause"

of disease in the higher plants, for they germinate and grow under the usual conditions of our summer weather, and penetrate and develope in and at the expense of otherwise healthy plants. Under these conditions it is only necessary to place the matured spores on the parts of the plants inhabited by the fungus to ensure its growth, and, in consequence, the disease. It is, however, even in these cases, evident that much must depend upon the peculiarities of the weather, etc., whether the host or the parasite is specially favored or repressed, and so whether or not the disease is seriously injurious.

Rust spores on young wheat leaves in spring time are as certain to germinate and penetrate the tissues as arsenic is to poison mammals. In this case development goes on but slowly however, unless specially favorable conditions occur for the parasite, when, in the latter case, it makes its presence easily recognized by the disastrous results too often witnessed. Smut in wheat is less affected by peculiar states of the climate. The spores send their germinal tubes into the tissues of the seedling plants; the fungus grows with the host, and finally, just before harvest time, matures its spores again in the aborted wheat grains. Blight-bacteria, again, need only to be introduced in few numbers into the living bark cells of a healthy pear tree, during ordinary Summer weather, to insure their reproduction and multiplication in myriad numbers, and the death of the invaded cells in consequence of their deleterious action. It is by on means true that plants must be in an enfeebled condition that such parasites may grow upon them. The very vigor of the host often adds, by furnishing more assimilable food, to the extreme development of the parasites.

On the other hand there are many fungi that only grow on the higher plants when these have been injuriously affected by something else, or when the conditions are peculiar and altogether unfavorable for their proper development or growth. Thus apples become "scabby" by a fungus belonging to the preceding class, but they often rot while hanging on the trees through the effects of other fungus never injurious to perfectly sound fruit with an unbroken skin or epidermis. Peaches rot upon the trees under the effect of a mould-like fungus which produces myriads of spores that readily float like dust in a dry atmosphere, but these do not germinate except in moisture, and, as their duration of vitality is very short, few succeed in reproducing the plant except during rainy weather, when one decaying peach may be a source of contagion for hundreds of others. There are too great numbers of leaf-dwelling fungi which only grow upon these organs when from old age or other causes they have lost their powers of existence through the diminution of their vital forces, so that the mycologist learns to look upon old and fading leaves for numerous specimens. In the descending scale we find vast numbers of still other fungi which only grow upon really dead organic matter; these however have no share in the title parasitic.

It may therefore be concluded that, in the struggle for existence, many species of fungi have acquired the physiological power of overcoming the defensive forces of certain higher plants in a state of health under ordinary conditions of plant life and growth, while others, truly parasitic in their nature, are obliged to seize upon favorable chances to take advantage of slight or serious misfortunes happening to their hosts, thus giving the kick to one already going down hill.

I have thus endeavored to point out some of the general truths of vegetable pathology as they appear to one who accounts himself a student but not a master of the subject. I enter the open gateway of a great field, and make little incursions here and there, gathering now and again from the abundance offered, material for many odd hours of microscopical work, which again furnishes "food for thought" when the lamp has been extinguished and the scalpels laid away. There is much room for many better workers, and much interest for those who will work.

CROTON WATER OF NEW YORK.

It is admitted on all sides that an improved supply of water for New York city, both in regard to quantity and quality is imperatively demanded by its citizens, and the subject in one form, will shortly be discussed by the legislature at Albany.

In regard to quantity, the solution appears a simple one, as the present supply is adequate for all legitimate purposes; in fact, if it were not for the great waste of water now practised, the supply would exceed all demands of the present population.

It is claimed by the "*Sanitary Engineer*" that this waste is due to imperfect plumbing, and the facts and figures given, show that such a supposition is, in part, correct. Every householder, however, knows that a wasteful use of the water, due to the whole supply of the city being at the command of every individual, must lie at the root of the evil.

Much printers ink has been wasted in printing proclamations from the authorities to the people, counselling economy in the use of the water, but the time has, perhaps, now arrived when the legislature should decide to employ some remedy and make radical changes in the method of distributing the supply.

The method of running the main supply direct into every house, is certainly the most primitive and least scientific or practical of all means at command. It is an inviation for waste and extravagance, and has proved an utter failure, as the means for thus distributing a supply are so defective that, while one family in a house can draw on the Croton river at will, others, less fortunate, on another level endure a constant water famine.

The whole evil of this imperfect distribution of the water could be remedied, if the supply were made by cisterns only. This system has always been in use in London and answers admirably. Every householder under this system has one or more cisterns filled twice daily, and is not restricted either to the number, capacity or location of the cisterns. Thus each householder pays *pro rata* for the actual amount of water he consumes annually, which, beyond doubt, is the only equitable method of charging a water rate. In the case of manufacturers a meter is substituted, if desired.

A natural accompaniment of this system is a universal high pressure of water throughout the city, which provides that cisterns in the highest part of every house shall receive its supply daily. This mitigates the evil under the present system, of pumping and carrying water above the first and second stories, now necessary in most houses in New York city.

The economy of the cistern system is self evident for no one would call for more water than he could legitimately use and increase the annual water tax. Thus a premium for economy rather than waste is offered. In a sanitary point of view many advantages are attached to the use of cisterns, as the large amount of impurities have time to subside and the water is consumed in an improved condition. It is usual to construct one of the cisterns with slate, which is reserved for drinking purposes.

The plea that such a system curtails the proper use of water has no foundation in fact. The writer lived in a house in London for many years, under this system of supplying water, and found he received not only abundance for family use, but sufficient to water a large garden. If the system here described were put in practice in New York city, and the plumbing perfected, the present supply of water would be found ample, and part of the money now proposed to be wasted in making new storage reservoirs might be profitably used in building pumping stations, which would give a high pressure of water to all the upper rooms in the city, and increase the efficiency of the means now at command for extinguishing fires.

In regard to the quality of the Croton water, we will give an analysis by Professor C. F. Chandler.

CROTON WATER—GRAINS IN U. S. GALLONS. (CHANDLER).

Soda	0.326
Potassa	0.097
Lime	0.988
Magnesia	0.524
Chlorine	0.243
Sulphuric acid	0.322
Silica	0.621
Carbonic acid	2.604
Organic and volatile matter	0.670

Calculating 100,000,000 gallons as the daily average water supply of New York city, the impurities would be as follows:

CROTON WATER. (CHANDLER). IMPURITIES IN 100,000,000 GALLONS.

	TONS.
Soda	2.319
Potassa	0.692
Lime	7.038
Magnesia	3.742
Chlorine	1.735
Sulphuric acid	2.300
Silica	4.429
Carbonic acid	18.600
Organic and volatile matter	4.785

We have the same authority for stating that the organic and volatile matter occasionally reaches 1.14 grains to the U. S. gallon.

It will thus be seen that the constituents of Croton water show it to be excellent as a water supply to a city, being unusually free from mineral matter and

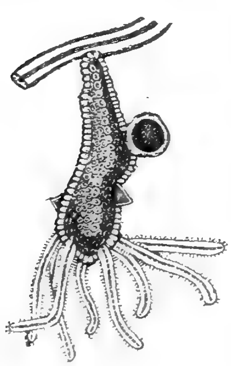


FIG. 2.
Hydra magnified.

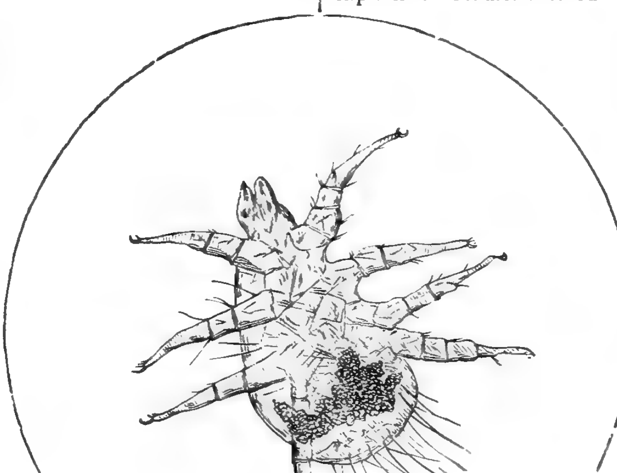


FIG. 1.
A parasite from some, bird or animal and foreign to the water. Seen for two weeks in countless numbers. They were all dead and surrounded by a jelly-like mass of putrescence.—(Michels.)

having no inorganic substance in excess to make it objectionable.

In discussing, therefore, the quality of Croton water it will be sufficient to confine our attention to the amount of organic matter it contains, and its nature.

To decide this point correctly a comparison of the analyses made by various chemists is desirable, but the greatest difficulties are here met, which are due to the erratic methods of those who have made and recorded these investigations. No two chemists appear to adopt similar methods of making analyses of water, and it is notorious that different methods give quite different results. On the other hand some chemists record their results in United States gallons, some in imperial gallons, and others in parts of 100,000,000, and others in 1,000 gallons.

From necessity rather than choice we will make an average amount of organic matter in Croton water from the following calculations:

	Tons in 100,000,000 gallons.
C. F. Chandler	4.785
E. Waller	18.324
Leeds	27.070
Herald	14.528
Average Tons in each day's supply,	16.176

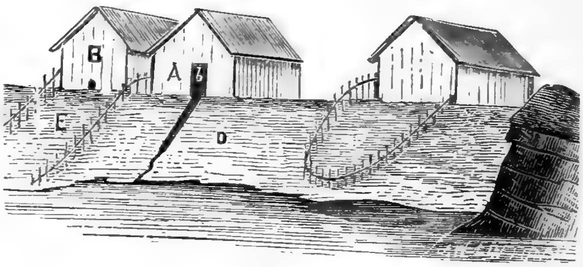


FIG. 4.
Whitlock's Slaughterhouse, showing drainage into Croton water supply.

To determine the nature of this organic and sedimentary matter a microscopical examination is necessary, and, as we have for nearly ten years continuously made such examinations, an attempt will be made to explain the results arrived at.

If a glass of Croton water freshly drawn be held up to the light, it will be noticed, that dispersed throughout

FIG. 3.
Hydræ natural size.

the water are very minute particles in countless numbers, which revolve as the water circulates round the glass, while here and there may be noticed (especially during the summer and autumn months), larger pieces having a reddish-brown color. If a piece of sponge is tied over the mouth of a faucet, and the water allowed to run for a short time, the sponge will be loaded in every pore with an accumulation of the impurities in the water. If the sponge is now squeezed in a glass, the water will be found to be opaque on account of the large amount of floating matter, it will emit a foul odor, and the resulting sediment will have the appearence of a foul blackish-brown slime. If a pipette is now used and some

of this deposit placed on a glass slide covered with a thin glass cover, and examined under a microscope, the field will be opaque with the dense nature of the impurities, but if diluted with a little fresh water, objects such as are drawn in figures—and—will be observed. These forms, which will be readily recognizable by microscopists, are composed of unacellular plants, very beautiful in form and mostly of a brilliant emerald green color. A nimallife is also well represented by forms usually found in stagnant ponds, from the purely microscopical forms to those visible with the naked eye; the hydra (figure) with its tentacles ready to grasp as its prey, the little crustaceans which are darting about.

It is no part of this paper to describe these forms specifically, but the sanitary effect of their presence will be referred to in general terms. To prevent any misinterpretation I would state that the forms shown are not seen at a single view, but the contents of the circles represent forms which are all present in the Croton water, and may be seen by making several examinations of the deposit at different seasons. While such forms may be noticed, *the bulk of the deposit* is found to be composed of dead, rotten, and decayed matter (omitted from illustration to make place for more interesting forms) from which the living chlorophil has long disappeared; the larger fragments are of a dirty brown color, in some of which a growth of fungi may be noticed, indicating its antiquity. On an average from six to eight tons or even more of such contaminations mixed with dead, effete matter, is mingled with each day's supply to the city, and when at its worst gives the water that fishy, sickening odor, which in the height of the summer is always present in the Croton water.

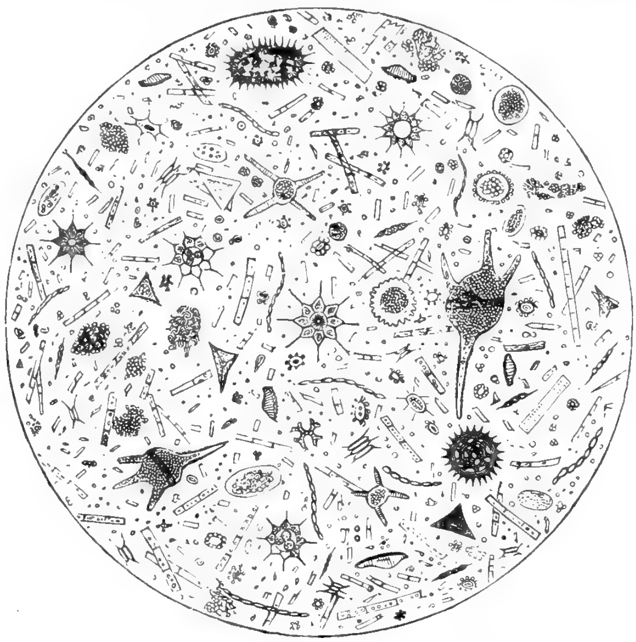


Fig. 5.
Living vegetable forms from the Croton water—(Michels.)

With the exception of the eggs of entozoa I consider most the living forms I have noticed in the Croton water to be perfectly harmless in their living state; but they are continually undergoing a process of decay, die, and when dead they are very offensive in their odor, and when present in such numbers as found in Croton water, must contribute to foul the water.

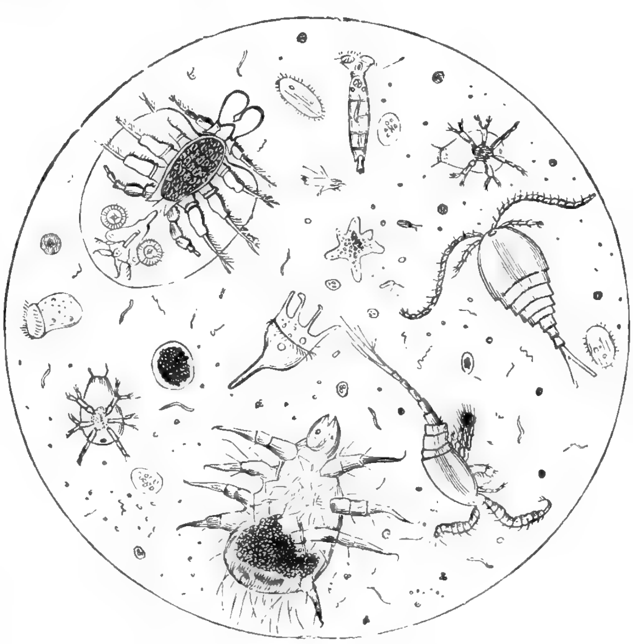


Fig. 6.
Animal organisms found in Croton water.—[Michels.)

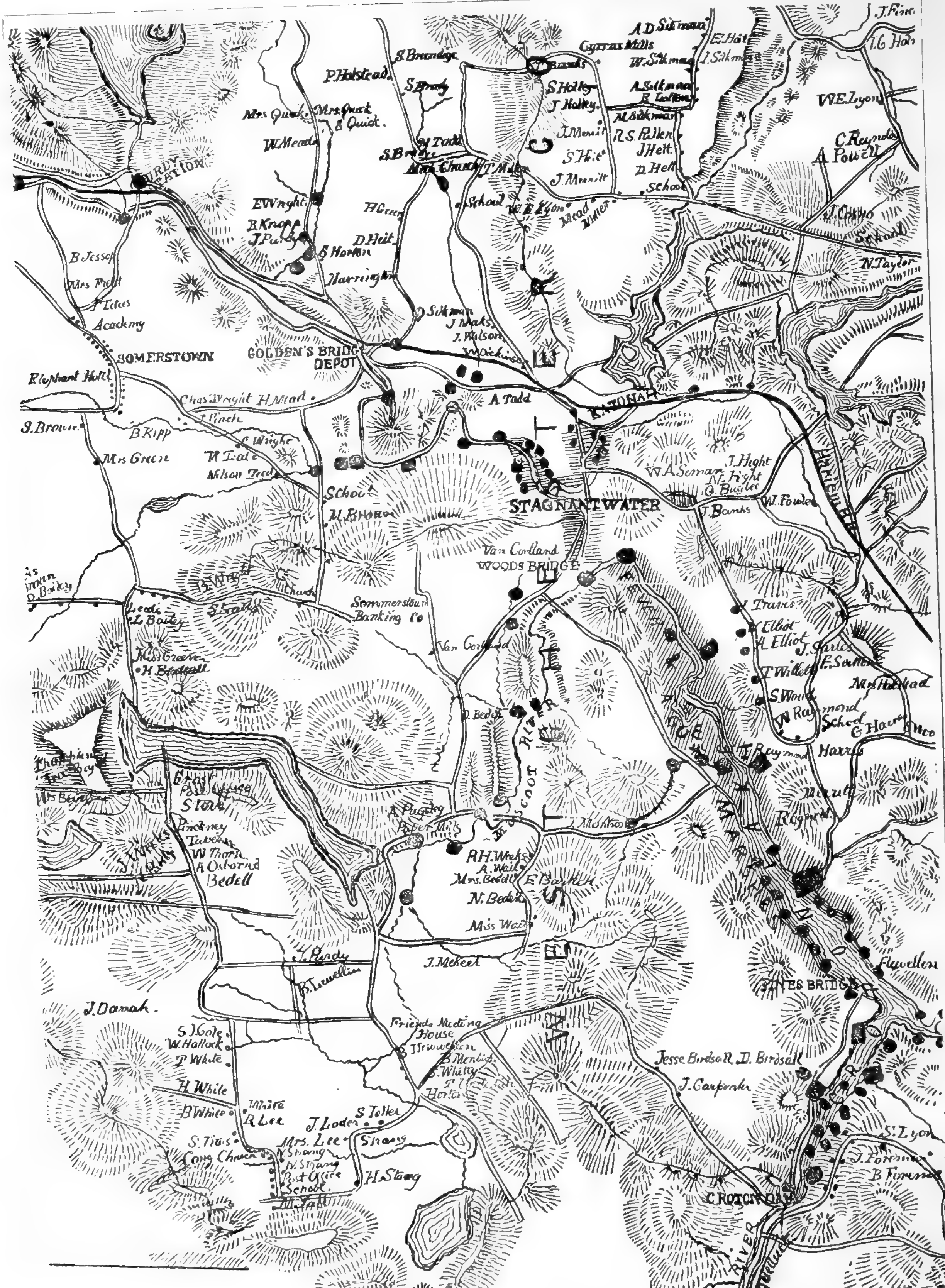
As I have stated, the bulk of the deposit is dead effete matter, forming a stinking slime which is repulsive in its nature, and must be dangerous to use as food. If collected in a spoon no person would swallow such black putrid slime; are therefore the conditions improved or changed when it is thinly dispersed in finely divided particles, so as to make its presence barely visible?

Where does all this filth come from which poisons the water of New York city? In answer to this question, a very brief description of the condition of the Croton water shed will be given, showing the condition of the source of the supply.

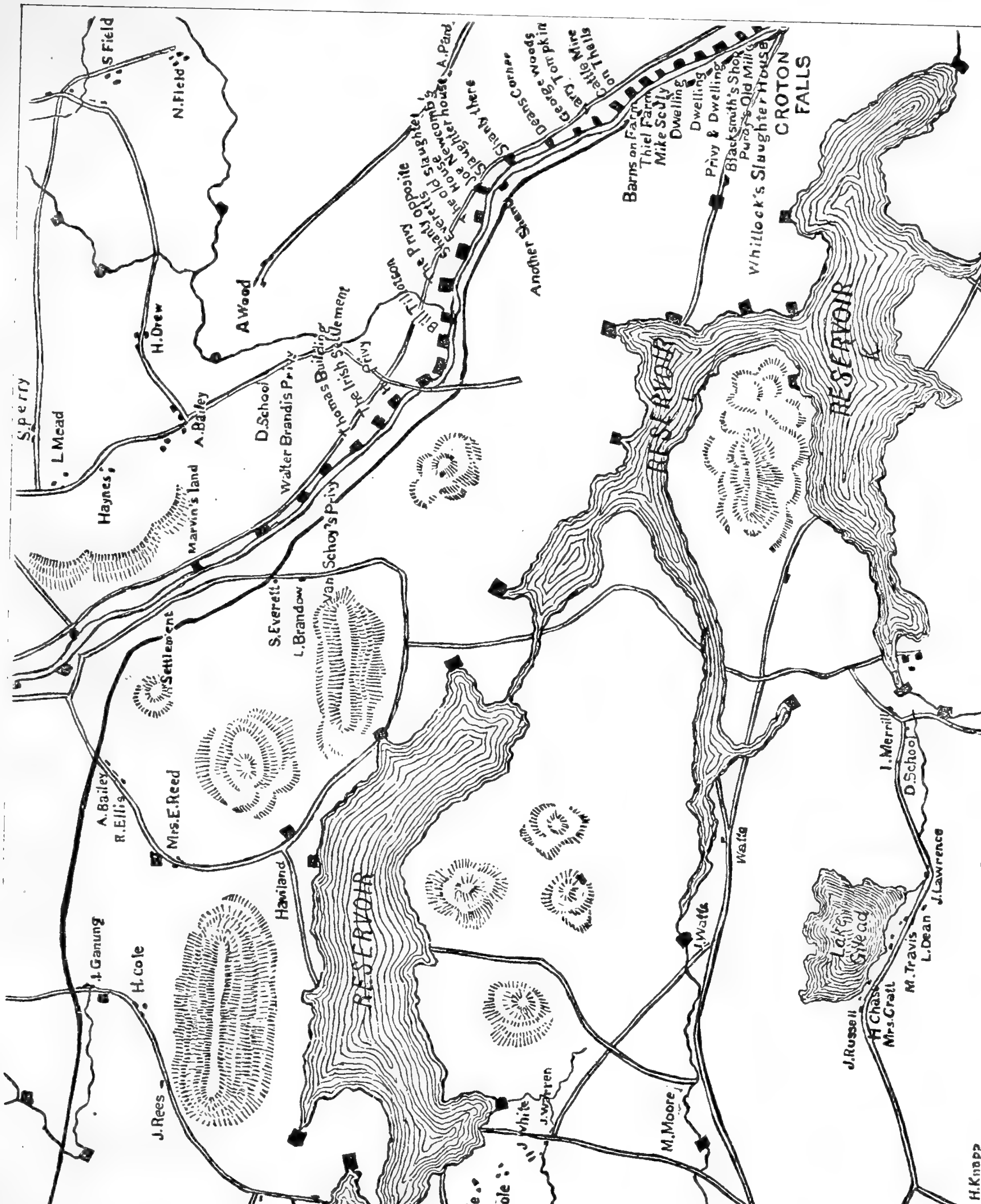
Croton water is the result of collecting the rain fall drained from a large extent of country covering forty miles, which is eventually stored in a series of reservoirs and lakes for future use. The borders of many of these lakes are very shallow and loaded with aquatic plants, and thus brought under the influence of the sun, which destroys the vegetation and converts it into a putrid deposit, which is broken up by the action of the water into fine particles, and eventually delivered at the faucets in our city.

Not long since the Croton lake and the source of the supply up to Croton dam was thoroughly surveyed by Mr. Robert Morris, for the New York *Herald*, whose report was confirmed later by Mr J. Y. Culyer, Engineer, of Prospect Park, Brooklyn, in a letter to the New York *Tribune*, and the *Sun*, *Times*, and *World* have all contributed to expose the state of things we shall now describe.

The report we refer to, states that even Croton Lake wore a green and stagnant look and its shores were sedgegrown, marshy, and laved by little streams that dripped down from barns, houses, hog-pens and farm yards. Various parts were covered with slimy grass, decomposed vegetable matter, and in parts the water was covered with a thick scum. Around other lakes he found stagnant water, fever and ague swamps, filthy drains, wayside sloughs, and on their banks cattle pens and dirty yards. In one place near Mr. Hyde's house, a hollow was found where every kind of rural filth had accumulated and decayed. On pushing his cane into the mass, a stench was stirred up that made him glad to give up further exploration in that direction,



MAP OF CROTON WATER SHED. SOURCE OF IMPURITIES MARKED BY BLACK SPOTS.



PART OF MAP OF CROTON WATER SHED. IMPURITIES MARKED BY BLACK SPOTS.

In another place right across the whole face of the lake stretched half a dozen islands, affording no foothold for man or beast, surrounded by stagnant green water filled with every conceivable vegetable rottenness.

The sewers from farm houses, cottages of laborers and factories were noticed to draindirectly in the water supply; in fact the source of water supply of New York city was found to be a common drain for about 300 cattle yards, dwelling-houses, factories, pig-sties, slaughter-houses, and other sources of impurities, every one of which are distinctly shown on the maps we present, the exact location being indicated by a black spot. Space will not allow us to give futher evidence on this point which it is in our power to offer, but we present a cut of one of these sources of pollution, showing the direct drainage into the Croton water.

Of the danger of drinking such water full of the vilest contaminations we will not dwell, each reader can take his own course, but those who are prudent will both boil and filter it before using for drinking purposes. Individuals and journals still claim that the source of the supply is free from contaminations, and the water pure and fit for drinking purposes; to be consistent they have to say that the water is wholesome.

Professor Leeds of the Stevens Institute recently showed that the Croton water contained more organic volatile mattert han the water supply of Newark, which is taken direct from the Passaic with all the sewage of Paterson and other towns. He found the organic matter in 100,000 parts in New York water to be 6.50, Newark 6.00, Hoboken 4.50. At the February meeting of the American Chemical Society (see page of this number), Dr. E. Waller, of the New York Board of Health, endeavored to deny this startling statement, by producing analyses of his own, showing quite different results. We understand that at the March meeting of the same Society, Professor Leeds asserted to the satisfaction of the Society that Dr. Waller's methods were bad and had led him to error, while the integrity of his own analysis was established.

We consider the method of storing the water supply of a city in shallow, marshy lakes, in fever and malarious districts, to be wrong in principle, and that a radical change in the management of the water supply of New York City, rather than an expensive extension of it, to be the most prudent course to adopt at the present moment.

ELECTRIC CONDUCTION AND DISCHARGE.

By F. E. Upton.

The question of the nature and the vehicle of the electrical discharge is an important one, and its determination will contribute greatly to the solution of many interesting problems in cosmical physics. It is desired in this article to call attention to some recent advances that have been made in this direction.

The view that the phenomenon is one of pure conduction, though it has received the attention of eminent physicists, can be said to be no longer entertained.

When a conductor is made to connect two poles or electrodes which are at a different potential, it is well known that the greater the cross section of the conductor, or in other words, the more of the conducting material is laid bare by a cross section, the *less* resistance will be offered to the union of the electricities of the two terminals, and the greater will be the ensuing current, with a given E. M. F.

Now, in the discharge, the contrary is observed directly. This characteristic of conduction is absent when the discharge takes place; in a tube containing air, the greater the pressure (above a certain inferior limit), or the more of the conducting material there is laid bare by a cross section, the *greater* will be the resistance to the passage of the spark, and the nearer together the terminals will have to be brought to effect a spark with a given difference of potential. Sir Wm. Snow Harris, in 1834, made an attempt to grasp at the law governing the relation of the length of spark to pressure; and he then stated that the length of spark is in the simple inverse ratio of the pressure. Gordon, in 1878, made a series of experiments to test this law, (Elec. and Mag. II. 55-62). He found that from a pressure of about eleven inches to that of the atmosphere, Harris's law held approximately good.

Representing resistance by r, and matter laid bare by cross section by s, in the case of conductor $r = \frac{1}{s}$; in the case of discharge 92 of $r=s$, approximately. Thus there is in question two entirely different order of phenomena.

Another distinctive characteristic of conduction will be recognized in the fact that whenever there is any conductor at all, however small and however long it may be, connecting two poles, some degree of current will flow, as long as there is any difference of potential. With discharge, however, a certain lower limit of distance apart of poles, and of interposed matter, is requisite for any current at all, and when that limit is reached the spark passes, instantaneously, and the discharge commences.

Whether the current passes by conduction or discharge, heat is equally developed; in the conductor in the one case, and in the interposed matter in the other. This common development of heat does not in any way assimilate the two phenomena. The condition of affairs in the two cases will perhaps become obvious if recourse is had to the corresponding hydraulic analogy.

Imagine a pond of water held in place by a dam, with a pipe leading from the bottom of the dam, for the purpose of drawing water from the pond. The smaller that pipe is in section, the smaller will be the current of water flowing through it under a given head, and a certain amount of heat will be developed by friction of the water against the interior of the pipe; moreover some degree of current will flow as long as the pipe has any size of cross section at all. That corresponds to conduction. Now let the pipe be imagined closed to the exit of water; as long as the dam is sufficient, no current at all will flow; but suppose the dam be diminished in thickness gradually and constantly, a point will be eventually reached when it will no longer suffice to hold back the water, which will break through the impediment; the friction of the water against the fragments of the dam, and of those fragments against each other will develop heat as in the first case. That corresponds to the discharge.

By this analogy the difference between conduction and discharge is clearly apparent. A conductor between two points at a different potential never offers any *resistance* to the passage of the current, strictly speaking. Instead of saying that a slender wire offers more resistance than a thick one, it would give a better understanding of the matter to say of the latter that it offered a freer passage to the current than the former. In the case of discharge, on the contrary, the matter interposed between the points acts always as a bar, or resistance to be overcome, and the more there is of it the more resistance. It is never an aid or way.

Mr. E. Goldstein, in the *Annalen der Physik*, describes an ingenious experiment bearing upon this point, which, if not conclusive, is entitled to some consideration. In a discharge tube which was filled with dry nitrogen, he placed a little sodium, which could be vaporized by heating. The positive light had a purplish red color, but in the vicinity of the sodium it was of a golden yellow. By careful heating and manipulation, the upper half of the tube could be kept red and the lower half yellow. Now the tube was brought over and near, in a horizontal and equatorial position, to a powerful magnet. The discharge light was repelled as a slender thread to the opposite (upper) side of the tube; but it was a pure eddish thread, and showed no trace of sodium yellow.

So the sodium was not displaced or repelled under the influence of the magnet, as it would have been had it been a *conductor* of electricity.

There has been considered the possibility that metallic particles thrown off from the electrodes might be the conductors of the current. To determine if this were the case, Mr. Goldstein made use of a tube with platinum electrodes, in which the light from the kathode was deflected by a second kathode. The light alone underwent this deflection, while the minute particles torn off from the platinum, which lodged on the opposite wall of the tube and formed a sort of mirror there, went exactly to the same point after the deflection of the light, as before. There was thus no connection between the light of discharge and the abraded metallic particles.

But the most elegant demonstration in this matter has been furnished by the experiments of De La Rue and Müller: they arranged that the discharge of 2,400 chloride of silver cells should pass through a circuit consisting of a vacuum tube and a large variable resistance, R—; now with different resistances R_1, R_2, the resistance of the vacuum tube formed a varying fraction of the whole resistance; and, according to Ohm's law for the fall of potential along a conductor, the fall of potential along the vacuum tube should have been variable, had its function been that of a conductor. It was found in fact, however, that the fall of potential along the tube remained constant, no matter what resistance was introduced in the remaining part of the circuit between the poles of the battery, showing that the discharge was not a case of true conduction, but that even at the lowest pressure it was disruptive.

THE AMERICAN CHEMICAL SOCIETY.

The February meeting of the American Chemical Society was held on Friday evening, the 3d inst.

Dr. Orazio Lugo was elected a regular member.

The first paper of the evening was "On Crystallized Anhydrous Grape Sugar," by Dr. Arno Behr.

It was customary in the preparation of the anhydrous grape sugar to crystallize it out from an alcoholic solution, particularly from that of methylic alcohol, but Dr. Behr was lead to believe it possible that a simpler method could be devised. After some experimenting, he found that it could be obtained from the ordinary hydrated solution. A solution with 12 to 15 per cent of water gave the best results. In the description of its properties, Dr. Behr stated that when dried in a current of dry air, the crystallized sugar would not retain more than two or three per cent moisture, its reaction was neutral, its melting point is between 141° and 145° C. When tested by the polariscope it showed birotation. Dr. Behr then briefly referred to its economic uses, how by its cheapness it would be largely used by the confectioner, the druggist, and by those who manufacture wines. He also stated that as regards its sweetening qualities, instead of requiring twice as much or more to make it equal to cane sugar, he had found that one and two-thirds as much was sufficient. Mr. Nelson H. Darton followed with a short paper "On the Precipitation of Tannic Acid as Tannate of Copper." This paper was a supplementary description of Mr. Darton's method, already read before the Society. It consists in the precipitation of tannic acid by the ammonia sulphate of copper. The precipitate was tested for ammonia with negative results, and therefore it was contended by Mr. Darton that the precipitate was composed of copper tannate and not the double salt as has been elsewhere claimed.

The final paper of the evening was by Dr. E. Waller, of the School of Mines, Chemist to the New York Board of Health. Its title was "On the Water Supply of New York City. The object of this paper was to contradict certain statements made by Prof. Leeds in his recent paper read before the Society and also published in the *Chemical News*. Dr. Waller produced the analysis made by Dr. Booth in 1843, then by Dr. Chilton running between the years 1843 and 1859, Dr. Chandler's results from analysis in 1869-72, and finally his own, which have been regularly reported since 1872. These latter were represented by means of curved lines on diagrams which showed exactly the amount of each constituent for any time during the past nine years. These we may condense and show by the following table:

	PARTS IN 100,000.		
	Maximum.	Minimum.	Average.
Mineral matter	8.44	3.20	5.702
Org. and vol. matter	4.40	1.67	0.04
Total solids	11.07	4.80	7.38
Hardness	5.40	1.88	3.21
Oxygen by permanganate method	0.383	0.047	0.180

The results ohtained by Prof. Leeds in comparison with those showed from the above table were in several instances quite different. Thus, Prof. Leeds finds the total solids to be higher than any result obtained by the New York Board of Health during the past fourteen years. In other determinations similar discrepances were shown by Dr. Waller. The statement that the Croton water was contaminated by tanneries and other factories was objected to as incorrect, the tanning having long since ceased on account of the scarcity of trees. A statement from the Chief Engineer of the Water Department was read, in which he claimed that the water shed of the Croton River was the cleanest of any from which the supply of drinking water was obtained, either in this country or abroad. The population of the country through which the Croton flows does not exceed 20,000 inhabitants, or about one person to every ten acres. In comparison with other cities, the number of inhabitants to the square mile residing along the water shed of Croton, was stated to be extremely small, thus:

	Population to the Square Mile
London	270
Boston	229
Brooklyn	119
Schnectady, Cohoes, West Troy, } Drawing their supply from the Mohawk River.	103
New York	65
Rochester	36
Albany	77
Poughkeepsie, supply from Hudson River	86

By arguments such as the above, Dr. Waller maintained that the conclusions reached by Professor Leeds were erroneous. In the discussion that followed certain of Dr. Waller's modes of analysis were criticized by Dr. Endemann, but his remarks were merely on a side issue, and had no bearing on the results. M. B.

To the Editor of "SCIENCE."

DEAR SIR:—I am sorry to find that I have been misled as to one important fact stated in my paper upon Standard Time which appeared in "SCIENCE" for January 21st. The Signal Service has not applied for an appropriation of $25,000 for the purpose indicated in the paper, but a bill introduced in the house by Mr. King of Louisiana, asks this amount to enable the *Naval Observatory* to establish and drop time-balls at the principal ports of entry; and this was confounded with the Signal Service bill in the mind of my informant.

I supposed I had good authority for what I wrote, but as the result shows I ought to have looked into the matter more closely before trusting the statement to type. I regret exceedingly to have aided in giving currency to an erroneous statement.

C. A. YOUNG.

PRINCETON.

JUPITER.

By Prof. G. W. Hough, Director Dearborn Observatory.

During the present opposition of Jupiter, the disc exhibits a variety of phenomena of interest to the practical astronomer.

Although this planet has received a good deal of attention during the past century, yet, but few new *facts* have been added, with regard to its physical aspect, since the time of Sir Wm. Herschel.

It appears to be the generally accepted idea that its surface is subject to sudden and extraordinary changes, sometimes accomplished in a few days or even a few hours. New belts are alleged to have been formed or to have disappeared in the course of an hour or two.

We believe conclusions of this kind have been too hastily drawn from the obstructions.

Owing to the rapid rotation of Jupiter, the various spots and markings follow each other so closely that one might readily imagine that what he saw was subject to change under his eye.

The great red spot, which has been an object of so much interest since 1879, becomes visible, in the Chicago telescope, at 2h. 25m. from the meridian of the planet, when its length is about one second of arc. As the rotation carries it further on the disc, it gradually increases in size, until, when on the meridian, it subtends an angle at opposition, of 15 seconds. The smaller spots and markings, of course, become most conspicuous when near the meridian of the disc.

The visibility of objects depend, very much, also, on the condition of the seeing. Sometimes the smaller spots are invisible for weeks, simply because the seeing is not good enough with limited optical power, and not because there has been any radical change on the surface of the planet. Its distance from the earth is another important element in modifying the appearance of phenomena. After conjunction, the great Equatorial Belt and Red Spot are first seen and, as the earth approaches nearer, other markings gradually appear, until the time of opposition, the greatest variety of phenomena is noticeable.

From September, 1879, when micometer measurements were first begun, with the Chicago Refractor, on the markings of the disc, considerable change has taken place in its appearance at different times. But all changes, whether due to the distance of the planet from the earth, variable seeing, or other causes, have been slow and gradual.

The most noticeable change has taken place in belt No. 3 situated 6″ north of the equator. This belt, which was not conspicuous in 1879, gradually increased in width and distinctness in 1880, until at the present time its width is about 2″.5 of arc, and of the same color as the equatorial belt, viz.; reddish brown.

The equatorial region situated between the two outlines of the equatorial belt has been subject to considerable change, but the margins of the belt have not sensibly varied in width or latitude during the past three oppositions.

The great red spot, a conspicuous object even in a small telescope, is alleged to have materially changed in length during 1879, and again in 1880, but numerous micrometer measurements do not confirm this statement.

The following are the mean results, reduced to the mean distance.

	Length. ″	No. of Obs.	Breadth. ″	No. of Obs.	Latitude. ″	No. of Obs.
1879.......	12.25	9	3.46	8	—6.95	8
1880.......	11.55	20	3.54	10	—7.14	12
1881.......	11.50	8	3.49	3	—7.41	7

These numbers indicate a small possible displacement of the center in latitude, but it would be premature to assume such to be the case.

The color of the spot is reddish-brown; however, when the seeing is unusually good, it appears almost a light pink.

The oval-shaped white spots, a number of which were observed in 1880, are quite numerous at the present time. They are about one second of arc in length and are generally difficult objects to observe.

The following spots have been seen on belt No. 6. Latitude 9″. 5 to 12″. 6. They pass the meridian of Jupiter after the great red-spot as follows:

h.	h.	h.	h.	h.	h.	h.	h.
+ 2.8	+ 3.0	+ 3.3	+ 4.0	+ 4.7	+ 5.2	+ 5.5	+5.8

There are also two white spots—more easily seen—near the great red spot in latitude 9″. 63 and longitude 0h. 36m. and +0h. 24m.

The two white spots situated in latitude 3″. 0 south of the equator, which were observed in 1879 and 1880, were first seen this year on July 22. They appear to make a complete revolution around the planet in about forty-five days, corresponding to a rotation period of 9h. 50m. 9 sec.

These spots, which are both occasionally seen at the same time, appear to be fixed relatively to each other.

The difference of longitude was measured with the micrometer as follows:

1880.
July 24, +23.5m.
Nov. 8, +22.6m.
1881.
July 22, +27.5m.
Nov. 26, +27.5m.
Dec. 10, +23.0m.

The fact that they have maintained for a year and a half the same relative position, and at the same time apparently drifted with a velocity of over 260 miles per hour, would seem to disprove the old theory that they are clouds floating in the atmosphere of the planet.

From observations made during the present opposition, it is probable that all the matter between the two margins of the equatorial belt, whether in the form of white spots or dark ones, moves with the same velocity, viz.: a period of 9h. 50m. 9 sec. And it is possible that the belt itself partakes of this motion.

The rotation period of the planet, deduced from our observations on the red spot, made in 1879 and 1880, was 9h. 55m., 33.2 sec. + 0.09 sec. $\sqrt{t}$, in which t is the number of days after Sept. 25, 1879. The observations during 1880 showing that the spot was retrograding with an accelerated velocity.

This formulæ is found to be essentially correct for the present opposition.

The "mean" period for Dec. 14, 1881, comprising an interval of 811 days, being 9h. 55m., 35.80 sec. from observation and 9h. 55m. 35.76 sec. from the formulæ

Assuming the rotation period as above, the centre of the spot has retrograded more than fifty degrees since Sept. 1879, not uniformly, but with an accelerated velocity. It seems difficult to account for this fact on any known hypothesis.

IMPROVEMENTS OF PLANTE AND FAURE'S STORAGE BATTERY.

In a previous number of "Science" No. 57, July 30th, 1881, we gave excellent directions for making Planté and Faure's storage batteries.—In a recent paper before the "Society of Arts, of London, Professor S. P. Thompson states that almost any oxide or hydrate of lead will answer for use in the Faure battery—Litharge will answer if sufficiently finely divided for being painted on. Litharge mixed with a small proportion of binoxide of manganese works well. The most satisfactory cells I have yet tried were made by painting the lead plates with a coat of the brown peroxide itself, which is obtainable in commerce, although its cost is greater than that of red lead or litharge.

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The journal will be printed in Dublin, and will be issued in numbers of from 16 to 24 pages each, demy 4to, whenever we have material enough for a number. Longer memoirs will not be cut up in several parts, but will be printed together in one number, or carried through two numbers without interruption. Shorter numbers will be issued whenever subjects of more immediate interest shall require it, and in particular ephemerides of comets, etc., will be issued on advance sheets, if necessary. Twelve numbers will contain 240 pages, and will form a volume.

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Vol. II. No. 31, - - - - - - - - - January 29, 1881.

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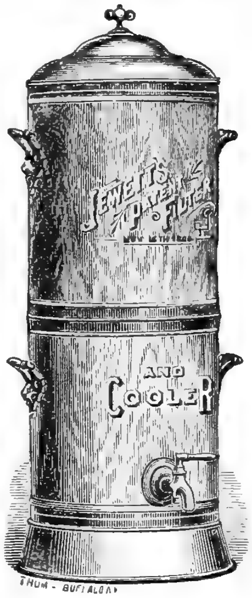

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No. 30, Vol. II. - - - - - - - - - January 22, 1881.

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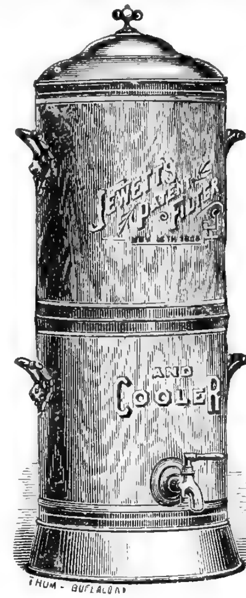

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No. 29, Vol. II. - - - - - - - - January 15, 1881.

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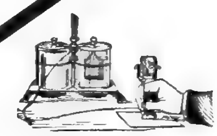

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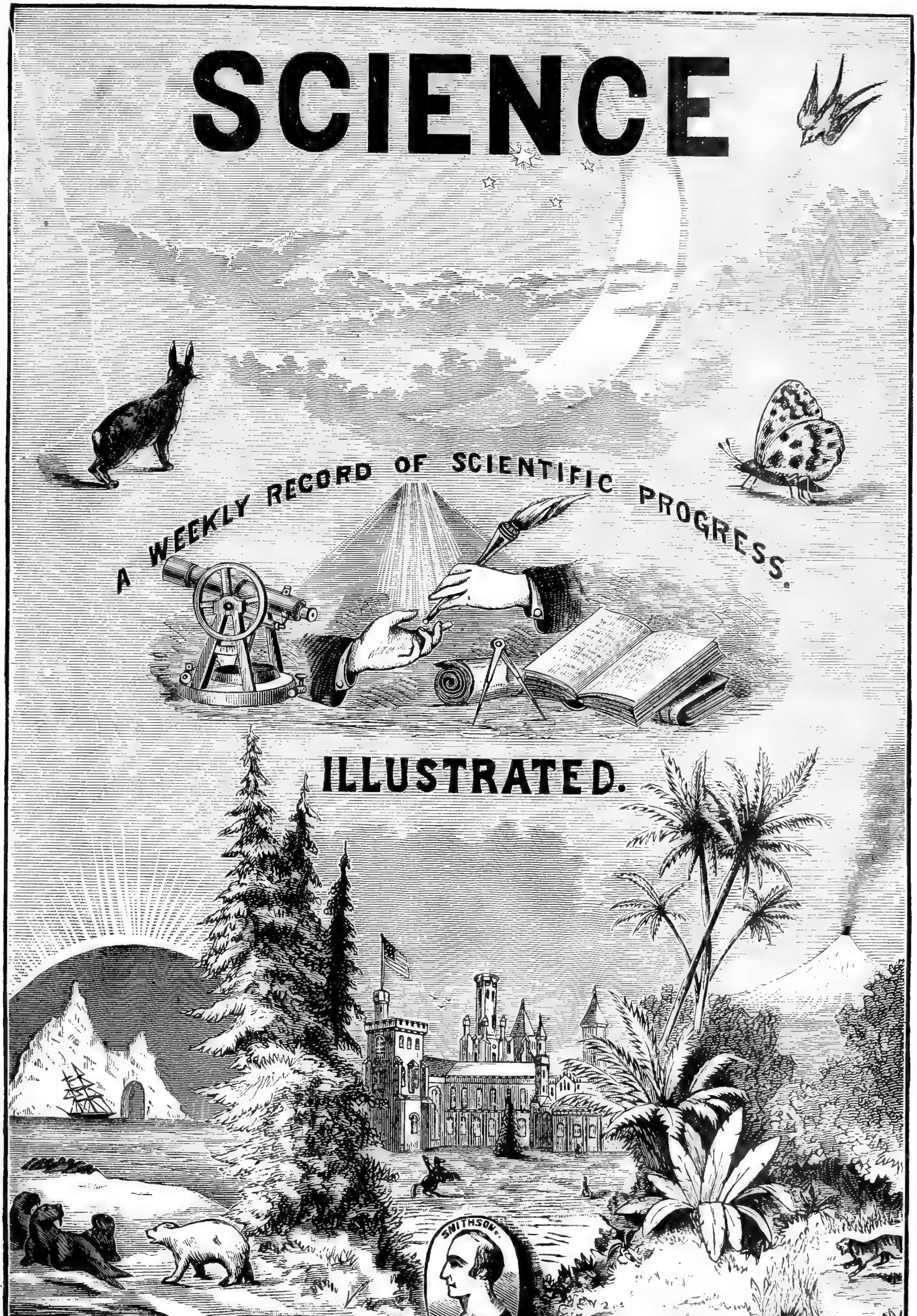

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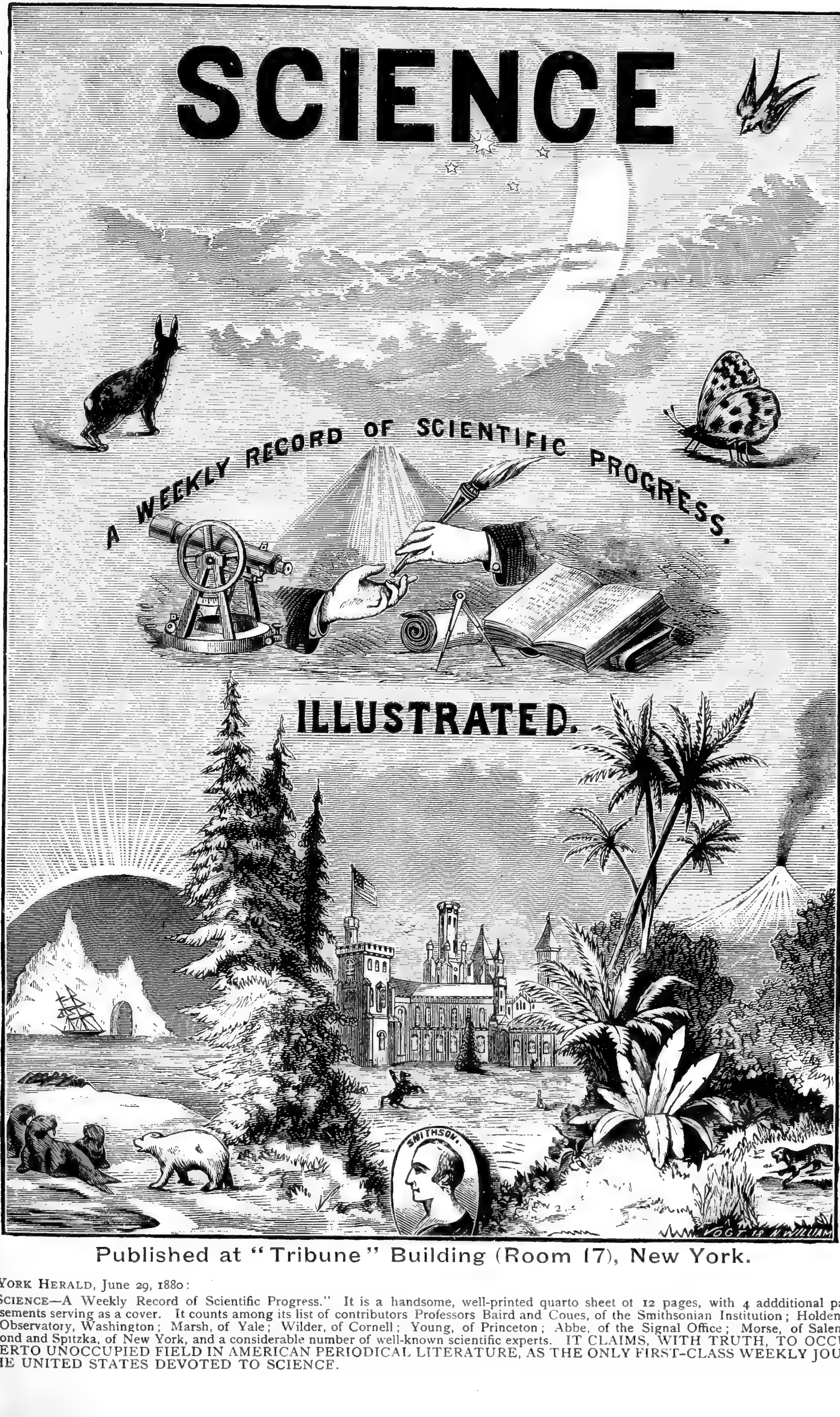
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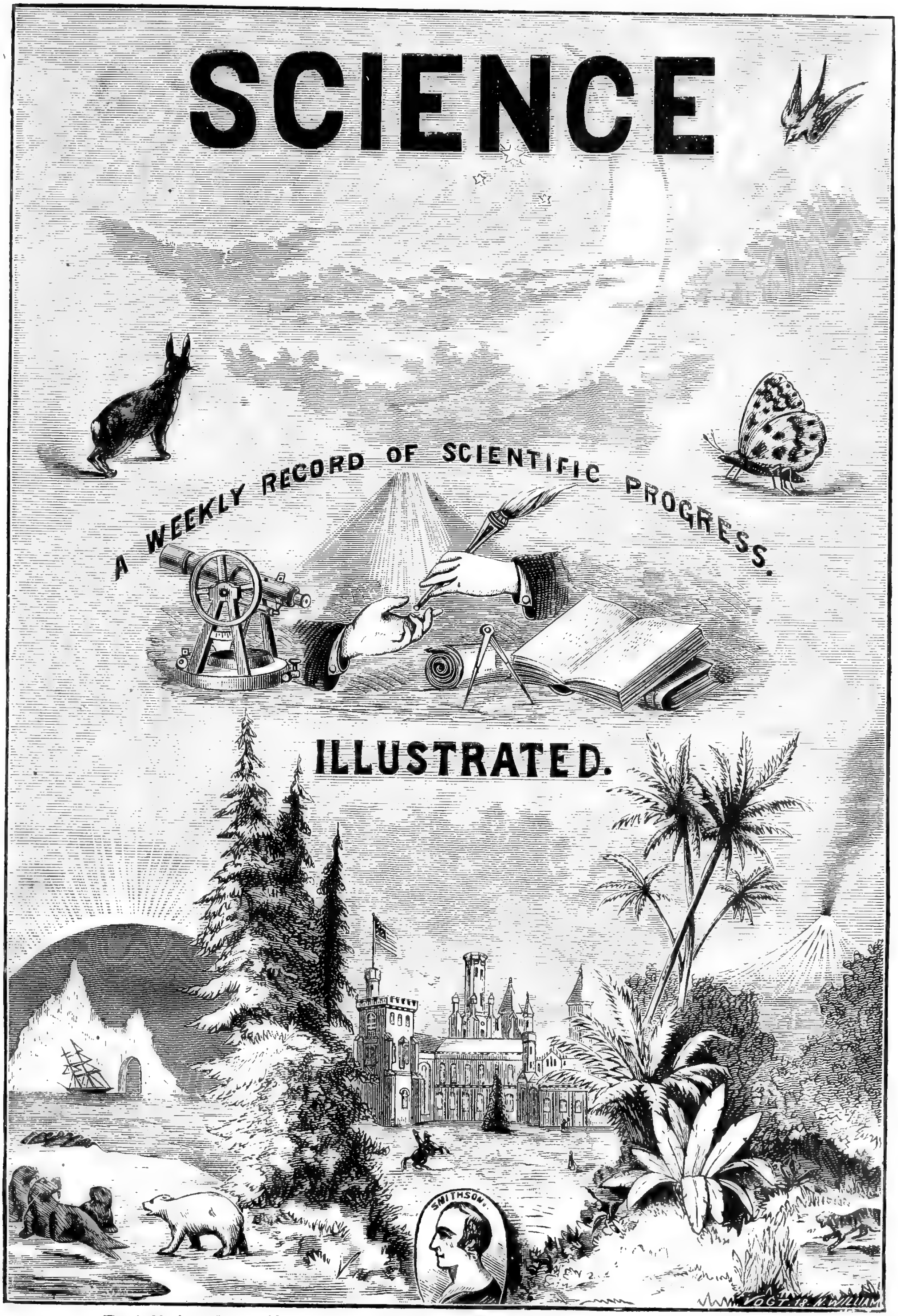

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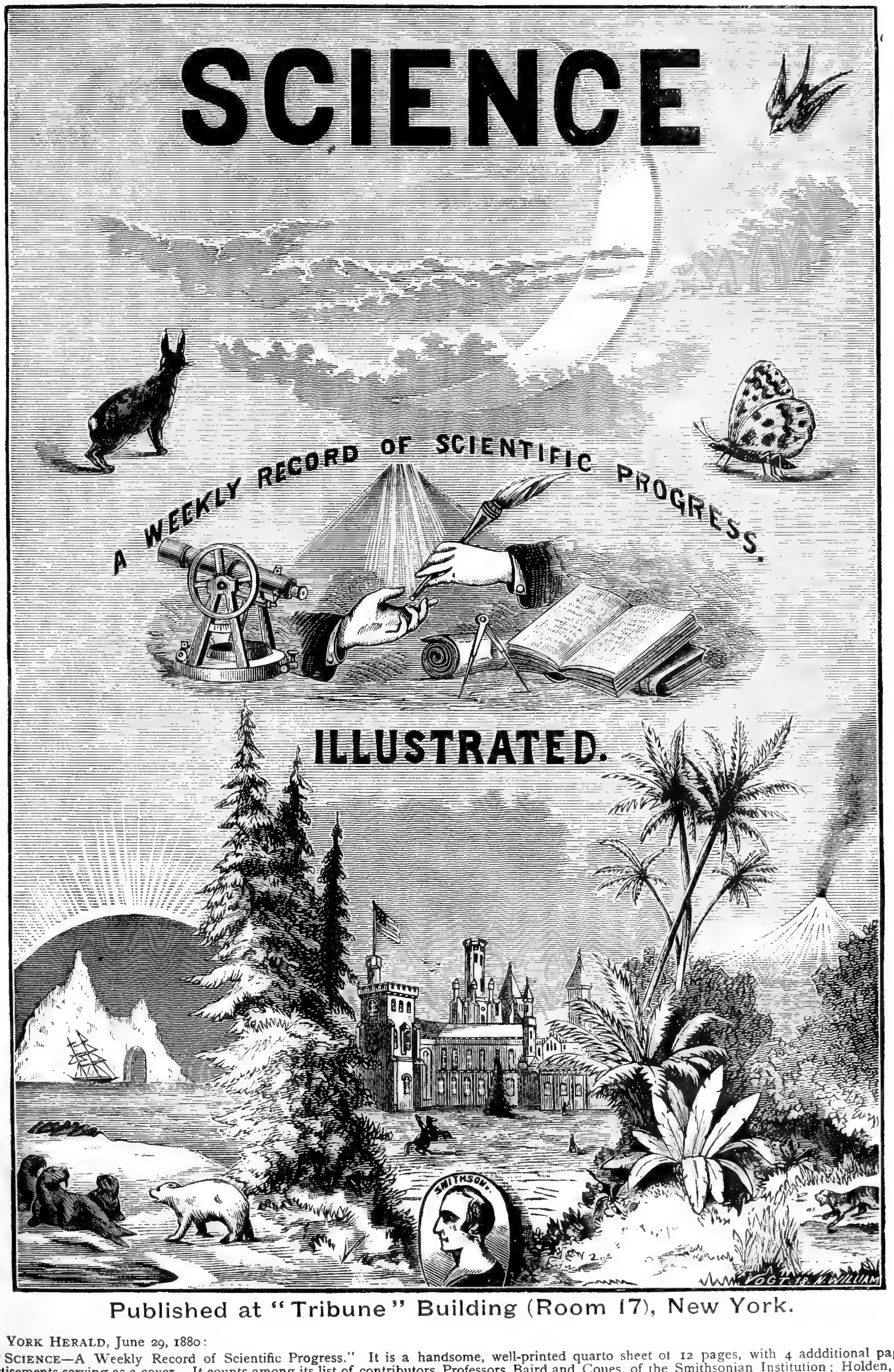

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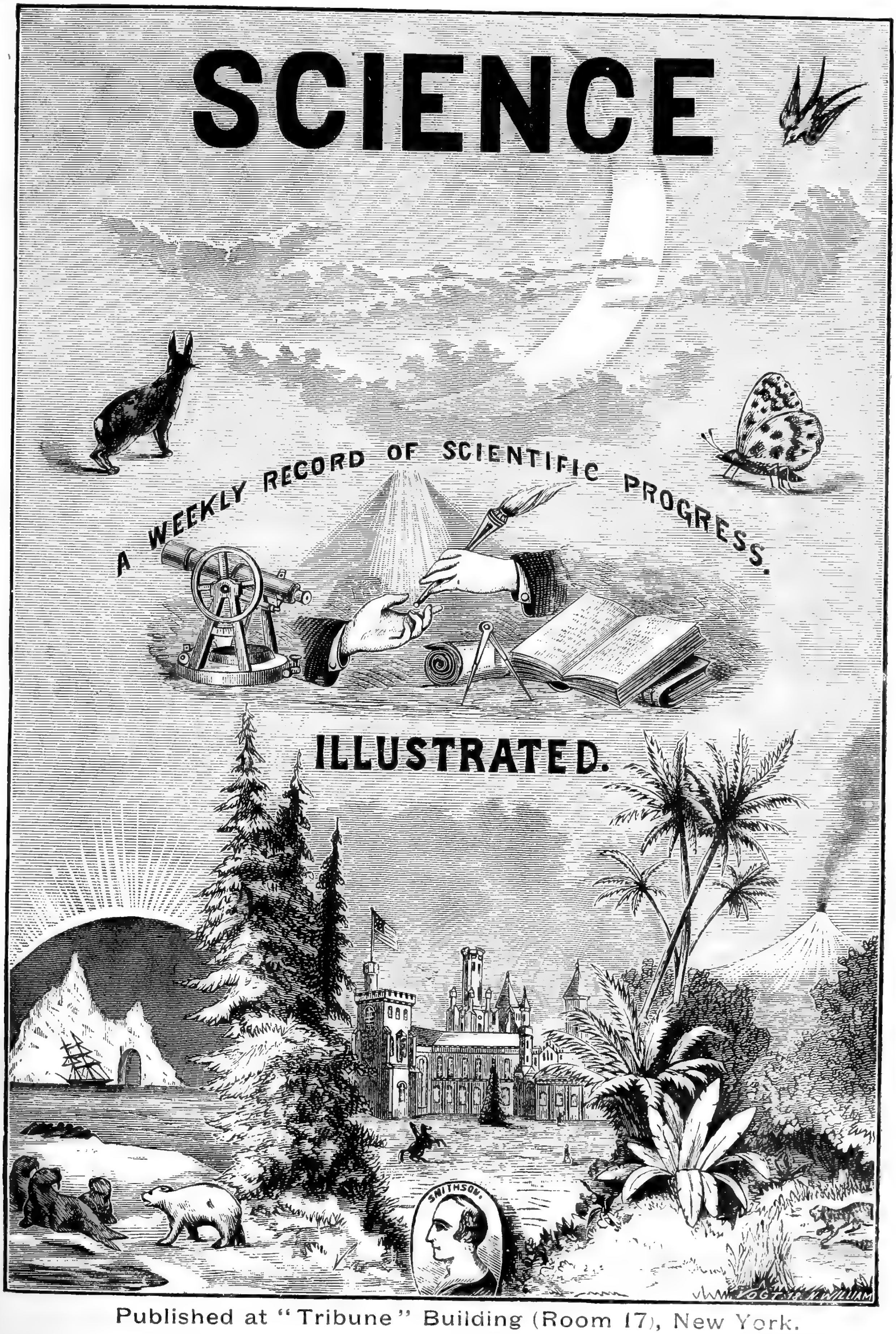

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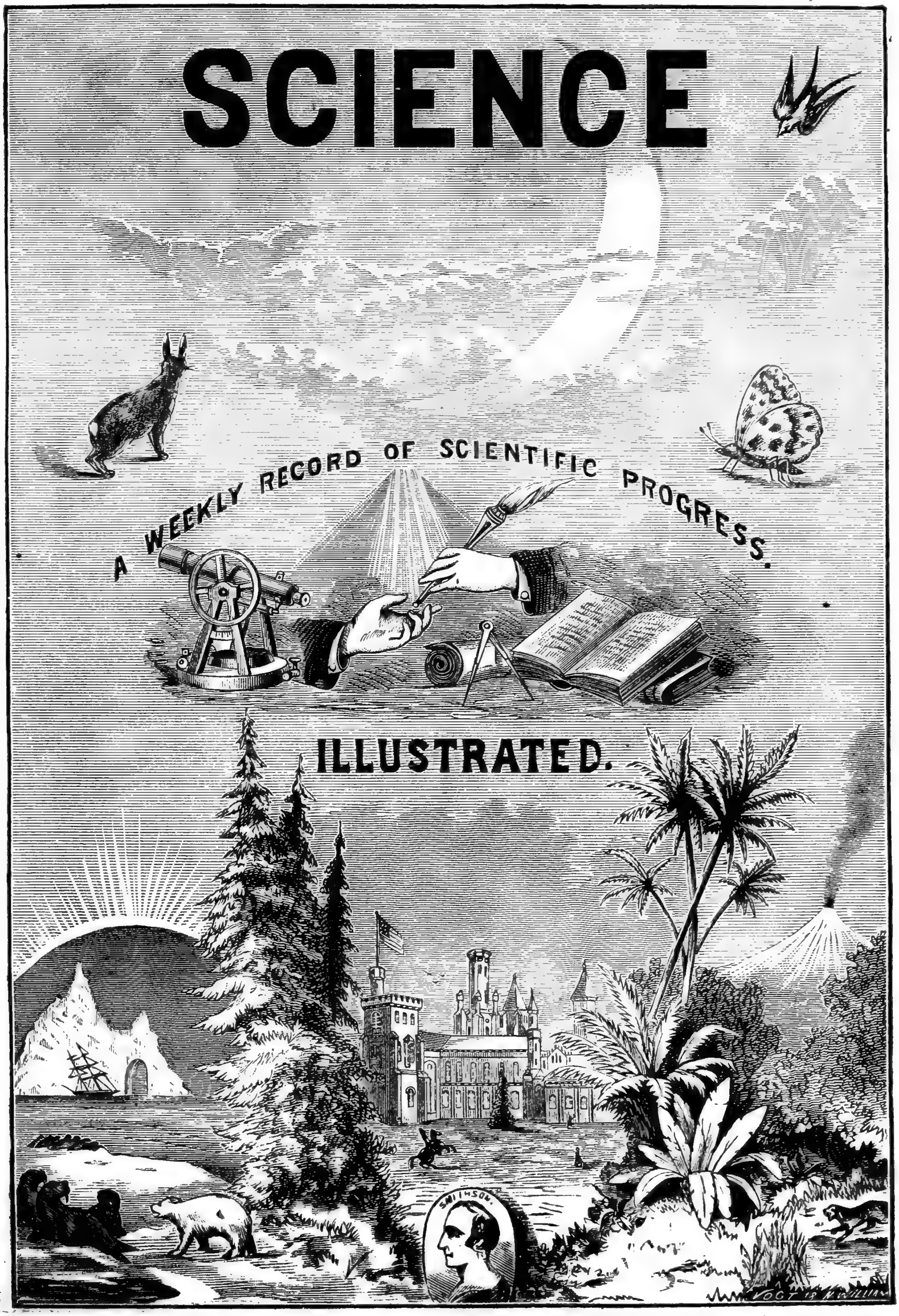

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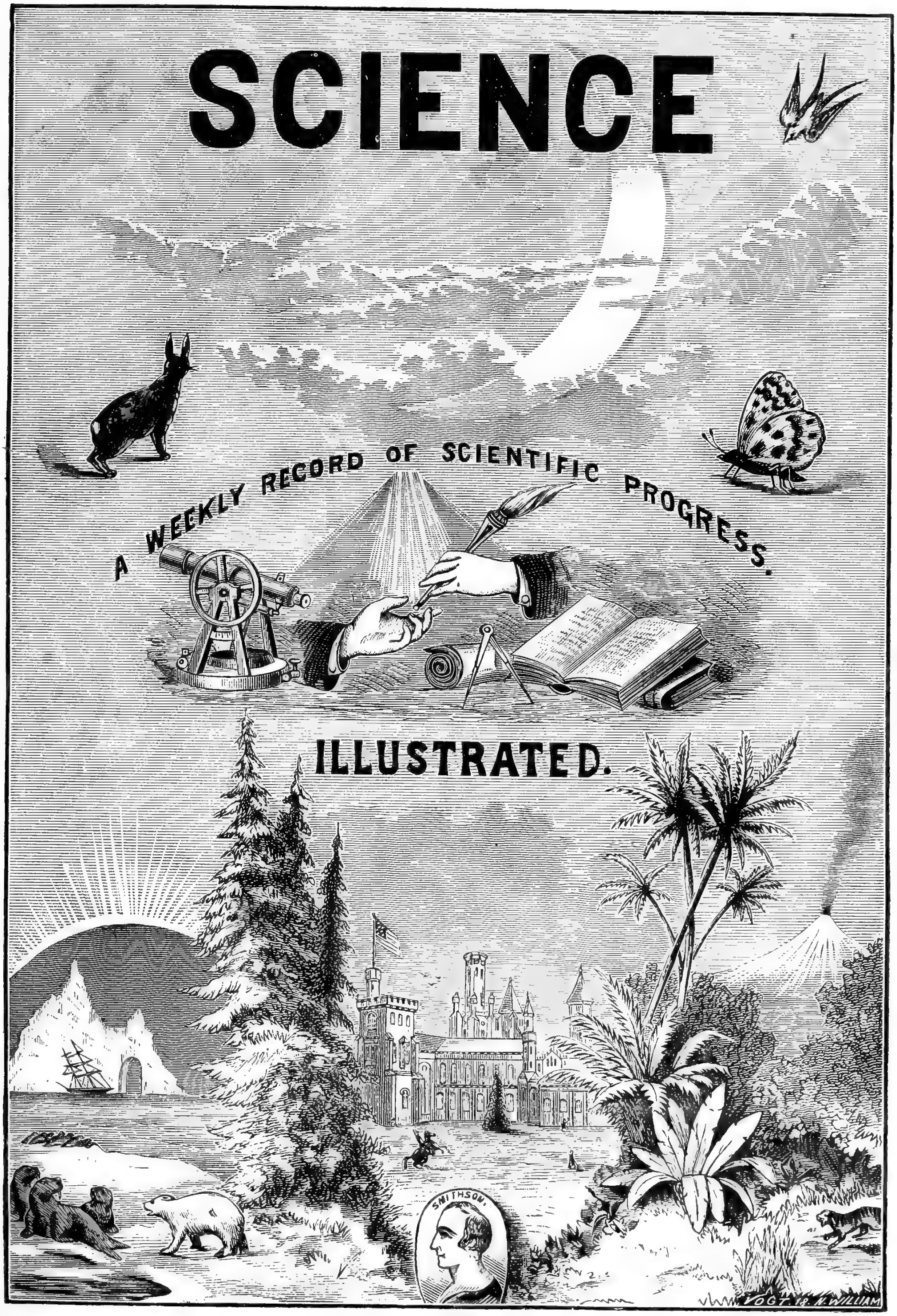

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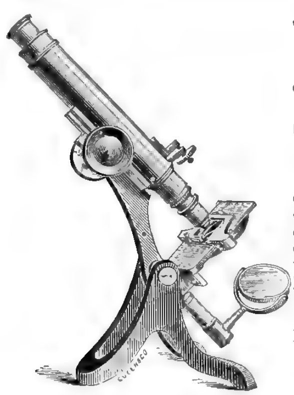

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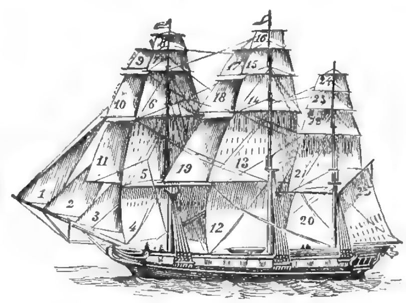

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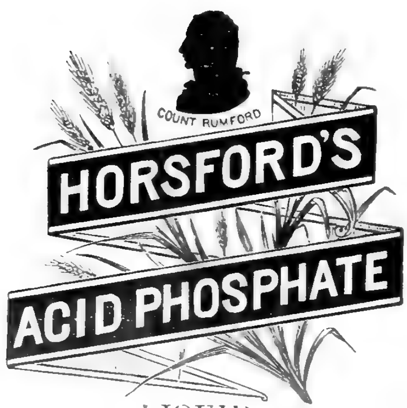

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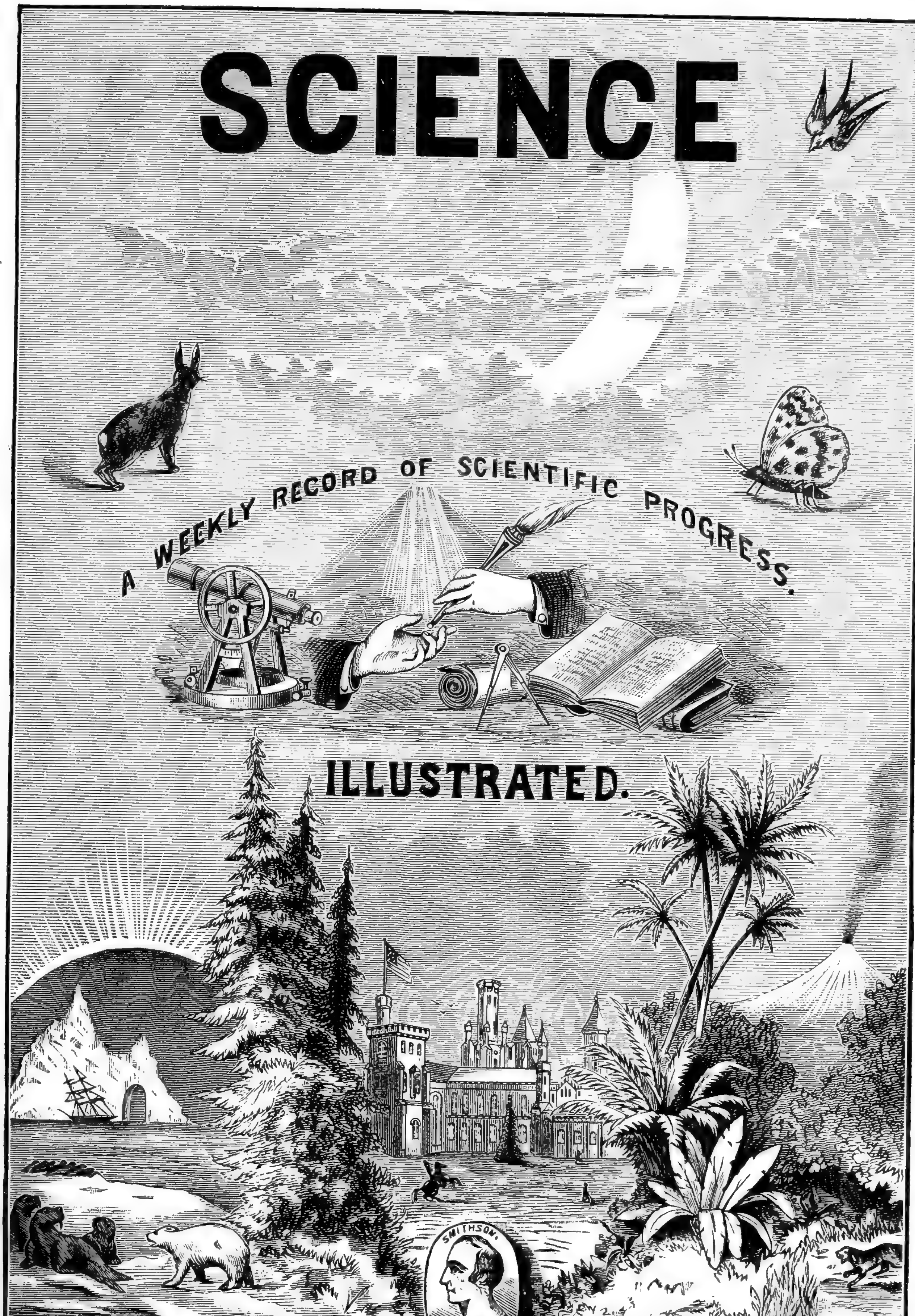

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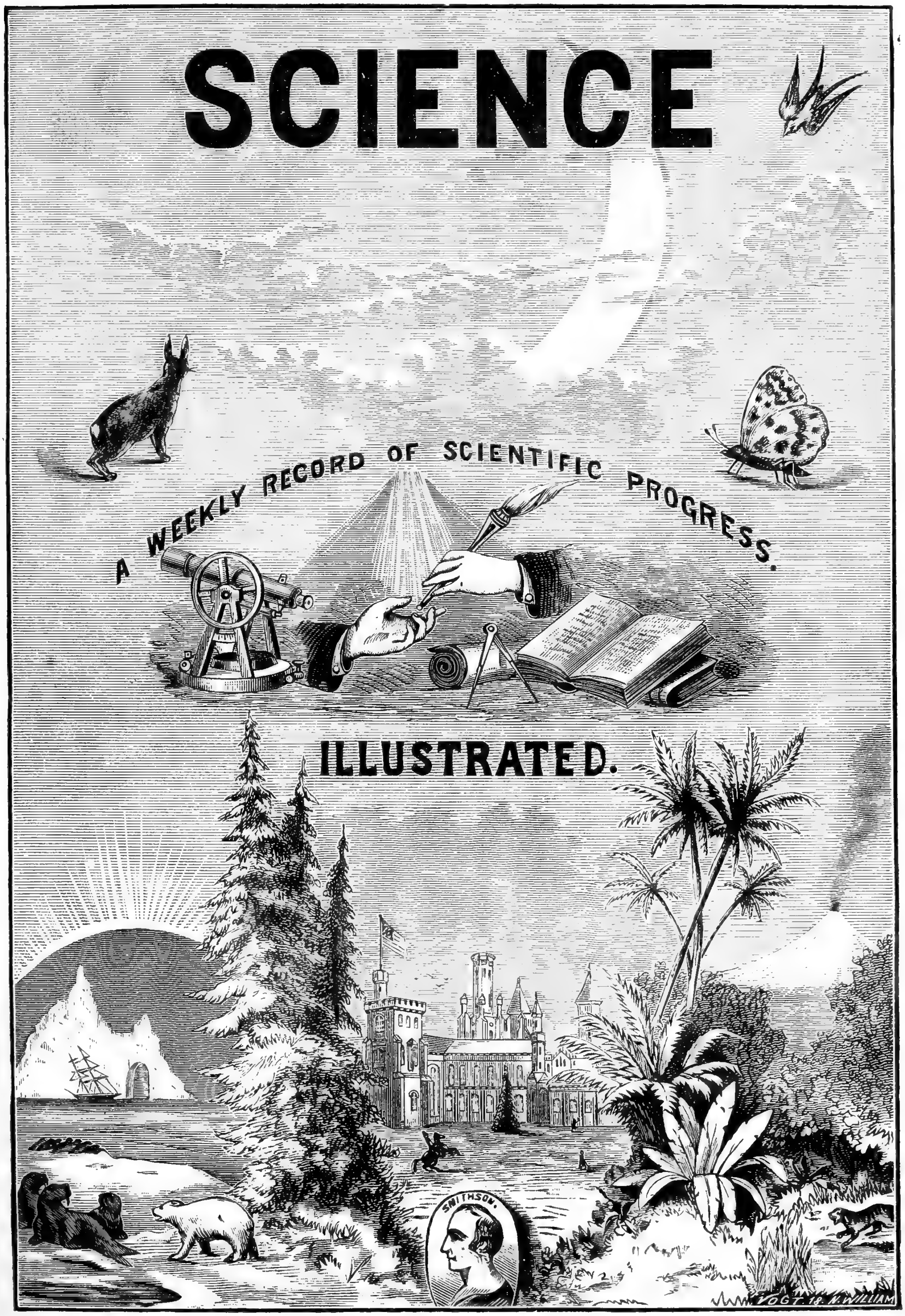

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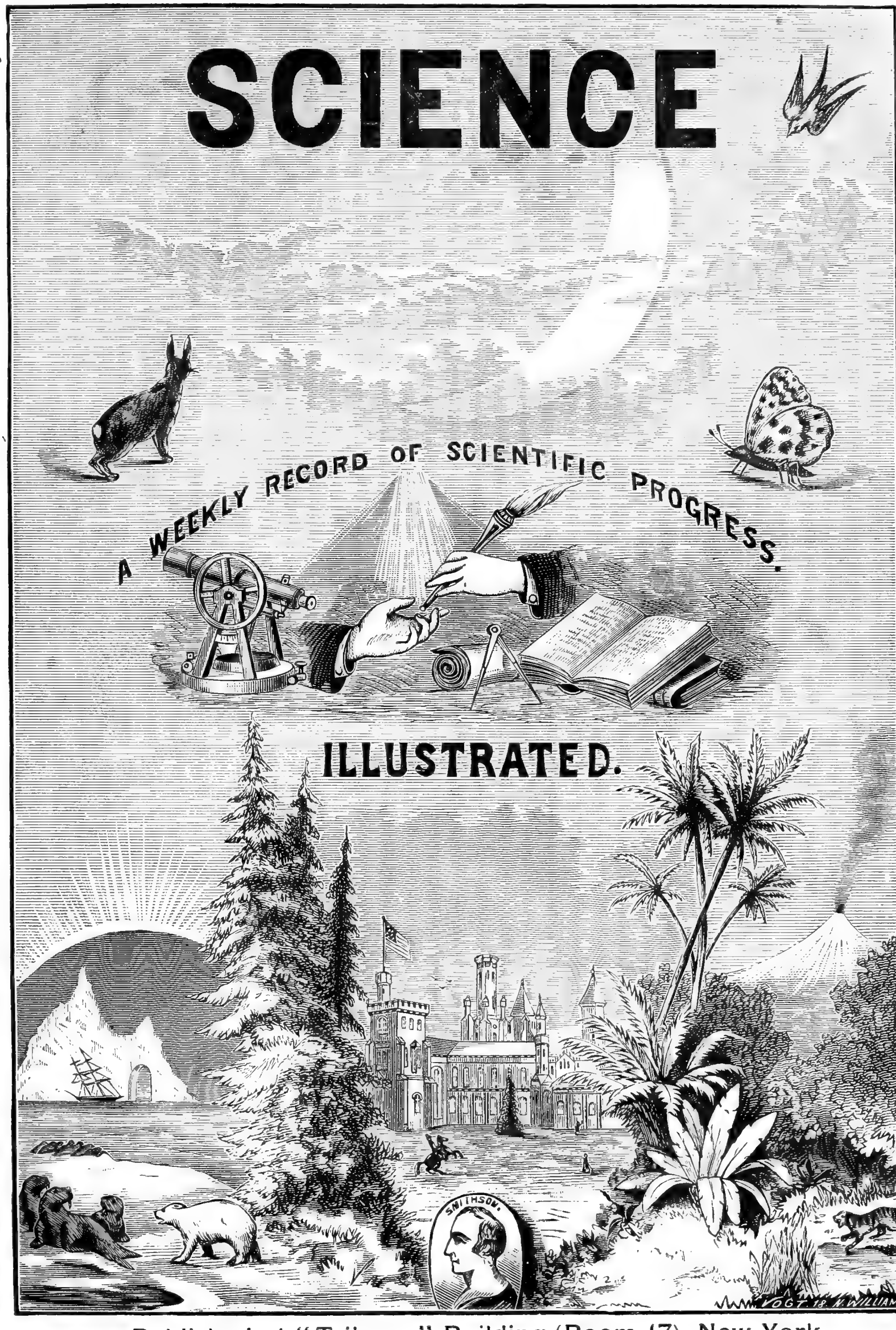

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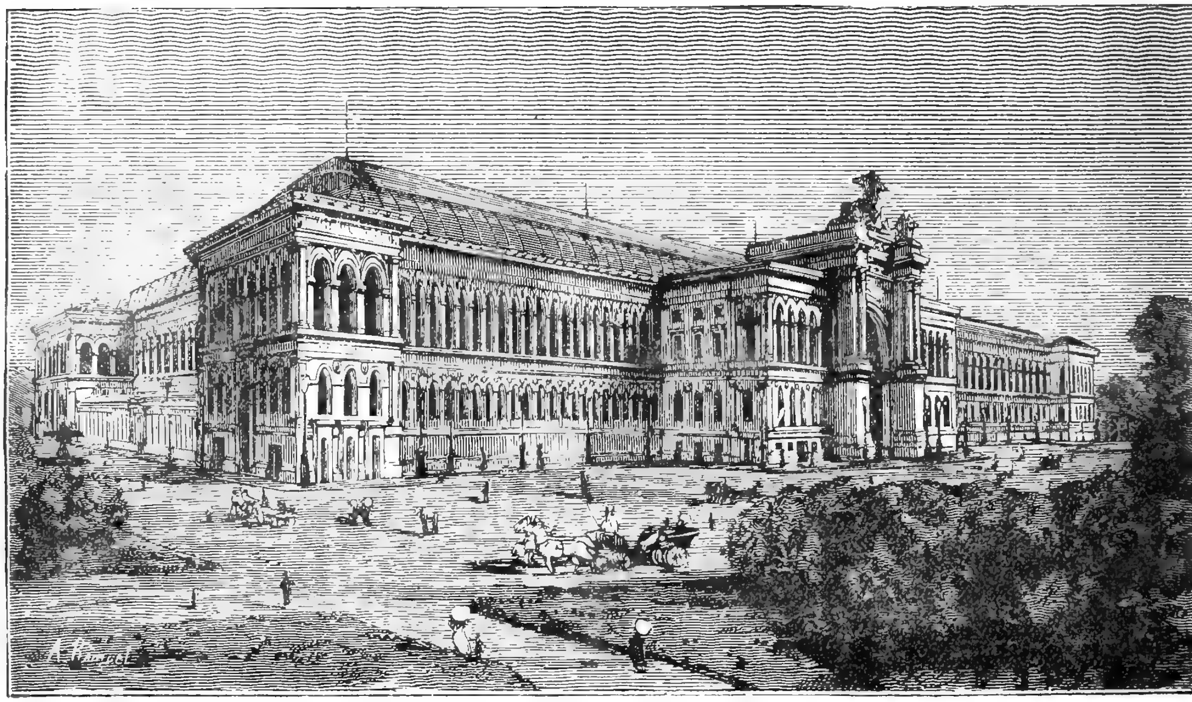

EXTERIOR.

INTERIOR.

ELECTRICAL EXHIBITION.—PARIS, 1881.

(See page 430.)

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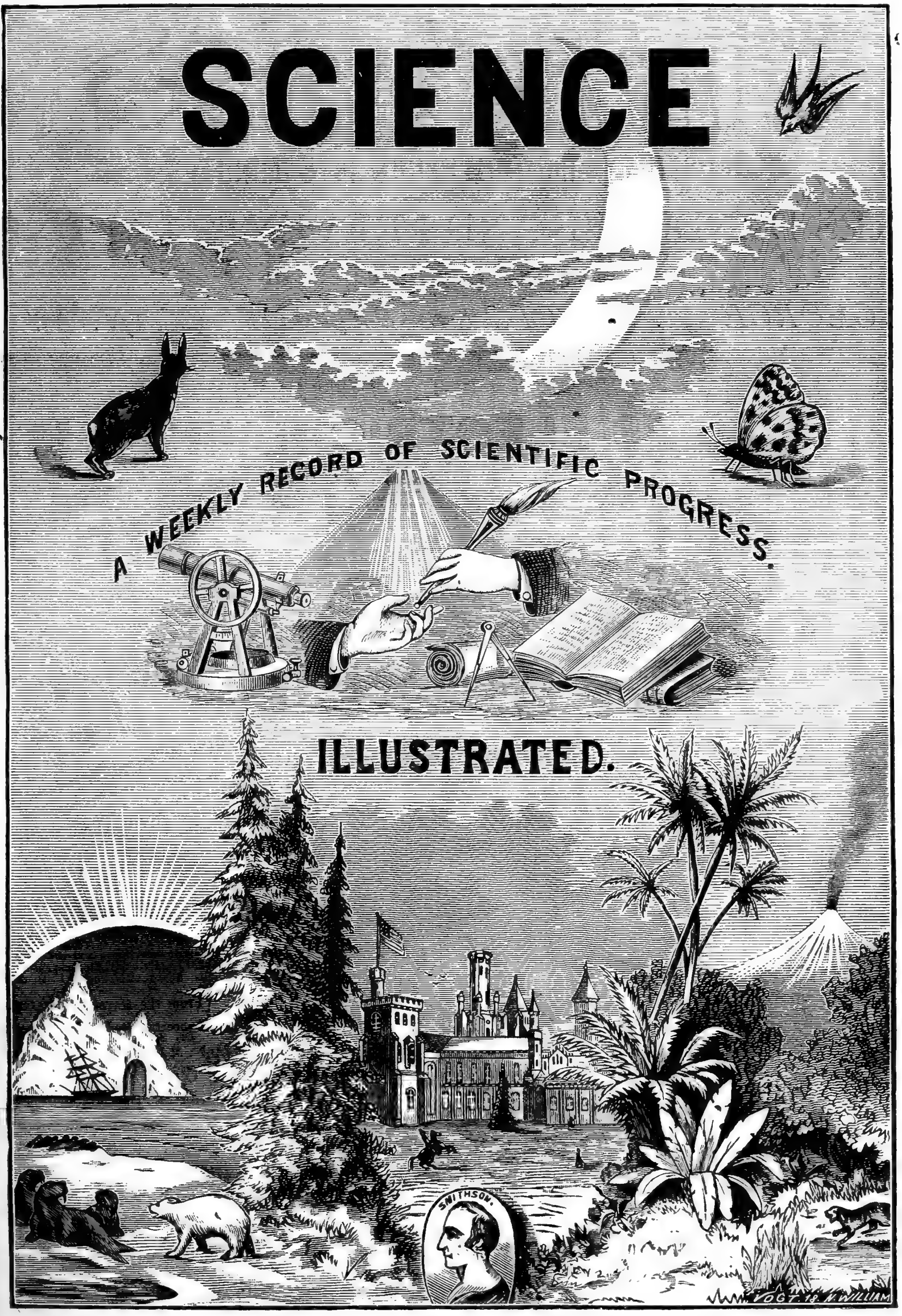

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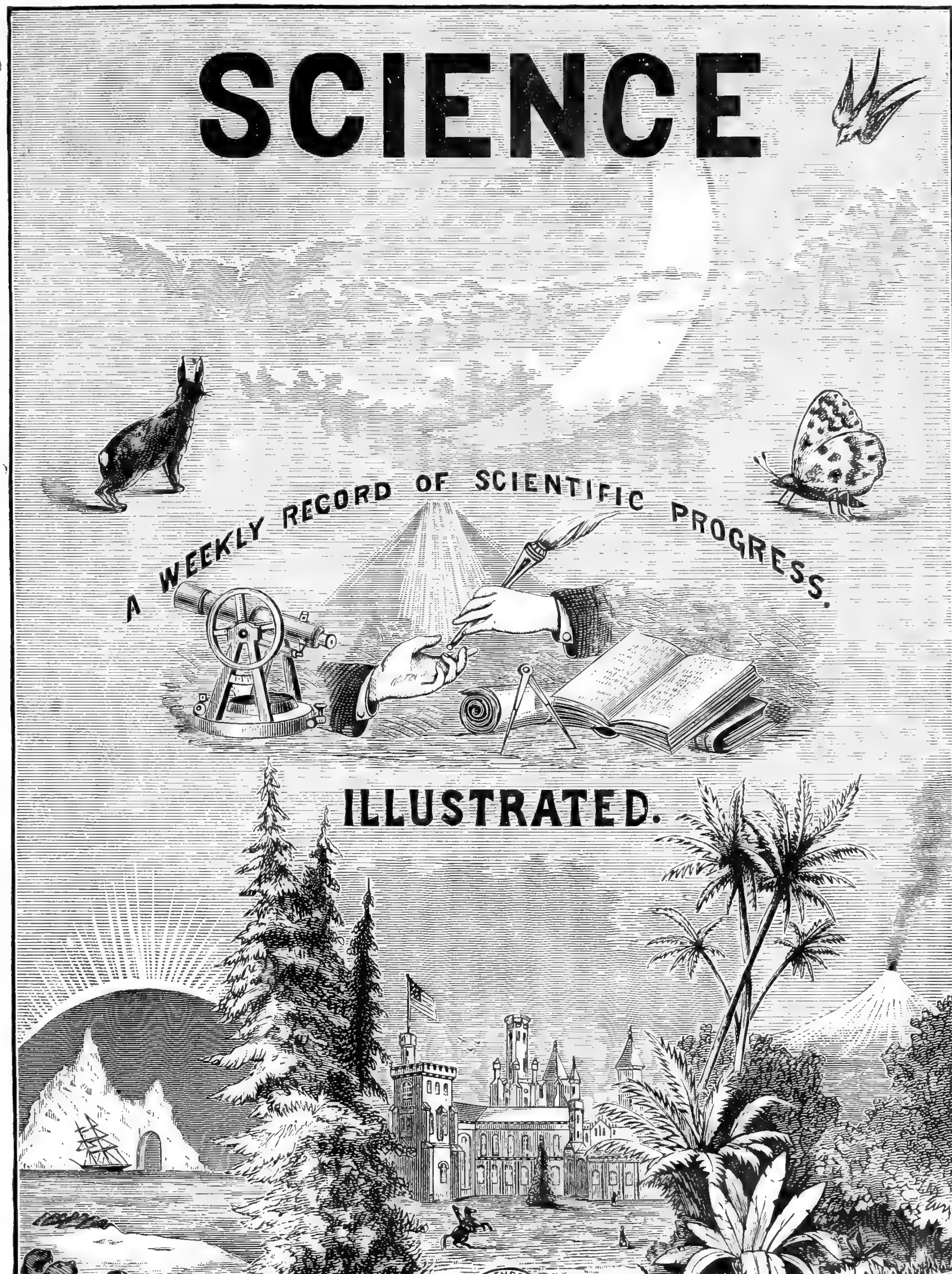

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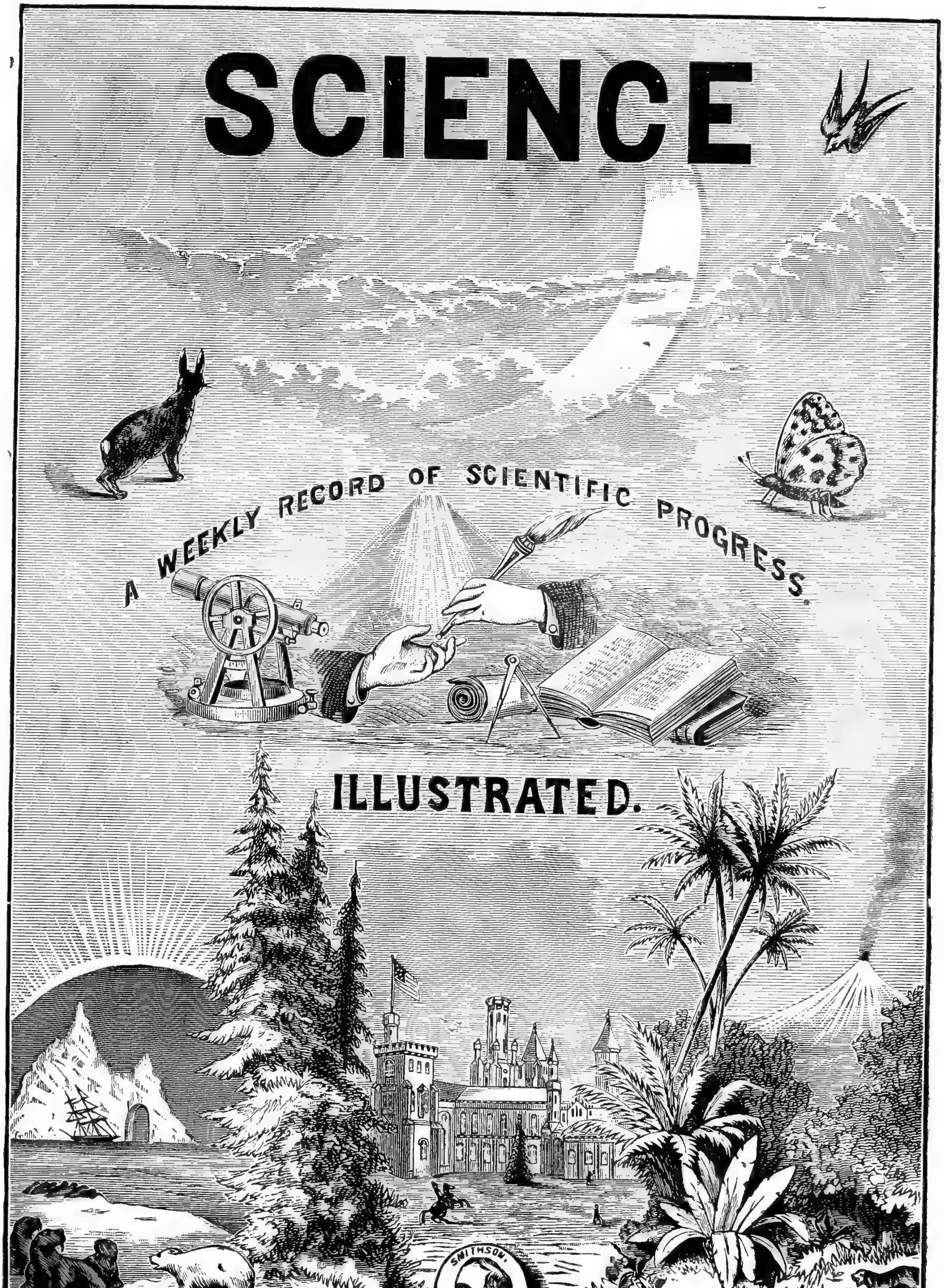

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. 64, - - - September 17, 1881.

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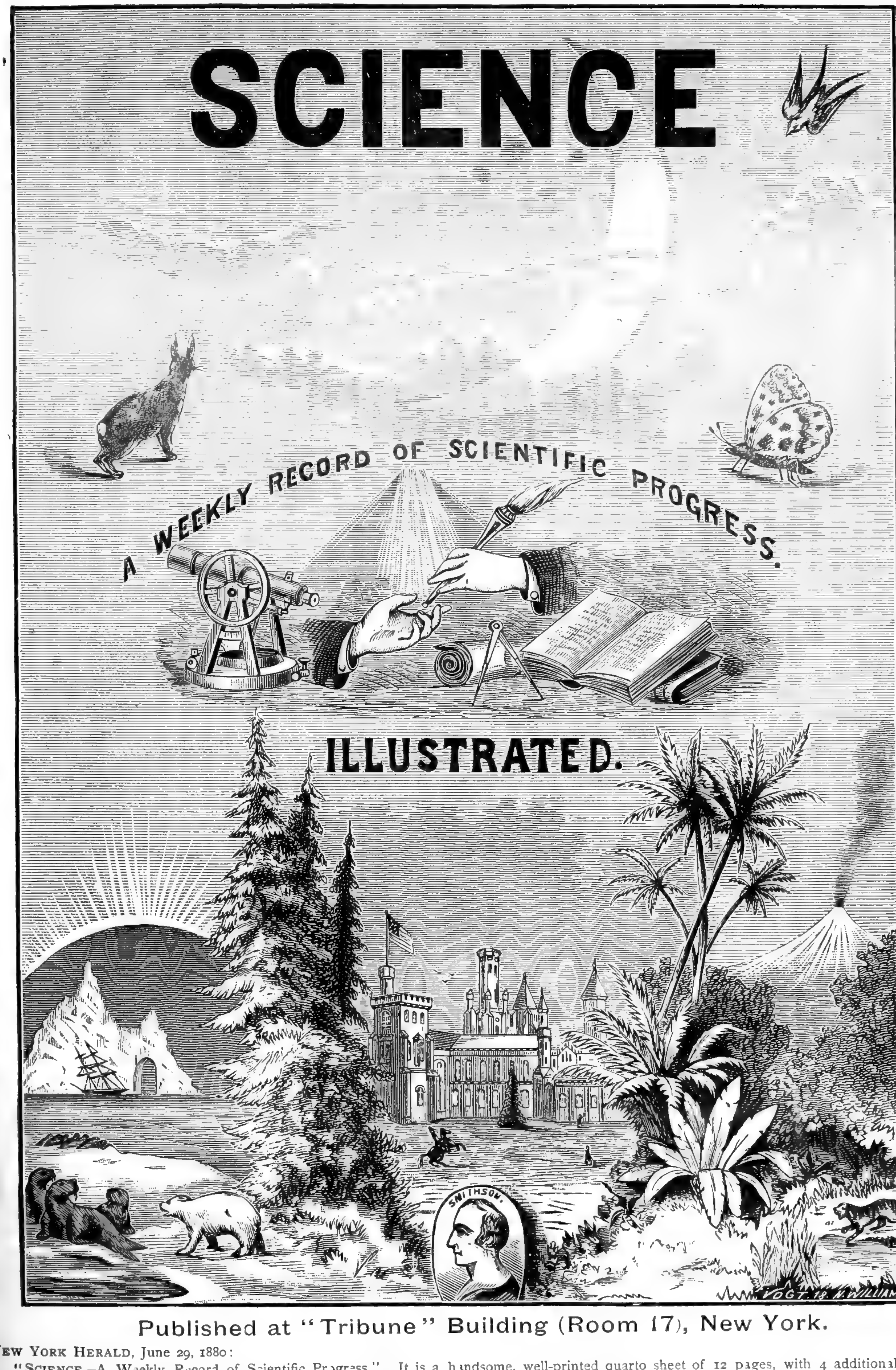

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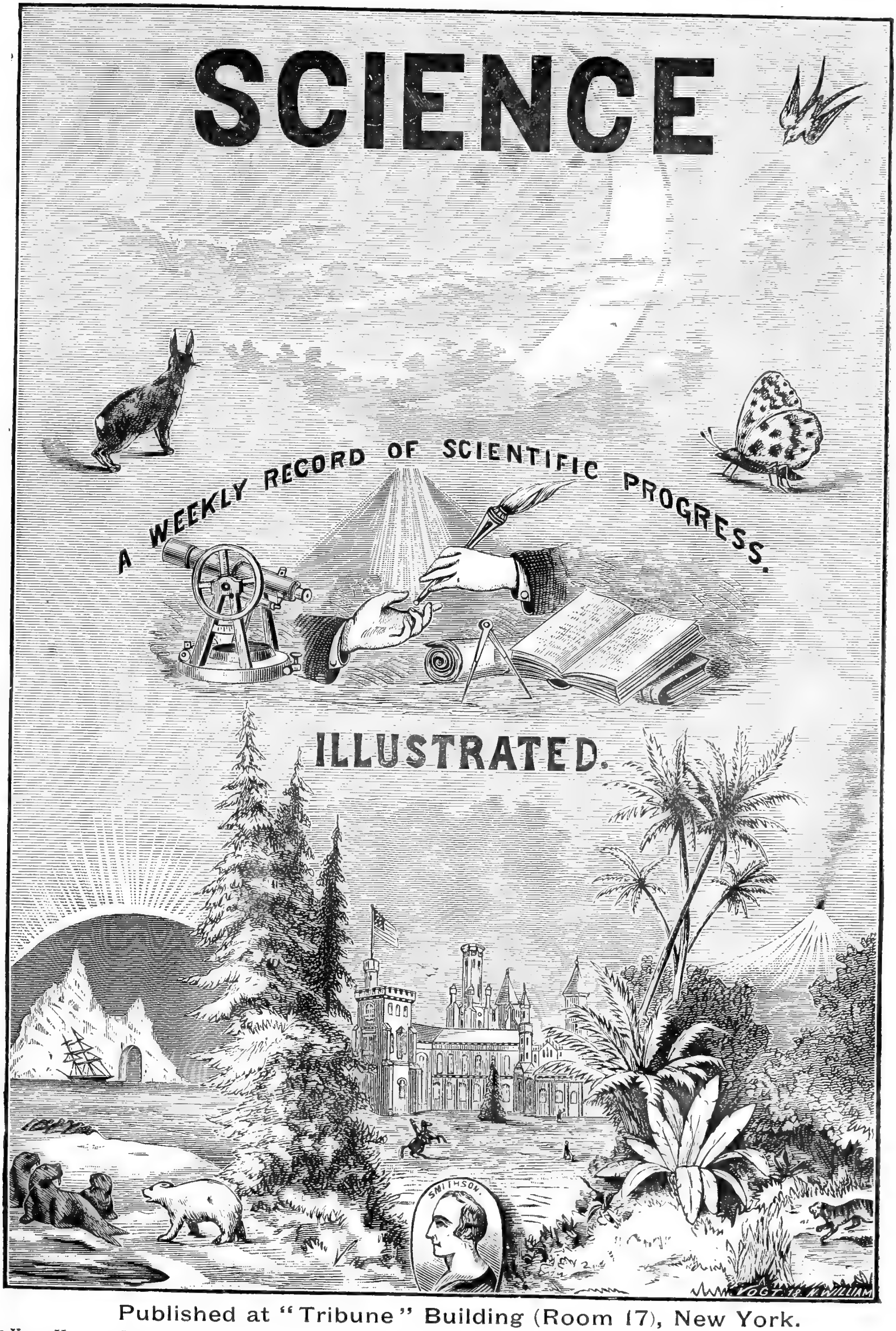

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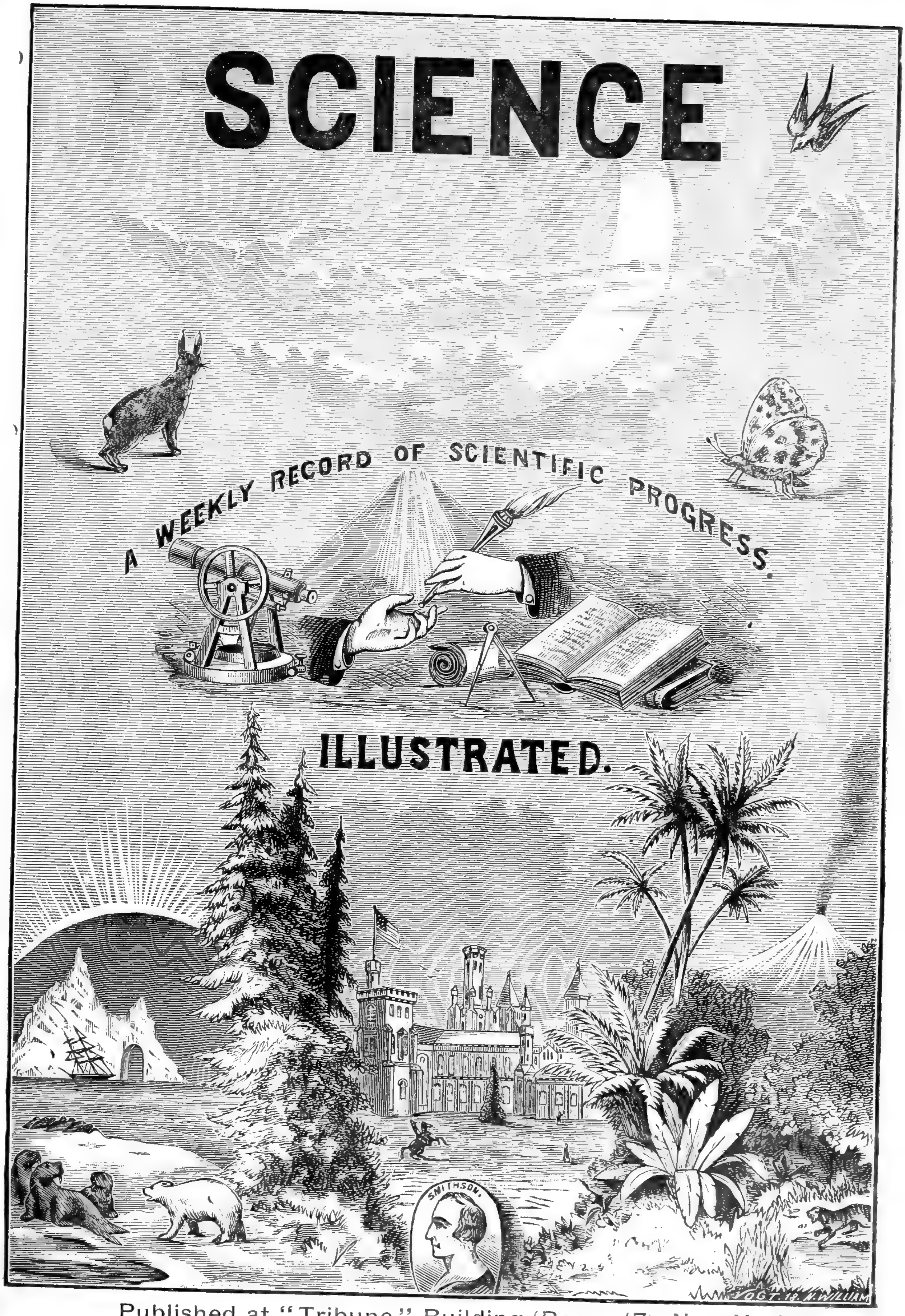

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[From Rev. Dr. Bridgeman.]

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The following from Webster, page 1164, shows the value of the Illustrative Definitions in

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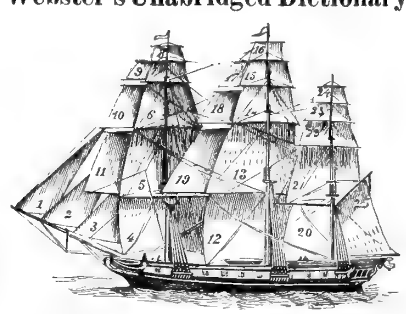

1, flying jib; **2,** jib; **3,** fore-top-mast-stay sail; **4,** fore-course; **5,** foretop sail; **6,** foretop-gallant sail; **7,** fore-royal; **8,** fore sky-sail; **9,** fore-royal studding sail; **10,** foretop-gallant studding-sail; **11,** foretop-mast studding-sail; **12,** main-course; **13,** maintopsail; **14,** maintop-gallant sail; **15,** main-royal; **16,** main sky-sail; **17,** main royal studding-sail; **18,** main top-gallant studding-sail; **19,** maintop-mast studding sail; **20,** mizzen-course; **21,** mizzen-top sail; **22,** mizzen-top-gallant sail; **23,** mizzen-royal; **24,** mizzen sky-sail; **25,** mizzen-spanker.

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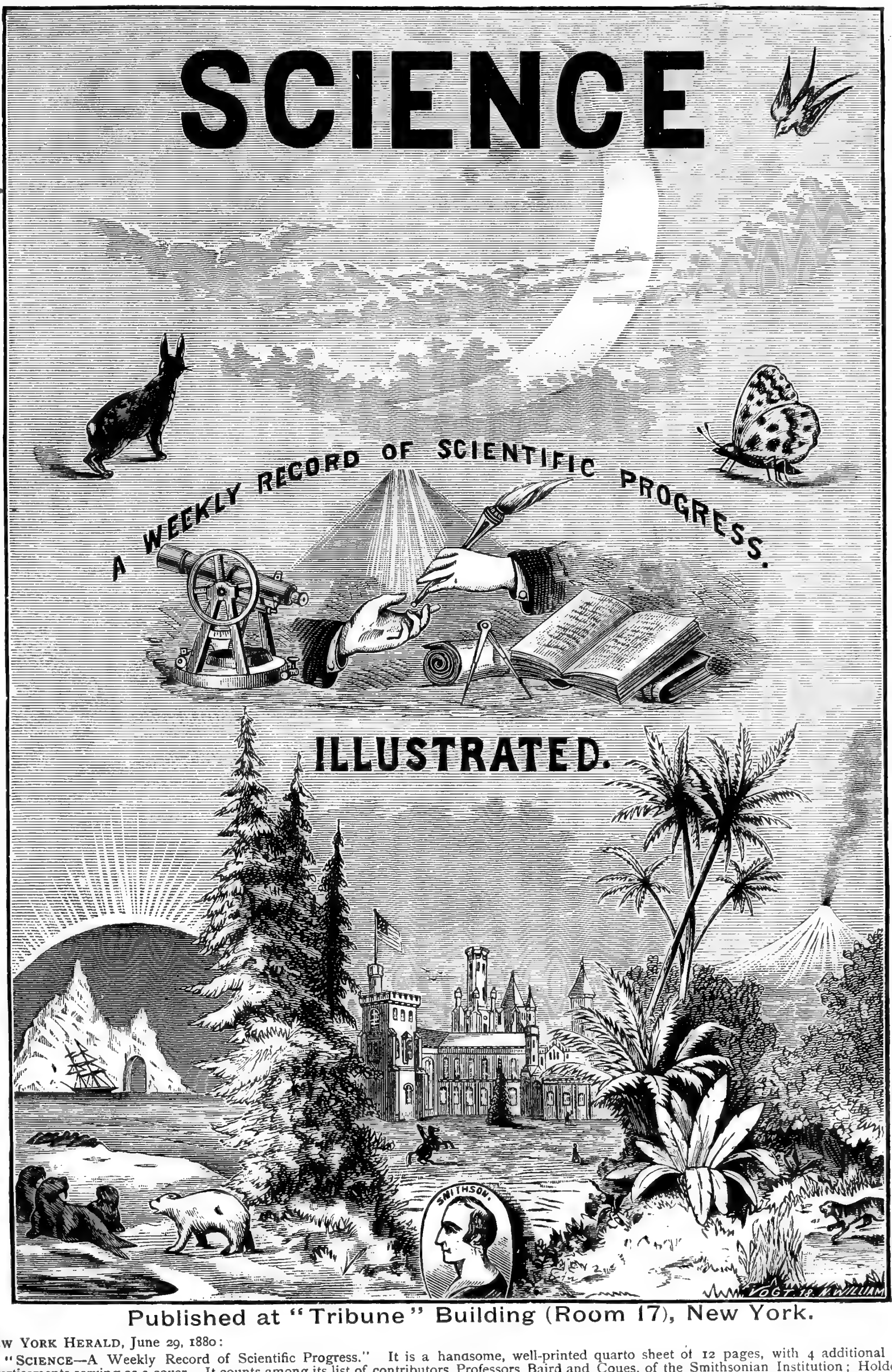

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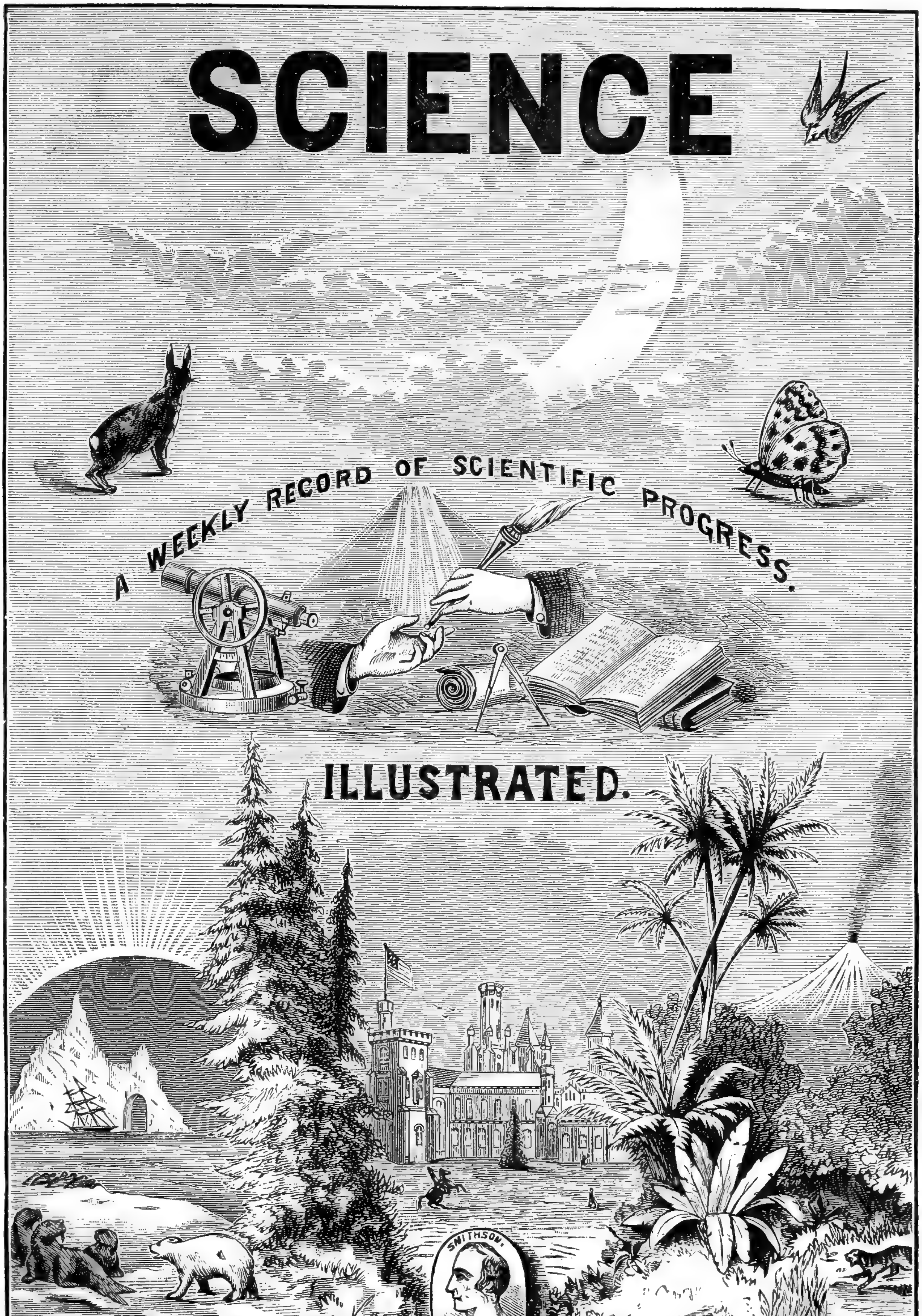

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SCIENCE

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. II, No. 71, - - - November 5, 1881.

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"SCIENCE:"

A Weekly Record of Scientific Progress.

ILLUSTRATED.

SCIENCE AND MEDICINE.

A few words on the relation of the natural sciences to med cine, as one of the greatest aids for the achievement of success, should be welcome reading to all members of the medical profession whose aspirations are above the dead level of mediocrity.

The physician may at first sight desire to stifle all discussion on this point by saying, that the requirements of study involved in acquiring a knowledge of medical practice *per se* occupy too much of his time to admit of his taking up outside issues, which he considers mere refinements of practice. There are others who take the absurd view that, to add a knowledge of the natural sciences is to become in the highest sense of the word a Chemist, a Physicist, or a Biologist. Seeing that the attainment of a complete knowledge of either of these sciences, is a work of a life time, it is argued, that they are to be shunned as impossibilities.

The path of the would-be scientific medical man is made clear by the encouraging words of one of his own profession, Dr. G. Vivian Poore, M.R.C.P., who says, there is a minimum of knowledge in this respect which is sufficient to endow the physician with a *scientific grasp of his art*. What is really wanted, is sufficient knowledge to enable a medical man to read these various sciences with intelligible results for himself, *when he needs* and as often as he desires to consult them, to show him as objectively as possible, those great principles which have already found application in his healing art. This will lead him to think and enable him to act with precision in any great emergency.

Let it be understood that there is no necessity for cramming the head with a mass of details, and that our object is to enrich and not encumber the mind of the medical practitioner.

To those who are ignorant of the advantages of some knowledge of the natural sciences in medical practices, the following observations of Dr. Poore may be read with interest.

"There are those who hold that the student of medicine has but little need of special training in the natural sciences, but such a position I believe to be untenable, and if I have to say one thing more emphatically than another to the first year's students, it is to advise them, not on any account to neglect their purely scientific studies. They are the very foundation of your professional knowledge, and without a solid foundation, no firm or worthy superstructure can be raised.

How can a man hope to rightly comprehend that most complicated of all machines, the human body, with its levers, pumps,and elastic canals, unless he be first furnished with the principles of mechanics and hydraulics? Who will say that a proper knowledge of the eye, or of the many optical instruments used in medicine, is attainable without some acquaintance with the laws of light; or that the intricacies of the ear, and the art of auscultation can at all be understood by him, who knows nothing of the laws of sound. The laws of heat must be studied in order to appreciate the difficult problems afforded by the animal temperature, its variations in health and disease, and the means of influencing it by therapeutic agents. Without the principles of chemistry we should be intellectually lost in the human laboratory, and unable to employ chemical agencies in the treatment of disease; and electricity is so correlated with the other physical sciences, and of so much service both in diagnosis and treatment, that its separate study has also become essential. Neither can we altogether neglect geology and meteorology, since conditions of soil and atmosphere are now recognized as important factors in the causation and relief of suffering.

It is scarcely necessary to insist on a knowledge of those sciences which are called "biological." *Anatomy* and *Histology*, formerly the mere handmaids of medicine, but now recognized as sciences worthy of independent study, are as necessary to us as is a chart to the navigator; while *Physiology*, which teaches us the use and mode of action of the anatomical and histological elements, is the medical practitioner.

Zoology and botany are not so absolutely necessary for us as are the other sciences, but it is evident that they are very necessary as preliminary studies for the biologist, to whom we look for instruction, for without a study of the simple forms and conditions of life a proper understanding of human anatomy and physiology is not attainable, and in so far as they teach us the conditions of existence of the various vegetable and animal parasites which affect the human body, from micrococci upwards, they are necessary for us as *surgeons and physicians*. This list of sciences is truly formidable, but I nevertheless assert that there can be no true study of medicine without a knowledge of the principles of all of them; and, for my own part, I have never had any difficulty, as a teacher of clinical medicine, in discriminating easily, by a perusal of their clinical reports, between those students who have, and those who have not, had an insight into the principles of pure science.

Scientific principles are to the physician and surgeon what the sextant and compass are to the navigator. Without them he cannot rise above the rank of a lighterman or a ferryman, but must be content to remain a mere "pill-monger," or a chirurgeon of a base mechanic sort. With them he may fearlessly launch his bark upon unknown seas, and may have the good fortune to extend the frontiers of science, or discover, as it were, new continents, to give a wider scope to the art which he professes."

To the medical man who would reap the advantages held out by Dr. Poore, *we confidently suggest the value of this journal* as a means of accomplishing the ends desired, at the least cost, and most convenient form. The impecunious can thus avoid the purchase of the mass of scientific literature with which the market is flooded, and the overworked practitioner receiving the journal weekly is not embarrassed by redundancy, and yet can safely rely on passing nothing of importance, while articles of special interest to the profession will be constantly brought before his notice.

In the previous numbers of "SCIENCE" may be found valuable articles by Professors Burt G. Wilder and Sage, of Cornell; Drs. Hammond and Spitzka, of New York; Dr. Clemenger, of Chicago; Dr. J. A. Mason, of Newport, and many other specialists of equal merit.

Now the value of a knowledge of science, as a means of "getting on" as Huxley terms it, is indubitable, and while there are few trades in which some knowledge of *science* may not be profitably applied to the pursuer of his occupation, we think that the words of Dr. Poore must carry conviction, that the *student* or Physician who would attain the higher stages of development of his art, must be kept "*au courant*" with such facts and principles, which are weekly published in "SCIENCE," for they will probably find their application in every intelligible diagnosis and discussion on medical practice.

"SCIENCE," November 5th, 1881.

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SCIENCE

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Vol. II, No. 70, - - - October 29, 1881.

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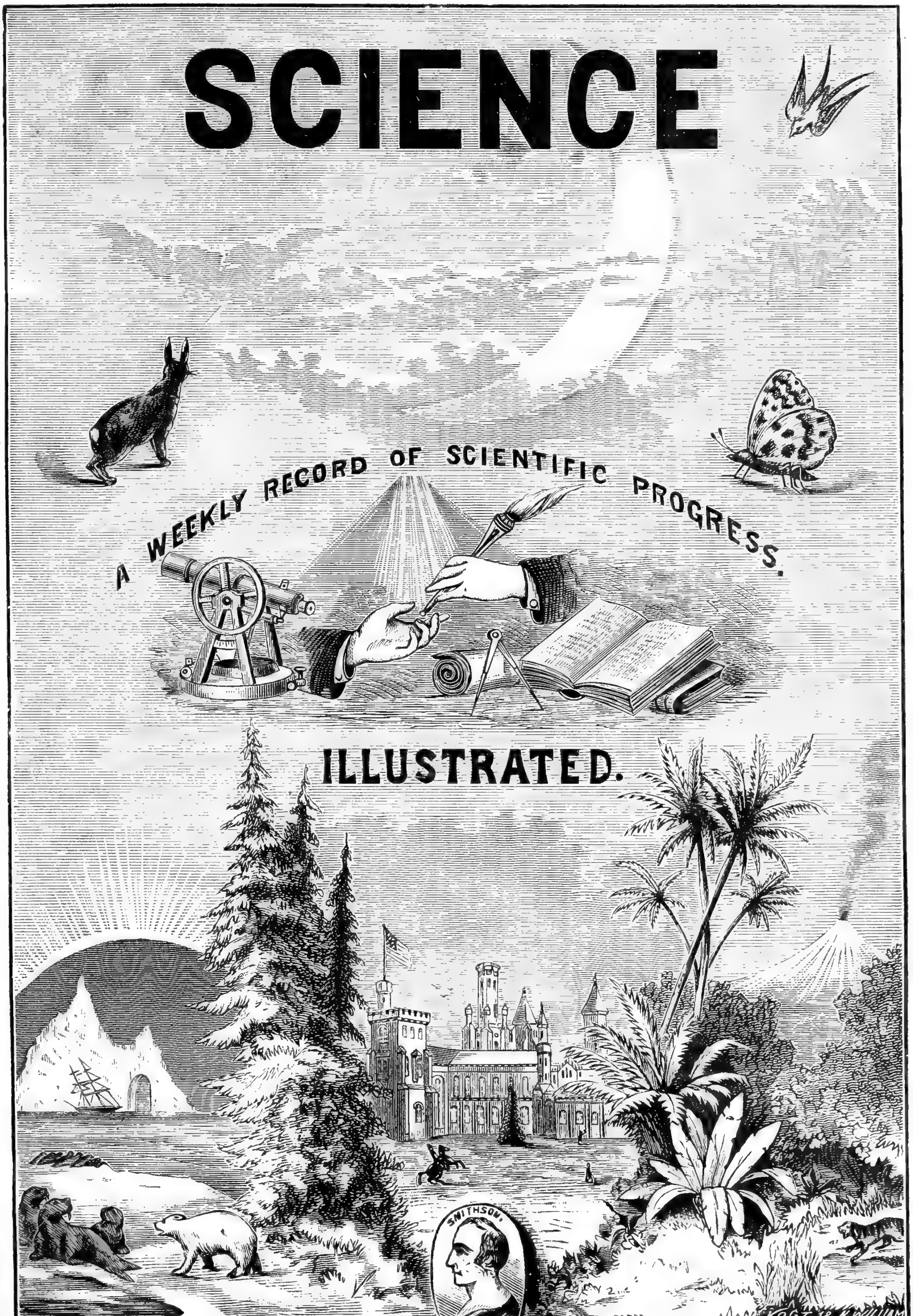
Vol. 2.] Whole Number 73. NOVEMBER 19th, 1881. [Price 10 Cents.



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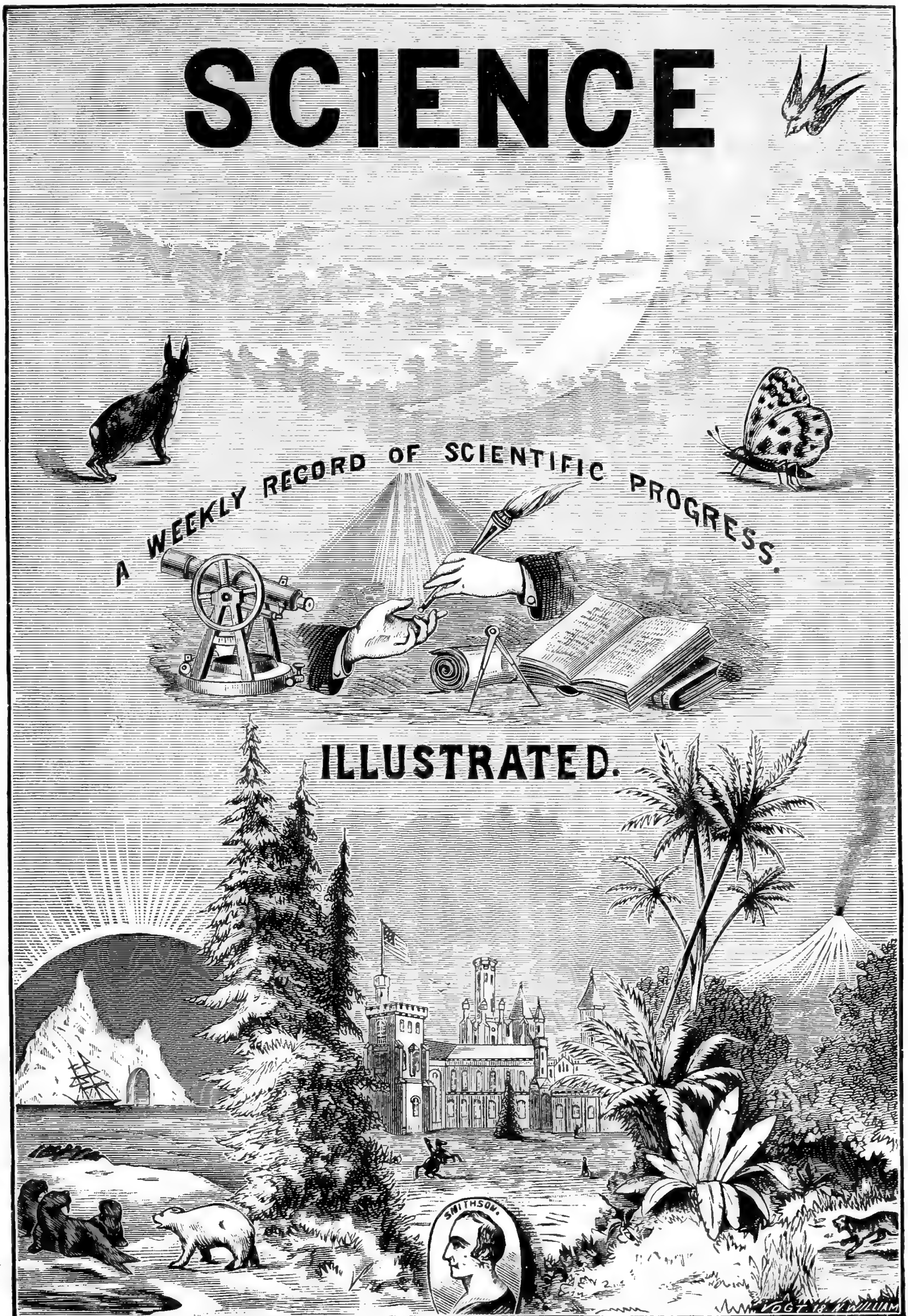

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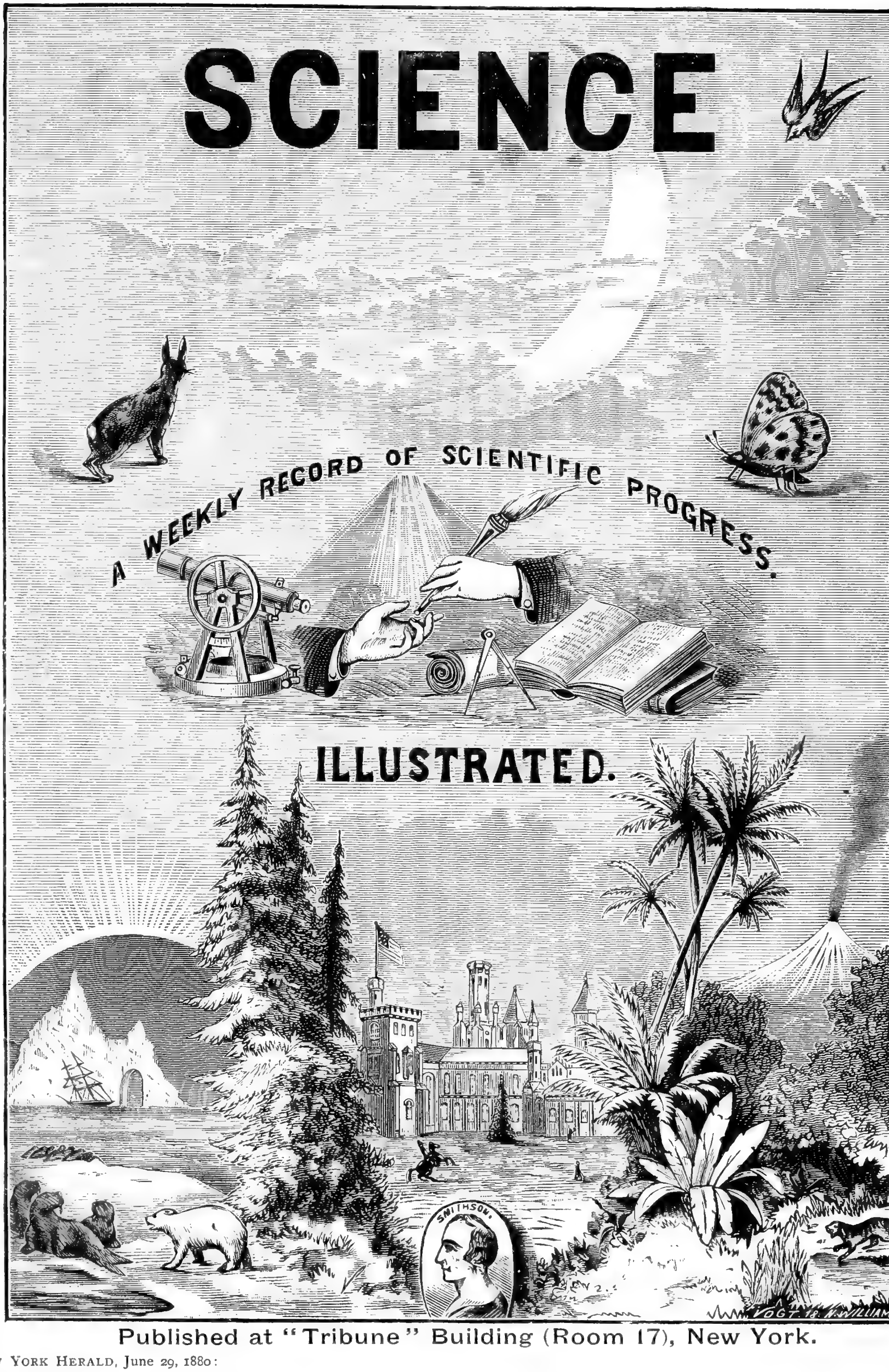

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NEW YORK HERALD, June 29, 1880:

"SCIENCE—A Weekly Record of Scientific Progress." It is a handsome, well-printed quarto sheet of 12 pages, with 4 additional pages for advertisements serving as a cover. It counts among its list of contributors Professors Baird and Coues, of the Smithsonian Institution; Holden, of the Naval Observatory, Washington; Marsh, of Yale; Wilder, of Cornell; Young, of Princeton; Abbe, of the Signal Office; Morse, of Salem; Drs. Hammond and Spitzka, of New York, and a considerable number of well-known scientific experts. IT CLAIMS, WITH TRUTH, TO OCCUPY A HITHERTO UNOCCUPIED FIELD IN AMERICAN PERIODICAL LITERATURE, AS THE ONLY FIRST-CLASS WEEKLY JOURNAL IN THE UNITED STATES DEVOTED TO SCIENCE.

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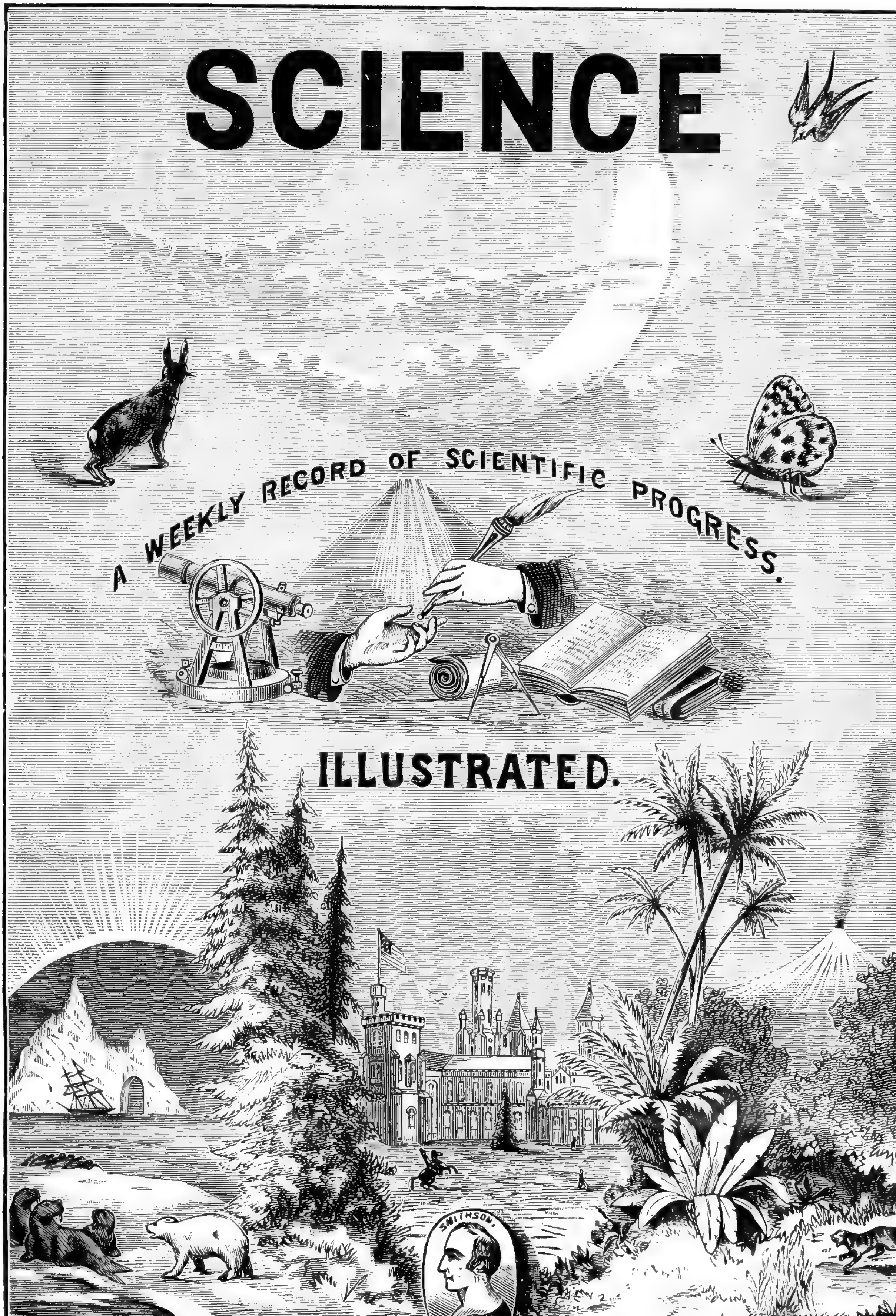

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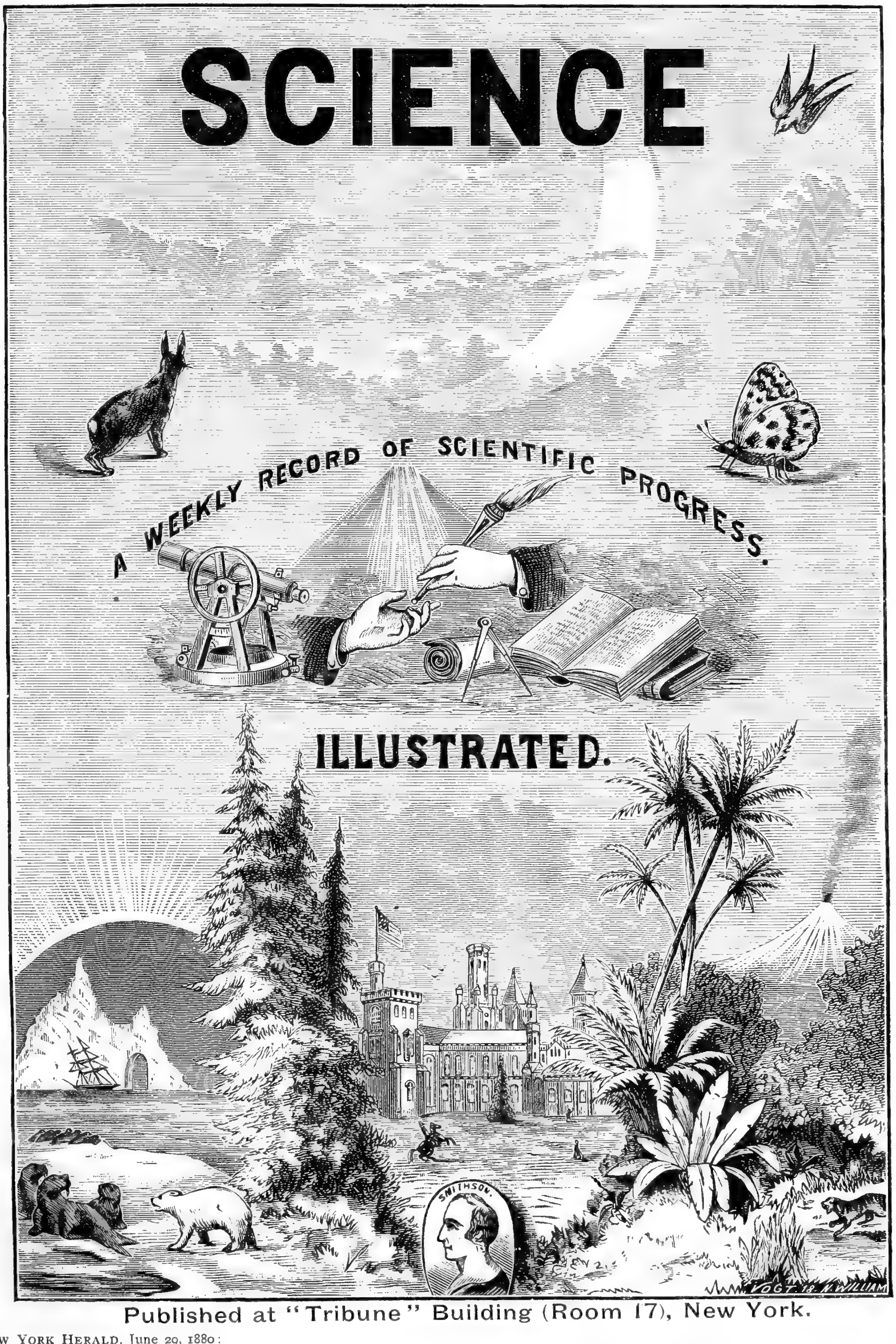

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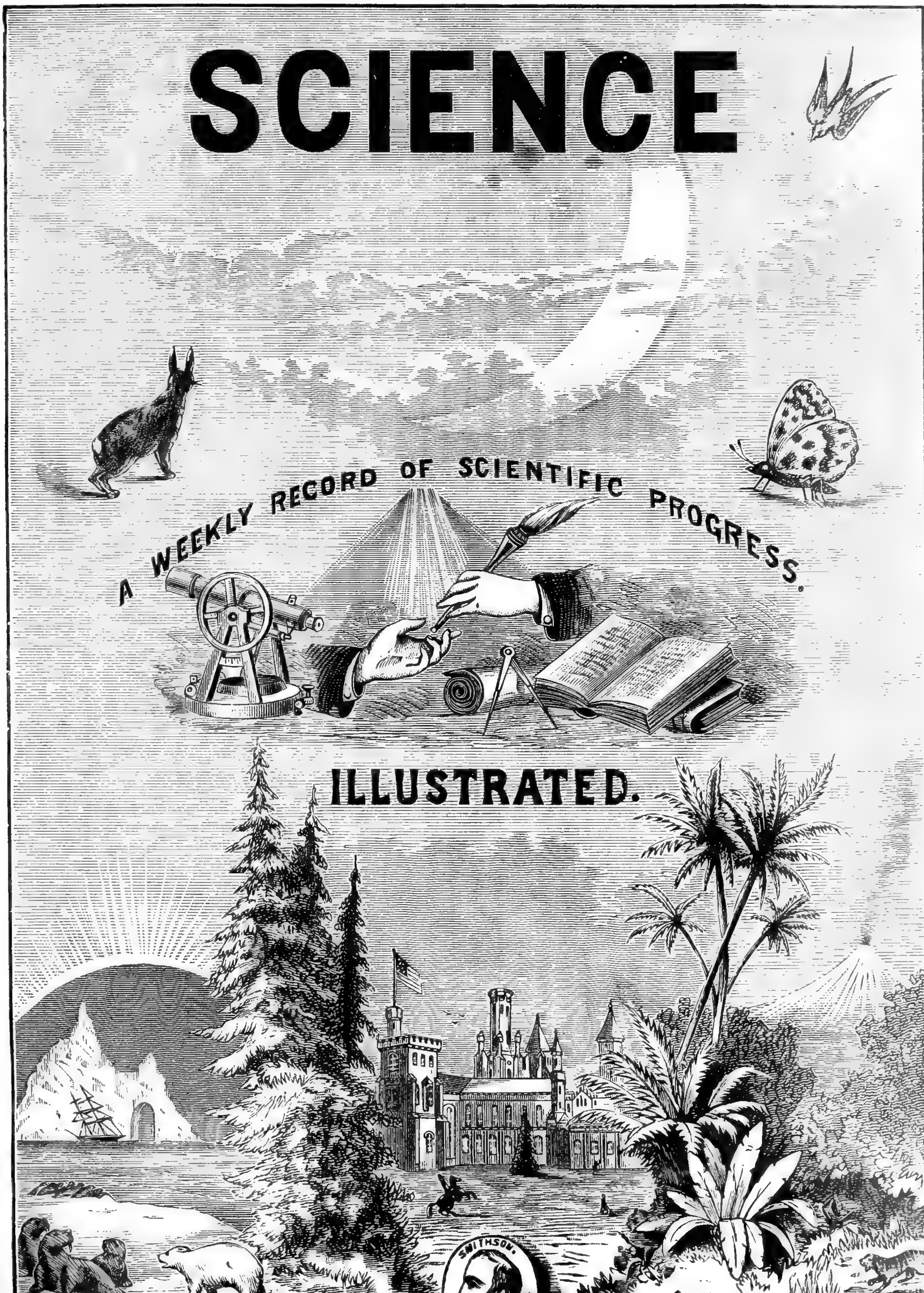

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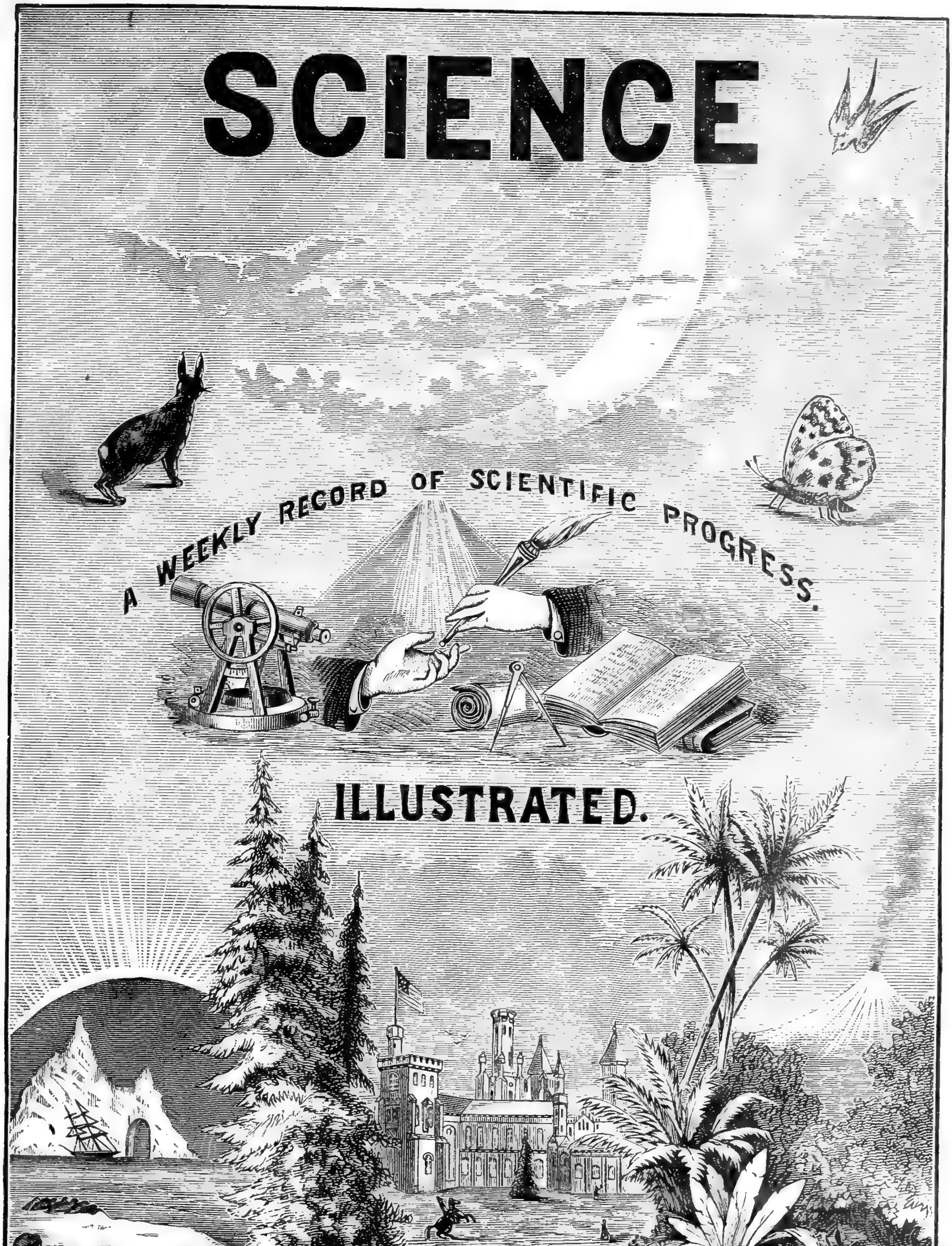

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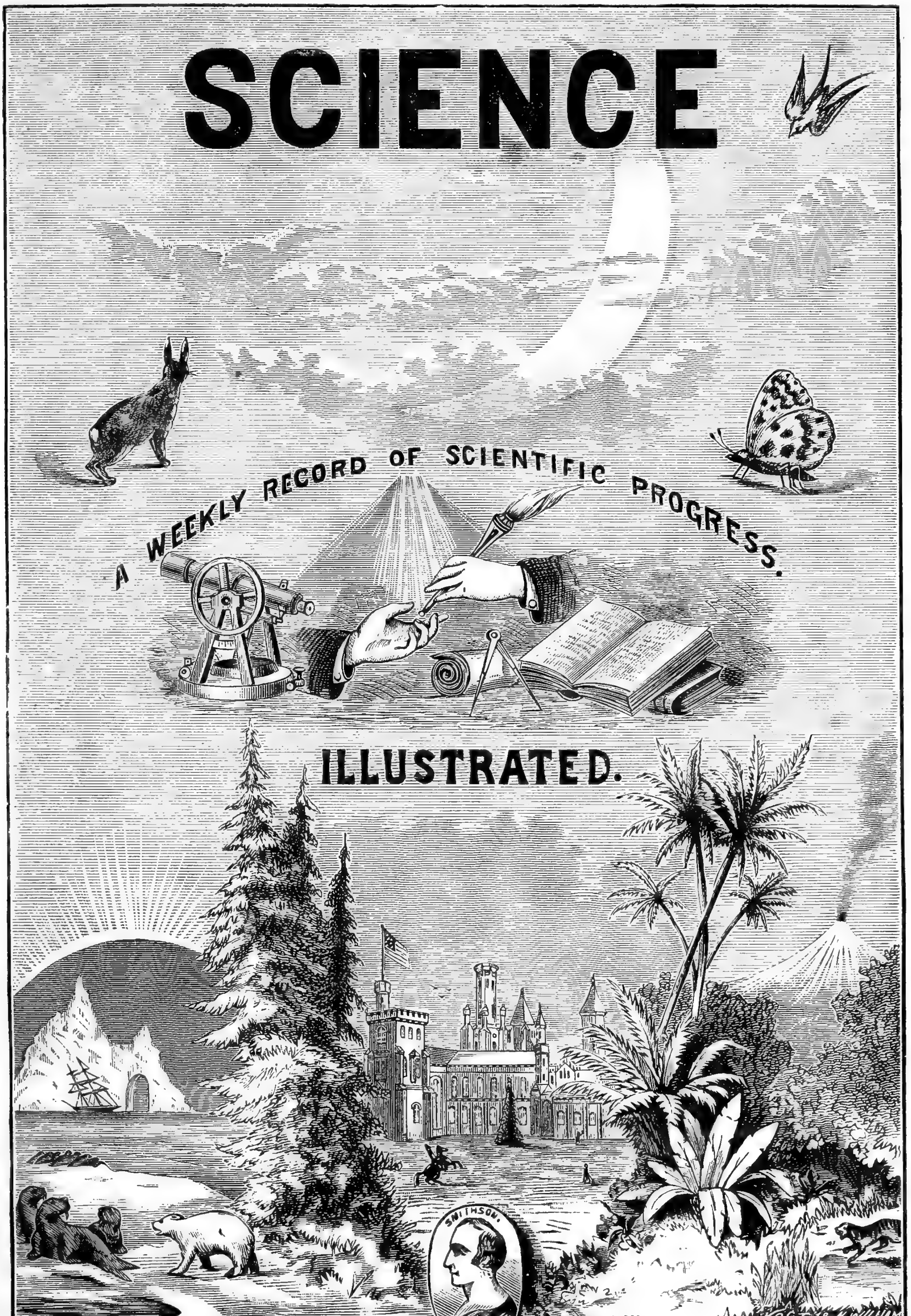

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